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Printed in the United States of America by George Banta Company, Inc., Menasha, Wisconsin.
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ARTICLES

441 **Confusion, Diffusion, and Innovation.** The apparent fact that interactive effects are more common in policy innovations taking a long time to diffuse among the states, contrary to the presumed effects of interaction, suggests the existence of alternate diffusion mechanisms. Some policies diffuse directly from a federal model, while others diffuse among states via a segmented pattern of emulations. The order of state adoption of fair employment practices legislation is compared with the adoption order for three labor policies and two civil rights policies. Fair employment practice, by this test, is identified as a civil rights policy and not as a labor policy. State minimum wage legislation is discussed as a case of federal influence in the diffusion process. A first wave of diffusion was followed by a period of federally inspired court rescission. Federal legislation in 1938 began another wave of diffusion. In a third wave of innovations, states with existing laws amended those laws by emulating the new federal legislation.

By ROBERT EYESTONE, Associate Professor of Political Science, University of Minnesota.

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448 **A Revised Theory of Winning in House-Senate Conferences.** The question, "Who wins in House-Senate conferences?" has largely been answered for recent Congresses. But the question, "Why does the Senate win?" has not been adequately answered. The research reported here presents and tests some necessary conditions of a theory that provides an answer to this question. The Senate wins because it is most frequently the second acting chamber and, because it has constitutionally derived power over House decisions, giving it the capacity to get the adjustments it makes in House bills accepted in conference. In the minority of cases in which the Senate acts first, the House "wins" in conference. Unlike earlier attempts to explain conference outcomes, the theory proposed here is consistent with the overall pattern of House dominance in the legislative process.

By GERALD S. STROM, Assistant Professor of Political Science, University of Illinois at Chicago Circle, and BARRY S. RUNDQUIST, Assistant Professor of Political Science, University of Illinois at Urbana-Champaign.

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454 **Behavioral Correlates of Political Support.** Theories of the behavioral consequences of political support coincide in the prediction that political support will correlate positively with indices of conventional behavior, negatively with indices of unconventional behavior. Survey data drawn from three communities in the Federal Republic of Germany show that an index of support for the structure of political authority is negatively correlated both with an index of actual participation in aggressive political behavior *and* with an index of participation in conventional electoral/pressure-group politics. Since the political behavior indices are themselves positively correlated, it is useful to construct a typology which differentiates between "pure" types – no participation, participation *only* in conventional, participation *only* in aggressive – and "mixed" types – participation in conventional *and* moderately aggressive, participation in conventional *and* highly aggressive. When the relationship between political support and the political-action type index is examined, it turns out that two of the types are associated with medium political support, while four of them occur at low support. To achieve more accurate explanation of types of political behavior, a model for prediction of each action type is proposed, taking into account interaction between political support, sense of personal political influence, and belief in the efficacy of past collective political aggression. The test of the model yields positive results, suggesting that it represents a fruitful beginning toward development of a theory of behavioral consequences of political support.

By EDWARD N. MULLER, Associate Professor of Political Science, State University of New York at Stony Brook.

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468 **The Cabal in Kabul: Great-Power Interaction in Afghanistan.** The July 17, 1973 coup serves as a case study of the nature and extent of great-power interest and involvement in Afghanistan. The dynamics of American, Soviet, and Chinese interaction are multifaceted and volatile, and imply that no one great power had outright control. Thus, this treatment concerns influence rather than control, and multilateral interaction rather than unilateral or bilateral action. The differing interests of the great powers in Afghanistan are outlined. Next, the possibility of great-power involvement in the coup is examined. Finally, the impact of the coup upon Afghanistan's relations with the three great powers is considered. Available material suggests that neither the United States nor the People's Republic of China had sufficient interest or influence to instigate the coup. Nor is there any concrete evidence that the Soviet Union played a significant role, although it did have the opportunity, influence, and interests to do so.

By SHAHEEN F. DIL, Assistant Professor of Political Science, Mount Holyoke College.

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477 **Partisan Dealignment in the Postwar South.** This study attempts to explain post-World War II southern electoral politics by examining the party identifications of southerners between 1952 and 1972. Pronounced decreases in Democratic loyalties and increases in Independent leanings appear during this

period and constitute a dealignment of the southern electorate. While interregional population exchanges have diluted Democratic strength, their effects are almost counterbalanced by the mobilization of blacks into politics. Instead, the principal source of dealignment is the generational replacement of the native white electorate. Its youngest members, who entered the electorate after World War II, have come to favor political independence increasingly in recent years. This behavior seems partially attributable to a tendency for young native whites in particular to bring their partisan loyalties into line with their attitudes and party images on racial issues. Even so, there are clear signs that the racial question is losing its place as the major determinant of the region's politics. For the future, one can expect a continuation of dealignment politics and little chance of a partisan realignment.

By PAUL ALLEN BECK, Associate Professor of Political Science, University of Pittsburgh.

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497 **Contextual Models of Electoral Behavior: The Southern Wallace Vote.** Many studies have sought to investigate contextual influences on individual electoral behavior using aggregate data. The shortcomings of this approach are discussed, focusing on the relationship between black concentration and southern white support for George Wallace for president in 1968. Through combining aggregate and individual-level data and comparing a series of models, black concentration is found to increase white support for Wallace. Intraregional differences in the relationship between white support for Wallace and local black concentration are equalized when contextual influences at the state level are brought into the analysis. Black concentration contextual effects are independent of those of urbanization, education, or residence in Wallace's home state of Alabama. Relative primary group support for Wallace and relative issue proximity to Wallace are then shown to be the intervening variables linking contextual characteristics and electoral choice.

By GERALD C. WRIGHT, JR., Associate Professor, Florida Atlantic University.

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509 **The Stability and Reliability of Political Efficacy: Using Path Analysis to Test Alternative Models.** The reliability and stability of survey items designed to measure political attitudes are important to the study of political behavior. Several past studies have examined the reliability and stability of items measuring one construct, that of political efficacy. The results of this prior research have been contradictory, in part because of the limitations of the methodologies used. In this article, the authors employ path analysis to examine more closely the stability and reliability of the four SRC items commonly used to measure political efficacy. The American Panel Study (1956–1960), in which efficacy is measured at two points in time, is used as the data base. The authors conclude that two of the four items (NO CARE and NO SAY) seem to measure best what is meant by political efficacy. These two items are more stable and reliable than previously thought, while the other items are relatively unstable and unreliable, and they display systematic differences from each other and from the NO CARE and NO SAY items.

By J. MILLER MCPHERSON, Associate Professor of Sociology, University of Nebraska; SUSAN WELCH, Associate Professor of Political Science, University of Nebraska; and CAL CLARK, Assistant Professor of Political Science, New Mexico State University.

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522 **The Philippines Before Martial Law: A Study in Politics and Administration.** In the period following World War II before the declaration of martial law in the Philippines, politics and heavy demands for patronage affected the allocation of resources, the administrative process, and personnel policies in ways inimical to the interests of businessmen and technocrats. When faced with intense competition, politicians pressed to employ growing numbers of clients and protégés in local administration. Strong political machines were better able than weak machines to ignore pressures for social services benefitting largely the lower class. The level of political mobilization and size of the lower class increased expenditures on primary education, and expenditures on intermediate and secondary education grew as the percentage of the electorate that was lower class increased. To the dismay of business interests and technocrats, "wasteful" expenditures on local administration and social services were most institutionalized and difficult to cut, while expenditures on economic improvements proved more elastic.

By THOMAS C. NOWAK, Assistant Professor of Sociology and Economics, Indiana University of Pennsylvania.

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540 **Divisive Primaries Do Hurt: U.S. Senate Races, 1956–1972.** This analysis shows, in contrast to previously published work, that a divisive primary hurts a senatorial candidate's chances for success in the general election. The relationship between divisiveness and lack of success continues to hold when incumbency and state party orientations are controlled. This relationship has an important implication regarding representation. The fact that divisive primaries do hurt strengthens the representation of weak and minor state parties in the U.S. Senate.

By ROBERT A. BERNSTEIN, Associate Professor of Political Science, Texas A & M University

546 **Authority and Community: The Contributions of Carl Friedrich, Yves R. Simon, and Michael Polanyi.** The problem of authority has a practical and a theoretical side. Practically, the decline of authority contributes to the crisis of legitimacy in contemporary governments and to difficulties in the practice of morality. Theoretically, authority is often confused with power, force, or coercion. Thus, it is viewed with suspicion. The thesis of this paper is that authority is not a form of power; rather, it transforms power. Like power, force, and coercion, authority is directive, but it is so in quite a different way. An examination of the concept of authority in three very different theorists – Carl Friedrich, Yves R. Simon, and Michael Polanyi – reveals that a sound concept of authority must be rooted in community as a system of shared beliefs, experiences, and traditions and in transcendent standards referred to by such beliefs, experiences, and traditions. Authority is that which directs a community to its proper end.

By CLARKE E. COCHRAN, Associate Professor of Political Science, Texas Tech University.

559 **The Relative Importance of Socioeconomic and Political Variables for Public Policy.** Since Dawson and Robinson, a dominant issue in the quantitative study of public policy has been the relative importance of socioeconomic and political variables for determining policy outcomes. It is argued here that past efforts to resolve this issue have been unsatisfactory, largely because they relied on inadequate statistical techniques, i.e., simple correlation, partial correlation, or multiple regression. Coefficients from these techniques are irrelevant for all but the most peculiar models of public policy. In general, if the researcher wishes to assess the relative importance of independent variables, it will be necessary to resort to path analysis of a formally constructed causal model. The comparison of "effects coefficients," derived from path analysis, is offered as the preferred means of evaluating independent variables, superior to comparisons of coefficients from simple correlation, partial correlation, or multiple regression. When the effects coefficients are actually calculated for a popular model of welfare policy, socioeconomic variables appear much more important than political variables, contrary to interpretations coming from the more traditional statistical techniques.

By MICHAEL S. LEWIS-BECK, Assistant Professor of Political Science, University of Iowa.

567 **Political Literature and the Response of the Reader: Experimental Studies of Interpretation, Imagery, and Criticism.** The influence of political literature has often proved elusive to empirical political science, partly because of the subjectivity of literary response, and partly because of social science methods which are largely incapable of dealing with subjective phenomena in a satisfactory way. A distinction is made between the experimental methods of expression which focus on objective responses, and the methods of impression which focus on subjective responses. Experimental methods are then applied to interpretations of Golding's *Lord of the Flies,* to the effects on imagery of reading Mazlish's *In Search of Nixon,* and to reactions to Burdick's *The Ninth Wave.* An illustration is also given of the experimental study of literary response in the single case.

By STEVEN R. BROWN, Professor of Political Science, Kent State University.

585 **COMMUNICATIONS**

From Jeff Stonecash, Vaughn Altemus, Theodore F. Macaluso, Jerrold G. Rusk, George Otte and Ruediger Zuelch, Robert C. Fried, Alexander L. George, Rubin G. Cohn, Byron W. Daynes.

603 **EDITORIAL COMMENT**

606 **BOOK REVIEWS AND ESSAYS**

The Georges' Wilson Reexamined: An Essay on Psychobiography. Psychobiography interprets a life-course in terms of a consciously thought-out interpretation of the subject's personality. There are criteria for judging the relative merit of differing lines of interpretation. The Georges' *Woodrow Wilson and Colonel House,* rightly recognized as a psychobiographical classic, proceeds from Harold Lasswell's formula for "political man" as one who seeks power to overcome a low self-estimate. The formula is in some respects questionable. The Georges' generally effective application of it falls short of full success. An alternative interpretation, based on the view that Wilson's life exemplified the neurotic search for glory as described by Karen Horney, is explored. The two different lines of interpretation are compared with respect to what Alexander George has called the "self-defeating pattern" in Wilson's career as a leader.

By ROBERT C. TUCKER, Professor of Politics, Princeton University.

619 **Khrushchev Remembers: A Review Essay.** Strobe Talbott, editor and translator, *Khrushchev Remembers;* Strobe Talbott, editor and translator, *Khrushchev Remembers: The Last Testament.*

By SIDNEY I. PLOSS, Washington, D.C.

624 **Comment.** By GEORGE BRESLAUER, Assistant Professor of Political Science, University of California, Berkeley.

627 **Rejoinder.** By SIDNEY I. PLOSS.

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Confusion, Diffusion, and Innovation

ROBERT EYESTONE
University of Minnesota

In a recent article, Virginia Gray discusses the diffusion of policies in education, welfare, and civil rights among the American states.[1] Her work builds on an earlier article by Jack Walker.[2] Other recent studies, while not attempting to take sides in this interchange, have also added to our understanding of policy innovations.[3] A number of points in these articles merit comment, but in this research note I have restricted my attention to four. First, I suggest that several models of policy diffusion seem to be operating, and that there is more than one possible pattern of emulation of policy innovations. Second, I comment briefly on the problems of inferring anything useful about state politics from the study of policy innovations among the states. Third, I present a technique for identifying clusters of similar policies on the bases of their diffusion patterns, and illustrate its use with fair employment practices, civil rights, and labor legislation. Finally, I examine the history of state minimum wage legislation, showing the importance of repeals, amendments, and reinstatements in addition to initial adoptions.

What is Diffusion?

Any pattern of successive adoptions of a policy innovation can be called diffusion. The interesting question, then, is how adoption decisions are made in the states. Which of several possible diffusion mechanisms is operating? Professor Gray fits the diffusion process with an interactive equation assuming complete intermingling and interchangeability of the states, and she finds significant interactive effects in six of 12 policy areas. But for those policies diffusing without interactive effects, the equation reduces to one of simple linear growth, equivalent to postulating independent adoption decisions by the several states.[4] Professor Walker, in contrast, had earlier suggested an emulative pattern with a regional component, in which states follow regional leaders.[5]

It might be tempting to see in these alternate models a choice between a "state forces" and a "national forces" explanation, or even between a "political" and a "bureaucratic" explanation. But I am afraid this is too simple. Unless we are willing to assume very powerful state bureaucracies, it must be expected that any innovation requiring legislative action would become law only if the legislature saw a need for it, over and above the suggestions of policy professionals in the state executive branch.

Or let us suppose that a problem rankles in the legislature, becoming a political liability for legislators and an issue on which they feel a clear need to act. In this instance they may emulate the policy of another state not because of expert pressure, but because the other state provides a *timely model* which may be seen as the solution to a vexing local political problem.

In the situations just described, the impetus to action is both "in-state" and "extra-state," and it is not really possible to separate the two. The existence of emulation effects certainly does not prove that any substantial extra-state forces (whether national or regional) are involved. Nor does the apparent absence of emulation effects in a given policy area deny the possible existence of individual emulators of the sort described above. Dealing as they do with policies and not with states as units, the Gray and Walker models are limited in what they can say about the motives for individual state adoptions.

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[1] Virginia Gray, "Innovation in the States: A Diffusion Study," *American Political Science Review,* 67 (December 1973), 1174–1185.

[2] Jack L. Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review,* 63 (September 1969), 880-899.

[3] In addition to the Gray and Walker articles, I have found the following useful: Douglas D. Rose, "National and Local Forces in State Politics: The Implications of Multi-Level Policy Analysis," *American Political Science Review,* 67 (December 1973), 1162–73; Donald C. Menzel, "Scientific and Technological Dimensions of Innovation in the American States," presented at the 1975 annual meeting of the Midwest Political Science Association; David R. Cameron, Stephanie H. Cameron, and Richard I. Hofferbert, "Non-Incrementalism in Public Policy: The Dynamics of Change," presented at the 1975 annual meeting of the Midwest Political Science Association. I wish also to thank Virginia Gray for the use of her original data, Jack Walker for some helpful suggestions, and two anonymous referees whose comments on an earlier draft have, I hope, improved this paper. I hasten to add that the "confusion" in the title is not a criticism of any of these writers, but is my impression of the collective impact of diffusion research on the unwary reader.

[4] Gray, "Innovation in the States," p. 1179.

[5] Walker, "Diffusion of Innovations," pp. 892-896.

Interaction effects can be, at best, only a partial explanation for policy adoptions by the states. A state's propensity to adopt a policy probably depends on three factors: some intrinsic properties of the policy, a state's politics, and emulative (interaction) effects. Of these, only the policy itself can be assumed to be invariant over time.

At the moment we do not know much about these relationships, but there are several ways we might improve our knowledge of them. If we assume that states exchange *favorable* assessments of new policies when they interact, then we can give a more precise form to the interaction concept. The initial consequence of interaction will be to hold back adoptions, but once the halfway point has been reached, interaction will stimulate adoption of the policy.[6] Thus a policy innovation which diffuses through interaction should complete its diffusion sooner than a policy diffusing without interaction effects. In the Gray article, however, the policies showing significant interactive diffusion are also the *slowest* to diffuse, as shown in Table 1. What kind of diffusion process could be operating here?

I believe there are two plausible interpretations of the data. First, many of the policies diffusing rapidly are adopted by point source diffusion, where the states respond directly to a federal government example and not to each other.[7] Such a pattern appears in the social security area especially, following the passage of the federal Social Security Act in 1935.[8] Later in this paper I will describe a case of intermediate federal involvement, where federal legislation spurred a second round of state adoptions of minimum wage laws.

Second, those policies diffusing over a long period, and showing interactive effects, may be following a kind of segmented emulation process which spreads adoptions over a number of years. In the case of regional emulation, for instance, regional leaders must adopt a policy first, only then becoming models for the other states in the region. In contrast, a completely intermixed model would allow any state to emulate any other state already having adopted a law, whether in the same region or not. It is worth noting that such a segmented model need not be regional. Similar effects could be achieved if states simply emulated "other states like us" or "other states with similar problems." Regional identity is only a special case of this pattern.[9]

The Mysteries of State Politics

If we can temporarily neglect interaction effects, or if we can find several policies where interaction appears to have been unimportant, we may be able to sketch out the relationship between adoption propensity and state political patterns. This is Jack Walker's concern when he seeks to identify and describe the policy leaders among the states.[10] But focusing on particular policies has its problems. For a given policy, the order of state adoptions will reveal something about the fit between that policy and the politics of states only if we examine a time period short enough so that state political patterns do not change much. For if a state's politics changed substantially before it adopted

Table 1. Interactive Effects and the Length of Diffusion Periods

	Diffusion Period		
	1–29 Years	30–59 Years	60 Years and Over
Significant interactive effects	1 case	2 cases	3 cases
No interactive effects	4 cases	1 case	1 case

Source: Virginia Gray, "Innovation in the States: A Diffusion Study," *American Political Science Review,* LXVII (December 1973), Tables 1 and 3, p. 1175 and p. 1179.

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[6] Gray, "Innovation in the States," pp. 1175–1176.

[7] See the expanded version of Walker's article, "Innovation in State Politics" in *Politics in the American States,* 2nd ed., Herbert Jacob and Kenneth N. Vines, eds. (Boston: Little, Brown, 1971), pp. 377-378, for a discussion of the ACIR as a national clearing house for state innovations.

[8] Gray, "Innovation in the States," pp. 1180–1181.

[9] Walker's general explanation of state emulations allows for nonregional factors. See Walker, "Diffusion of Innovations," p. 889. But the expanded version of Walker's article presents evidence showing the persistence of regional patterns of emulation. See Jacob and Vines, *Politics in the American States,* pp. 378–85. Cameron, Cameron, and Hofferbert, *Non-Incrementalism in Public Policy,* pp. 51–53, also argue for the existence of regional effects.

[10] Walker, "Diffusion of Innovations," p. 882.

a policy which had been "available" for some while, the difficulty of adoption would probably have altered, and the timing of adoptions by the several states would not be a consistent guide to the relative difficulty of adopting the policy under study.

In common usage, we often imply that policies diffusing in similar time periods are themselves similar, whether or not their patterns of adoption among the states coincide. Thus we speak of the Progressive Era policies that were adopted by many states between 1910 and 1930, or the policies of the post–World War II period, from 1945 to 1960. While there may be similarities among these policies, I am taking the more cautious position that similarity should not be assumed unless the diffusion patterns of these policy groups among the states are also similar. By a parallel argument, I would not assume that a Progressive Era policy and a postwar policy were similar, even if they diffused among the states in similar sequence, because the historical periods are (unless demonstrated otherwise) too disparate.

Admittedly, any time limit will be somewhat arbitrary. But looking at the rate of major change in national politics and the rates of change in state population characteristics, I would suggest that 25 to 30 years may constitute a kind of "political generation." Within such a time period, political changes, wherever they occur, may be assumed to be relatively minor. Beyond the 30-year point, however, substantial changes may have occurred in some or all states. Thus I would argue that adoptions coming more than 30 years after the initial adoption do not provide usable evidence about the intrinsic qualities of the policy in question. Unfortunately, this limitation proves to be serious, because the diffusion period for many policy innovations is longer than 30 years. Only five of the 12 policies Professor Gray examined meet this test (see Table 1), and only one of the three policies I discuss in this paper diffused in less than 30 years.

Two strategies remain feasible. We can restrict our attention to the first 30 years of each policy, asking which states are most often in the vanguard on a number of policy innovations. Thus Jack Walker ranks the states on innovativeness across a set of 88 policies.[11] The reliability of this procedure can be increased if we are content simply to describe *the* innovators for a variety of policies, asking which states were most often the initial adopters for a group of policy innovations. But this approach may not reveal much about state politics (and hence not much about the policies under study), especially if the same few states are always first.

A second feasible strategy concentrates on policies which diffused rapidly in response to federal government initiatives. Granted that an externally stimulated innovation is different from an innovation begun among the states themselves,[12] still the emulation of a federal program is an innovative step which may often encounter strong political opposition within the states. States which adopt such policies first can legitimately claim some status as innovators.

Which Policy is Which?

Even though we may be unable to reason directly from the order of adoption to policy similarity, because diffusion periods are too long, we can still learn something about diffusion (and about policies) by comparing states' treatment of programs commonly regarded as similar. Virginia Gray examines several programs in each of three broad areas; in particular, she presents data on fair employment practices, nondiscrimination in public accommodation, and fair housing laws under the general heading of civil rights policy. The rank orders of adoption of these three programs correlate highly, but they do not correlate with the adoption ranks for the other two policy areas, nor do the rank orders of program adoption correlate *within* the other two areas (education and welfare).[13]

To interpret these correlations we must assume that specific laws "belong" in the groups to which they have been assigned. The fair employment practice legislation is particularly interesting because, prima facie, this policy could be seen as both a civil rights measure and a pro-labor action. Viewed as a labor policy, fair employment practice has affinities with "right to work" laws and possibly with job training programs, but probably not with laws encouraging labor unions or any other form of labor market restriction. Viewed as a civil rights policy, fair employment practice has clear logical similarities to open housing and other sorts of social desegregation laws, but not necessarily to voting rights and other political rights guarantees.

I would like to argue that, under the proper conditions, the correlation of the rank orders of state adoptions of any two policies can be taken as a rough measure of the similarity of the *content* of the two policies. The assumption underlying this approach is that the rank ordering of state adoptions of a policy reflects the results of a series of experiments in which the proponents of the policy attempt to gain support for it in each state political system, each state having its own combination of formal structures and informal political processes. The more similar two policies are, the more similar should be a given state's response to them.

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[11] Walker, "Diffusion of Innovations," p. 882.

[12] Gray, "Innovation in the States," pp. 1180–1181.

[13] *Ibid.*, p. 1185.

Diffusion of the two policies must begin at roughly the same time, so that interaction effects and changing state political patterns do not confound the comparison. A simple measure of the potential overlap of diffusion periods can be generated as follows: note the later of the initial adoption years for the two policies being compared; for the *previous* year count all those states which have not yet adopted either policy. Divide this number by N (the total number of states) -1 to provide a measure ranging from 0 to 1. A value of .50 might provide an arbitrary, rather lenient standard below which comparability should not be assumed.

Fortunately, the requirement of a short total diffusion period may be relaxed when comparing policies, since any changes in state politics will affect adoptions in similar fashion if the policies are themselves similar. So the logic of the comparison is this: if the two policies being compared are in fact similar and simultaneously available for diffusion, they should diffuse among the states in roughly similar order whether interaction effects are present or not. If the policies are dissimilar but available for diffusion at the same time, they should show different diffusion patterns because of state politics and possibly also because of interaction effects. If the policies are not simultaneously available for diffusion, the test cannot be interpreted, even if the rank orders of adoptions are similar.

Reducing the complexities of policies and state politics to a single metric may seem to be moving in reverse, but the approach can help to identify empirical clusters of policies and to test intuitive feelings about types of policies. Why should one want to do this? After all, the clusters so produced may very well not coincide neatly with traditional nominal policy categories. The reason, put simply, is that the *evidence* of diffusion can be used to generate clusters of policies similar in their diffusion patterns among the states, and then by examining these clusters, we can improve our understanding of the *process* of diffusion. The common elements among the policies in these clusters will presumably also be important in explaining the observed diffusion pattern. In this connection, the time constraint is introduced not so much as a control in the statistical sense (though it is this), but as a condition necessary for proper interpretation of the diffusion similarity test.

Table 2 presents data testing the speculative identification of fair employment practices legislation as a labor policy. The figures reported are Spearman's rank correlation rho, corrected for ties. Below each correlation is the measure of potential overlap of diffusion periods just described.

It is clear from the table, first, that the diffusion period for fair employment practices legislation overlaps almost completely with the diffusion periods for civil rights laws. But the overlap between fair employment practices and the supposed labor policies in the table is slight: of the three labor policies represented, only anti-injunction legislation shares even half its potential diffusion period with fair employment practices. Moreover, these two policies are probably dissimilar in their effects. Anti-injunction laws ("little Wagner Acts") are pro-union, while fair employment practices are if anything anti-union, though unions support such laws.

If fair employment practice is tentatively identified as a civil rights policy, then the three civil rights policies can be seen to share one diffusion period and the three labor policies apparently to share a different period. State labor laws are the product of the Progressive and New Deal eras, while civil rights laws have emerged only since World War II. And the civil rights policies, including fair employment practice, make a more convincing cluster (average correlation .72) than the labor policies (average correlation .45).

The cross-correlations are generally unreliable because of dissimilar diffusion periods, but the average of those meeting the minimum comparability criterion is as high (.45) as the labor policy correlations.[14] In other words, the supposed labor policies could just as convincingly be called civil rights policies on the basis of this evidence. Indeed, if the diffusion period were similar, it would be tempting to place workmen's compensation in the civil

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[14] It might be thought that overlap of diffusion periods was in itself a cause of higher rank-order correlations. For these data the relationship is weak, however – for the 15 entries in Table 2, the product-moment correlation is only +.14.

Table 2. Correlations of Adoption Rank Orderings and the Overlap of Diffusion Periods (Spearman's rho corrected for ties; diffusion overlap index in parenthesis)

	Fair Employment	Public Accommodations	Fair Housing	Minimum Wage	Anti-Injunction	Workmen's Compensation
Fair employment practices	1.00 (1.00)	.77 (.94)	.73 (.96)	.44 (.43)	.43 (.53)	.60 (.02)
Desegregation of public accommodations		1.00 (1.00)	.65 (.94)	.37 (.43)	.57 (.53)	.60 (.02)
Fair housing			1.00 (1.00)	.35 (.55)	.44 (.60)	.60 (.04)
Minimum wage				1.00 (1.00)	.48 (1.00)	.49 (.77)
Anti-injunction labor laws					1.00 (1.00)	.39 (.70)
Workmen's compensation						1.00 (1.00)

Sources: Public accommodations and fair housing – data supplied by Virginia Gray. Fair employment, minimum wage, anti-injunction, workmen's compensation – U.S. Department of Labor, *Growth of Labor Law in the United States* (Washington: Government Printing Office, 1967).

rights group because its diffusion pattern is more like those of the civil rights policies than those of the other labor policies. Minimum wage and anti-injunction laws do not appear to have a home in either category.

Rough Roads to Innovation

I want to conclude with a general caveat about the study of policy innovations, and a specific explanation for the waywardness of minimum wage policies among the states. Figure 1 shows two curves, one the monotonic trend of the states[15] that have ever adopted a minimum wage law, the other a reversing trend line of states currently having such a law in effect.[16] Several processes account for the discrepancy between the two curves. Some states adopted a minimum wage law and subsequently repealed it or allowed it to lapse. Some states adopted a minimum wage law which was later struck down by a federal or state court or declared unconstitutional by the state Attorney General. Some states readopted a minimum wage law when judicial tempers changed, but others decided to do without such legislation. And in the middle of this sequence (1938) the federal Fair Labor Standards Act was passed, drawing off some of the political pressure for minimum wage legislation at the state level. Table 3 summarizes this complex pattern.

Should we now speak of the "extinction" of innovations, or the "rescission" of innovations by outside forces? It seems that some kinds of disinnovation may be as contagious as the initial pattern of adoption was. Figure 2 shows a clear tendency for innovations and disinnovations to cluster, in the years prior to the New Deal. It also shows the impact at the state level of the national political changes reflected in the New Deal, an impact which *preceded* the adoption of federal minimum wage legisla-

Table 3. Disinnovation in State Minimum Wage Legislation, 1912–1967

Law repealed		4 states
Law declared unconstitutional		10 states
By federal court	5 states	
By state court	4 states	
By state Attorney General's opinion	1 state	
Law allowed to lapse or to become technically inoperative		3 states
Total disinnovations		17 states

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[15] Including Alaska, Hawaii, the District of Columbia, and Puerto Rico.

[16] Data are drawn from U.S. Department of Labor, *Growth of Labor Law in the United States* (Washington: Government Printing Office, 1967), pp. 93–97.

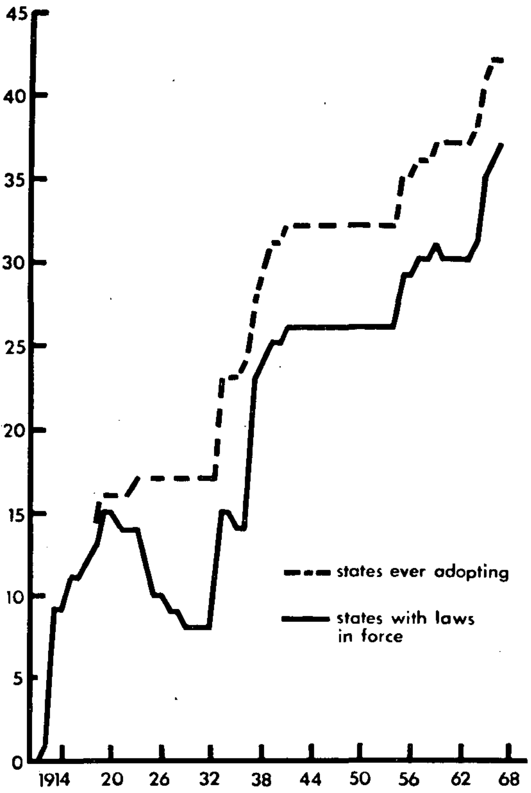

Figure 1.
State Minimum Wage Legislation

tion.[17] A total of 17 states gained effective minimum wage legislation between 1933 and 1937 – 10 by new adoptions, 3 by re-enactment of laws previously declared unconstitutional, and 4 by judicial opinions reinstating laws previously declared unconstitutional.

It might be said that the loosening of economic conservatism during the New Deal allowed the states to complete a first wave of diffusion of minimum wage laws. Another wave of adoptions began in 1955. Figure 2 also reveals a third wave of innovation, beginning in 1939 and directly patterned after the federal legislation passed in 1938. In this third wave, which would not appear at all in a simple tabulation of new minimum wage adoptions, states with minimum wage laws already on the books moved to upgrade them,[18] typically by extending coverage to men, by adding supplemental pay for overtime, or by replacing a wage board or daily minimum rate with an hourly statutory minimum wage. In each of these three areas, the federal Fair Labor Standards Act was the innovator and the states followed the federal lead. When states began again to adopt minimum wage legislation, after 1955, they emulated the new national model.[19]

Conclusions

The diffusion of policy innovations among the states is a complicated phenomenon for which several models provide partial explanations. Interactive effects can be found for a selected set of policy measures, but it is not possible to say in advance what policies are members of this set. For other policies each state is apparently on its own, though the ease or difficulty of adopting a particular policy may be altered during the policy's diffusion period by changes in state or national politics.

Some states are consistently among the leaders on a variety of policies, while others lag behind in nearly every area. Yet diffusion patterns may record the spread of necessity rather than the emulation of virtue: leaders

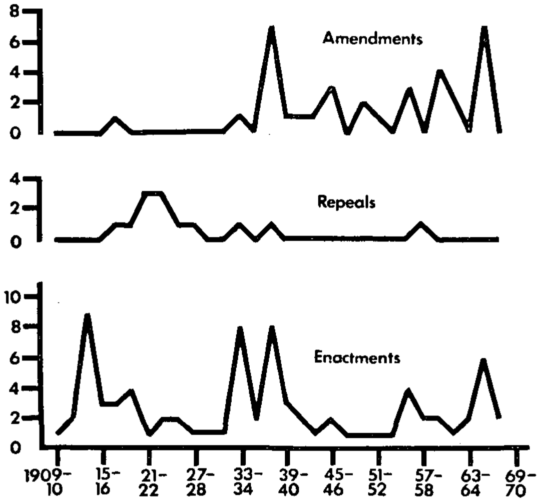

Figure 2. Components of Minimum Wage Diffusion

may lead because they are also the first to suffer the undesirable side effects of urban and industrial growth which create demands for state policy responses.

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[17] See Rose, "National and Local Forces in State Politics" for a general discussion of the openness of states to national political influences. Cameron, Cameron, and Hofferbert, "Non-Incrementalism in Public Policy," p. 10, also support Rose's nationalization hypothesis. Menzel, "Scientific and Technological Dimensions of Innovation," p. 23, presents examples of informal federal pressure on states.

[18] Menzel generally emphasizes the possibility of innovation by nonlegislative means. Cameron, Cameron, and Hofferbert, p. 55, also argue for the importance of innovation within areas of long-established state action.

[19] U.S. Department of Labor, *Growth of Labor Law*, pp. 93–97.

A third model, that of state response to federal initiatives, is also prominent in some policy areas. The federal model, however, contains a mechanism qualitatively different from those of the interactive and independent decision models – i.e., the provision of positive incentives for states choosing to adopt the federal policy. Nevertheless, states may still want to review the experiences of other states before taking the plunge themselves, depending on the strength of the incentives put forward by federal agencies, and perhaps on the level of residual state resistance to federal innovations.

With all these qualifications, is it still useful to study the diffusion of innovations among the states? I think it is, for several reasons. Innovations are important in themselves, and we should know where they come from. It would also be useful to delve into the mysteries of state politics to learn more about the mechanisms of emulation.

While it might be possible to find similarities in emulation processes across a wide range of policy areas, I would prefer to look at selected individual policy areas or clusters. We do not yet know enough about policy content, I think, to risk the confusions of lumping together large numbers of policies, especially if in doing so we would be mixing representatives of several distinct diffusion models. As I have tried to show in this research note, comparison of diffusion patterns may provide a way of generating policy clusters empirically, according to their political similarity. It is arguable whether this kind of similarity is the most appropriate for the study of innovations, but I think the proposition is worth testing.

A Revised Theory of Winning in House-Senate Conferences*

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The question, "Who wins in House-Senate conferences?" has been answered, at least for recent Congresses.[1] Almost all evidence shows that the Senate wins more often than does the House.[2] Why the Senate wins, however, has not been adequately answered. Speculation and hypotheses abound, especially since much of what we know about Congress leads to the conclusion that the House rather than the Senate should win.

In this paper we examine the available explanations of why the Senate conferees tend to win and suggest that an alternative explanation is not only supported empirically but is more consistent with the prevailing understanding of the roles of the House and Senate in Congressional policy making.

Conference Outcomes

Senate dominance of conference decision making has been demonstrated repeatedly. Fenno, in his study of 331 appropriations conference decisions, found that the Senate won 56.6 per cent of all outcomes and 65 per cent of the outcomes other than those where the House and Senate split the differences; the House won 30.5 per cent and 35 per cent respectively.[3] Similarly, Manley found that the Senate won 56 per cent of all tax legislation outcomes and 70 per cent of the outcomes in which there was a winner; the House won 25 per cent and 30 per cent respectively.[4] In a study of conference outcomes during five Congresses, Vogler found that the Senate won an average of 65 per cent of the time, while the House won 35 per cent.[5] Kanter found that the House accepted the Senate's recommendations on defense appropriations bills 57 per cent of the time.[6] Finally, in examining the 136 identifiable conference outcomes during the 92nd Congress, we found that the House won 30 per cent of the time while the Senate won 46 per cent, and, when only decisions which resulted in a victory for one side are considered, the Senate won 60 per cent to 40 per cent for the House (N=103).[7] Thus, where there is a winner, the Senate generally wins between 60 and 70 per cent of conference outcomes, while the House wins 30 to 40 per cent.

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[1] Richard F. Fenno, Jr., *The Power of the Purse: Appropriations Politics in Congress* (Boston: Little, Brown, 1966), pp. 616–678; John A. Ferejohn, *Pork Barrel Politics: Rivers and Harbors Legislation, 1947–1968* (Stanford: Stanford University Press, 1974), pp. 116–126; John F. Manley, *The Politics of Finance: The House Committee on Ways and Means* (Boston: Little, Brown, 1970), pp. 269–370; David J. Vogler, "Patterns of One-House Dominance in Congressional Conference Committees," *Midwest Journal of Political Science,* 14 (1970), 303–320; David J. Vogler, *The Third House: Conference Committees in the United States Congress* (Evanston, Ill.: Northwestern University Press, 1971).

[2] The evidence is consistent, even though different writers use different definitions of "winning." Fenno used differences in dollars at the bureau level and declared a victory for the chamber whose version was closest to final conference version; Manley used various measures including expected total revenue gain or loss; and Vogler used the codings of Congressional Quarterly. See also, Gilbert Y. Steiner, *The Congressional Conference Committee: Seventieth to Eightieth Congresses* (Urbana: University of Illinois Press, 1951) for an alternative set of results which show the House predominates in conference.

[3] Fenno, pp. 661–670.

[4] Manley, pp. 272–279.

[5] Vogler, "Patterns of One-House Dominance," p. 309.

[6] Arnold Kanter, "Congress and the Defense Budget: 1960–1970," *American Political Science Review,* 66 (March, 1972), 129-143.

[7] In the present study, 136 conferences in the 92nd Congress were identified and separately coded four times, twice at one-month intervals by the senior author, and once each by two graduate students who were purposely kept unaware of the main hypothesis of this study. If the conference bill was closer overall

Explaining Conference Outcomes

The scholars cited above offer several different explanations for their findings. For appropriations conferences, Fenno suggests that the greater support provided Senate conferees by the Senate itself allows them to win:

> The Senate is stronger in conference because the Senate [Appropriations] Committee and its conferees draw more directly and more completely upon the support of their parent chamber than do the House [Appropriations] Committee and its conferees. . . . The position they defend will have been worked out with a maximum of participation by Senate members and will enjoy a maximum of support in that body.[8]

This explanation takes account of a custom, dating to the 1840s, for conferees to be drawn from the standing committees in each chamber to which the bill was originally referred.[9] Fenno also examines and rejects the alternative explanation that the Senate wins because it supports the higher budget estimate.[10]

Manley explains Senate victories in the tax social-security tariff areas with another version of the outside support explanation:

> The reason the Senate does better in cases of conflict with the Ways and Means Committee is because politically Senate decisions are more in line with the demands of interest groups, lobbyists, and constituents than House decisions. Ways and Means decisions, made under the closed rule, tend to be less popular with relevant publics than Senate decisions.[11]

Finally, Vogler seems to suggest that the chamber represented by the most prestigious committee in conference will win. His analysis shows, however, that the opposite is true: "House conferees drawn from committees with lower prestige than that of the Senate committees involved in conference were generally more successful than were House conferees from committees with prestige rankings above those of their Senate counterparts."[12] Vogler offers no explanation of this finding, but Ferejohn shows that it is a special case of Fenno's chamber support explanation: "The less prestigious House Committees may be more permeable to influence from the House as a whole than the prestige committees, which are expected to control some natural excesses of the House. Thus conferees from low prestige House committees enjoy more floor support than do conferees of the prestige committees."[13]

That the emphasis of these explanations falls on the greater outside political support given Senate conferees as opposed to House conferees is not surprising. This is one of the few variables related to success in conference decision making that apparently favors the Senate. Most other sources of House-Senate differences lead one to expect that the House will win in conference. For example, the greater specialization of representatives in their committee work makes them better prepared for conferences. Similarly, the tighter organization of House committees is reflected in preconference strategy sessions, smaller and more hierarchically structured bands of conferees, better conference attendance records, and "tougher" bargaining stances by the House conferees.[14]

We are troubled, however, by two related aspects of this explanation of Senate victories. First, how exactly does senators' greater outside political support help them to win in conference? Both unit voting and the tendency for the House conferees to be major proponents of the House committee's bill appear to limit the utility of outside political support as a bargaining resource. Unit voting means that senators have to change the position of a majority or more of the House conference delegation, not just that of enough to form a majority with the Senate conferees. Moreover,

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to the House bill, that conference outcome was coded a House victory; if the conference bill was closer to the Senate bill, it was coded a Senate victory; and if the conference bill was equally close to the House and Senate bills, it was coded a draw. "Closeness," however, is a subjective judgment. This is problematic, but is somewhat less so if the subjective judgments of different coders agree. For the 92nd Congress, the reliability of the coding was very high. Intracoder reliability was 95 per cent; intercoder reliability averaged 83 per cent. On only three bills were there major disagreements (i.e., one coder coding a House victory, another a Senate victory or vice versa). These three were declared unclassifiable. All other disagreements involved one coder coding a draw while another coded a House or Senate victory.

[8] Fenno, *Power of the Purse*, pp. 668–669.

[9] Ada C. McCown, *The Congressional Conference Committee* (1927; rpt. New York: AMS Press, 1967), pp. 61–64.

[10] Fenno, pp. 667–670.

[11] Manley, *Politics of Finance*, p. 274. See also, Bertram M. Gross, *The Legislative Struggle: A Study in Social Combat* (New York: McGraw-Hill, 1953), pp. 317–327.

[12] Vogler, "Patterns of One-House Dominance," p. 319.

[13] Ferejohn, *Pork Barrel Politics*, p. 118fn.

[14] See especially, Fenno, *Power of the Purse*, pp. 649–652; and Lewis A. Froman, Jr., *The Congressional Process: Strategies, Rules, and Procedures* (Boston: Little Brown, 1967), pp. 5–15.

it is unclear why any House conferee would be willing to defer to the outside support of the Senate conferees rather than hold out for the bill his committee shaped and obtained House support for.

Second, even assuming that in some way greater outside political support is an important conference resource, its mobilization and use by Senate conferees seems to imply that there are important victories to be won in conference. But most observers seem agreed that conferences are *not* as *important* as other stages of the legislative process. As Fenno states, "conferees make the least consequential decisions of all."[15] If there is so little to be won, why would outside support be mustered to win it? The outside political support explanations seem to require the assumption that conferences are worth winning.

Our alternative explanation assumes that conference committees are less like wars or battles to be won and more like the peace talks that occur after major battles have been fought. It predicts that the chamber that will lose in conference is the chamber that has won the most prior to the conference. We would expect that conferees from this chamber would have the most incentive to induce the other conferees to leave their prior gains intact by conceding to their conference requests. Thus for the victor, conferences are a means toward ensuring what has been won elsewhere, not an arena for new victories. Therefore, strategic considerations, not the things that lead to winning battles (e.g., outside support), should affect winning in conference.

Which chamber is likely to have gained the most prior to conference? We will argue that the chamber that acts first on a bill tends to have the greatest impact on the content of a bill prior to the conference. This argument, a version of which has been advanced by Froman to explain why chamber disagreements are not taken to conference (i.e., the first acting chamber accepts the second acting chamber's revisions), and by Ferejohn to explain the results of appropriations conferences on Corps of Engineers civil works bills, is based upon the conception of the legislative process which follows:[16]

(1) Action on legislation usually begins in one chamber because two separate *de novo* considerations of the bill would be costly in time and resources and would make resolution of differences between the House and Senate versions more difficult. As a senior member of the House Public Works Committee told us: "Well, we have to delay awhile right now. The Senate has been ahead of us in working up a bill so we will wait until they have a basic version. Then we will work from that. [Is this because it will be easier to reach an agreement of conference?] Of course. You can't take two totally different bills to conference and hope to reach agreement."

(2) The first acting chamber negotiates a compromise on the bill. The compromise reflects the preferences of various chamber actors (e.g., chairmen, subcommittee chairmen, other legislators interested in the bill for whatever reasons) and those of the executive agency most involved. If the bill did not originate with the agency, the appropriate committee requests the agency's position on the bill.

(3) When the second acting chamber considers the bill, a preliminary compromise has been struck between the originating chamber and the agency, and the second acting chamber may be thought of as the minority member of a three-actor game. In this situation, members of the second acting chamber have two alternatives: they can either put together an entirely new bill, in effect forming a new majority coalition with the agency (or for that matter with the other chamber); or they can make marginal adjustments, and therefore the second acting chamber would prefer to make marginal adjustments.

(4) Actors in both the first acting chamber and the agency have an incentive to grant the second acting chamber's marginal adjustments in order to pass as much of its original compromise as possible. Of course, both actors as well as outsiders would also have an incentive to have the second acting chamber insert preferences they had sacrificed in the initial compromise as marginal adjustments.

(5) In conference committees the preferences inserted by the second acting chamber will be accepted by the first acting chamber.

(6) To the extent that marginal adjustments added by the second acting chamber deviate from the first acting chamber's original position, legislators from the first acting chamber have a short-run incentive to resist the adjustments. Moreover, if the first acting chamber consistently agreed to the marginal adjustments of the second acting chamber, in the future it might add more and more adjustments. Hence the first acting chamber has both a short-run and long-run incentive to oppose some of the second chamber's proposals. The conferees

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[15] Fenno, *Power of the Purse*, p. 678.

[16] Froman, pp. 155–158, and Ferejohn, pp. 118–119.

from the first acting chamber therefore have an incentive to grant some marginal adjustments so as to pass the legislation, but it is clearly in their interests as well to resist granting all the requests of the second acting chamber.

This view, which is consistent with the evidence that the Senate usually wins in conference, does not mean that the Senate's version of a bill usually predominates. Just the opposite is true: the House has more influence on the content of the bill because it acts first.

Evidence

If this explanation of conference outcomes is correct, it must follow either that the Senate is always the second acting chamber, or that the House also tends to win in conference when it acts second. Table 1 shows that the Senate is *not* always the second acting chamber, and that when the Senate does act first, the House wins in conference. In the 92nd Congress, when the House acted first, the Senate won 72 per cent of the conference outcomes; when the Senate acted first, the House won 71 per cent of the conference outcomes.[17]

Of course one reason the House acts first so often is that it is constitutionally required to initiate action on revenue bills and by custom initiates action on appropriations bills. When money bills are separated out, the House is still found to act first on 60 per cent of the bills that went to conference (see Table 2). More important, however, little difference exists in the proportion of conferences won by the Senate and the proportion of conferences on nonmoney bills won by the second acting chamber (see Table 3).

This strongly supports the inference that acting second is the general explanation of Senate predominance on both money and nonmoney bills.

These results may, however, be artifacts of the peculiarities of the 92nd Congress. To determine whether these findings could be generalized beyond the 92nd Congress, we hypothesized that the percentage of Senate victories in the five Congresses studied by Vogler[18] could be "predicted" by the following regression equation, derived from Table 1:

Number of Senate Conference Victories During a Congress = .72 (No. of House First Bills) + .29 (No. of Senate First Bills)

And where, using Table 2:

No. of House First Bills = .72 (Total of Bills Resulting in Conference)

No. of Senate First Bills = .28 (Total of Bills Resulting in Conference)

Applying these equations to the *N*'s presented by Vogler one can generate a set of predictions which can then be compared to Vogler's actual results. The more accurate the

Table 1. Conference Outcomes in the 92nd Congress in Relation to Which Chamber Acted First

	House Wins	Senate Wins	
House passes bill first	28%	72%	100% (*N*=75)
Senate passes bill first	71%	29%	100% (*N*=28)

Table 2. Conference Outcomes on Money and Nonmoney Bills in Relation to Which Chamber Acted First

	Money Bills	Nonmoney Bills	Total Bills
House passes bill first	41	57 (60%)	98 (72%)
Senate passes bill first	0	38 (40%)	38 (28%)
	41	95 (100%)	136 (100%)

Table 3. Conference Outcomes for Money and Nonmoney Bills Passed First by the House

	House Wins	Senate Wins	
Tax and appropriations bills	27%	73%	100% (*N*=37)
Other House first bills	32%	68%	100% (*N*=38)

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[17] Note further that of the 30 conferences which resulted in a draw, 21 per cent (*N*=95) were on bills that originated in the House and 26 per cent (*N*=38) were on Senate bills.

[18] Vogler, "Patterns," p. 309.

predictions, the more confidence we have that the assumptions and conclusions of the theory presented above are general.

Table 4 indicates the results of the application of these equations to Vogler's data.[19] The equations predict well, even with the strong assumptions that the percentages of bills passed first by the House and the percentage of Senate victories are constant. In fact, the results give strong support to both the theory and the corollary assumptions.

Table 4. Comparison of Actual and Predicted Conference Outcomes

Congress	Predicted Senate Victories	Actual Senate Victories (from Vogler)
79	25	23
80	28	33
83	35	42
88	29	34
89	43	42

$r = .86$

Implications

One implication of these results is that there is nothing about the Senate *per se*, other than its tendency to act second on legislation, that explains its tendency to win more often in conference than the House does.[20] The almost perfect symmetry of our basic findings indicates that neither the greater openness of Senate as opposed to House decision making, nor indeed any other characteristic that differentiates the House and Senate, exerts an independent influence on winning in conference. When the House goes second, it wins 71 per cent of the time. If outside political support or greater openness to this support affects conference outcomes, this symmetry would suggest that it does so equally for both chambers.

A second implication of our conception of the legislative process is that, in order to influence the *major* aspects of a bill, outsiders would concentrate on policy making in the first acting chamber and/or the relevant agency. This implication is further supported by Ferejohn's study of new starts added to the Army Corps of Engineers Appropriations bills (where the House always acts first). Ferejohn concludes that "... conference outcomes generally follow the pattern of House dominance," adding, "if the House adds a new start to the Corp's budget it almost always is funded in the final budget. A Senate-initiated new start usually has a much smaller chance of being in the conference budget.... House approval generally implies Senate approval, but the converse is not true."[21] Thus, although the Senate conferers prevailed in 65 per cent of the new starts over which the conferees disagreed, more House-initiated than Senate-initiated new starts were included in the ultimate statute.[22]

Conclusions

We have suggested that the reason Senate conferees win more often in conference is that the Senate more often acts second to the House on legislation. We contend that the conferees from the first acting chamber have an incentive to exchange marginal amendments in the bill with conferees from the second acting chamber to obtain the latters' support for the major aspects of the bill their chamber has passed. Thus conferences are best understood as the contexts in which conferees from the first acting chamber bargain for the support of the conferees from the second acting chamber, rather than as "the third House" in which the forces that lead to legislative victories are similar to those that lead to legislative victories at earlier stages of the legislative process.

In support of this explanation we demonstrated that the House wins about as many conferences when the Senate initiates legislative action as the Senate wins when the House initiates legislative action. This finding rather clearly indicates that the greater openness of Senate proceedings and the greater outside support for Senate bills does not cause the Senate victories. The Senate wins because it acts second and for the reasons we have suggested.

This is not to say that outside groups, bureaucrats, or congressmen do not wish to have their preferences prevail in conference. They clearly do. For example, agencies submit a reclama to the second acting chamber in hopes that the second acting chamber will pass a bill that includes their preferences and that will be upheld in conference. We suggest, however, that second chamber's conferees win not because they have more outside support, but because they have a strategic advantage in conference – namely, the ability to veto the compromise arranged by the first chamber.

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[19] Vogler, "Patterns," p. 309.

[20] This confirms Fenno's hypothesis that it is a characteristic of the Senate that explains conference outcomes; Fenno, *Power of the Purse*, p. 666. The invariance of the legislative sequence on appropriations bills, however, prohibited him from examining this particular characteristic of the Senate. Manley faced the same problem in his study of tax social-security tariff legislation.

[21] Ferejohn, *Pork Barrel Politics*, p. 123.

[22] *Ibid.*, p. 119.

Our theory also subsumes Froman's argument that the first acting chamber often accepts the second acting chamber's amendments without going to conference. The first acting chamber accepts the second acting chamber's amendments to protect its own version of the bill. The sending of bills to conference in the first place can be understood as an effort on the part of the first acting chamber to limit the number and extent of adjustments made by the second acting chamber. In this sense, conferences are a check on the logrolling between chambers that often characterizes House/Senate interaction.

Finally, this paper emphasizes the importance of the House in congressional policy making. We have shown that 72 per cent of the bills that went to conference in the 92nd Congress originated in the House. Moreover, of the 607 bills that became public laws during the 92nd Congress, 61 per cent (or 327) originated in the House, while only 39 per cent (or 237) originated in the Senate. If we are correct that acting first allows a chamber to have greater impact on the content of legislation, then clearly the House has more legislative impact than the Senate.

Behavioral Correlates of Political Support*

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If the concept of political support is to have relevance for macrolevel, systematic questions of political stability and instability, it must first be shown to yield relatively accurate predictions about the political behavior of individuals. But does political support, in and of itself, predict to political behavior? If it is too much to ask that levels of political support predict to specific behaviors such as working for a candidate in an election campaign versus engaging in a petition-signing drive, or engaging in an illegal takeover of a building versus a violent demonstration, can different levels of political support at least be linked to general modes of political behavior?

A variety of theories coincide in the prediction that different levels of political support will be associated with different broadly conceived modes of political behavior.[1] When political support is high, either apathy or participation in "conventional" electoral or pressure group politics is predicted to occur. When political support is low, the likely result is either withdrawal or else participation in "unconventional" forms of protest and aggressive political behavior. The consensual hypotheses thus see high political support as a necessary condition of conventional political behavior, low political support as a necessary condition of unconventional political behavior.

To date, few studies have investigated relationships between levels of political support and *both* conventional and unconventional kinds of political behavior. Notable exceptions are survey research studies carried out in Detroit in 1971 and in the San Francisco Bay area in 1972.[2] Political support, in the Detroit study, was operationalized by a five-item Political Trust index measuring degree of belief that local and national government generally does the right things and serves the public interest. Although the magnitude of association was not especially great, it turned out that, among blacks, the Political Trust index was associated not only with unconventional behavior but also with conventional behavior. Similar results were obtained in the San Francisco Bay area study. Here political support was operationalized by an eight-item Political Alienation index measuring degree of estrangement from the political process at the national level. When a multiple regression analysis was performed across all respondents, the Political Alienation index was estimated to have a positive (albeit small) effect on a four-item Conventional Participation index, in addition to the expected positive effect on a four-item Protest Participation index. These findings are disquieting theoretically. For if low political support can lead to conventional as well as to unconventional political behavior, then knowledge, say, that support was on the decline, might tell one precious little about exactly what kind of political action people would be likely to take.

A similarly provocative finding from the San Francisco Bay area study is that the Conventional Participation index correlates with the Protest Participation index at $r = .394$. This bespeaks a fairly strong linear relationship; and it suggests that these two modes of behavior are by no means mutually exclusive empirically,

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[1] See Ada W. Finifter, "Dimensions of Political Alienation," *American Political Science Review*, 64 (June, 1970), esp. Figure 1 at page 407; Jeffery M. Paige, "Political Orientation and Riot Participation," *American Sociological Review*, 36 (October, 1971), esp. Chart 1 at p. 812; David C. Schwartz, *Political Alienation and Political Behavior* (Chicago: Aldine, 1973), esp. p. 156.

[2] On the Detroit study, see Joel D. Aberbach and Jack L. Walker, *Race in the City* (Boston: Little, Brown, 1973); on the San Francisco Bay area study, see Jack Citrin, Herbert McClosky, J. Merrill Shanks, and Paul M. Sniderman, "Sources and Consequences of Political Alienation: A Progress Report on Indicator Development," paper delivered at the conference, "Public Support for the Political System," Madison, Wisconsin, August 12–17, 1973.

and should not be treated as such conceptually. Hence, in order to develop more precise predictions about behavioral consequences of political support, it would be necessary to construct a typology of political behavior which included mixed types – participation in both conventional and unconventional behavior – as well as pure types.

In this paper, relationships between political support, indices of conventional and unconventional political behavior, and a typology of political behavior are investigated. The typology is constructed by combining different levels of conventional and unconventional behavior so as to define the following political action-types: behavioral withdrawal, conformative participation, reformist action, realist revolutionary action, nonconformative opposition. A simple, three-variable political support theory of political action-type is then proposed and tested.

Measures of Political Support and Political Behavior

The data reported here come from a survey carried out in West Germany in 1973 by *Infratest.*[3] Interviews of approximately 90 minutes duration were administered to 259 respondents (137 males, 122 females) drawn from three research sites: Dorn-Dürkheim, a village in the predominantly agricultural state of Rheinland-Pfalz; Schönau, a working-class section of Mannheim, an industrial inland port on the Rhine River; and the University of Heidelberg. The sites were chosen in order to provide a diversity of community types and to secure greater than normal variation on attitudinal and behavioral variables relevant to political support and opposition.[4] Random samples of persons aged 18 years and over were drawn from Dorn-Dürkheim and Mannheim-Schönau; a purposive sample, including professors, lower-ranking faculty members, and students was drawn from the University of Heidelberg.

The index of political support includes items which measure the degree to which a person believes that (1) Parliamentary laws should be obeyed, even if one does not agree with them; (2) the national government can be generally trusted to do the right thing; (3) the police deserve great respect; (4) the courts give fair treatment; (5) the basic political views of the national political leadership have, in the recent past, been sound. The focus is thus on institutions which wield the power of the state and the content consists of very general evaluative statements about these structures of political authority.[5] The items used to construct the political support measure were administered as

Table 1. Inter-item Correlation Matrix (r): Support for the Structure of Political Authority

Items (numbered as in text)	(1)	(2)	(3)	(4)	(5)
(1) *Selbst wenn man mit dem Inhalt eines Gesetzes ganz und gar nicht übereinstimmt: Sobald es vom Parlament verabschiedet worden ist, sollte man dem Gesetz gehorchen.*	--				
(2) *Man kann sich im allgemeinen darauf verlassen, dass die Bundesregierung das Richtige tut.*	.33	--			
(3) *Alles in allem genommen, verdient die Polizei in der Bundesrepublik grossen Respekt.*	.49	.44	--		
(4) *Die Gerichte in der Bundesrepublik gewähren jedermann einen fairen Prozess – es spielt dabei keine Rolle, ob er arm ist oder reich, gebildet oder ungebildet.*	.53	.40	.62	--	
(5) *Die Grundeinstellung der in der Bundesrepublik politisch tonangebenden Personen war rückblickend immer in Ordnung.*	.23	.48	.42	.47	--

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[3] The interview schedule was prepared by the author and Jonathan Pool. We are grateful for the helpful advice received from many colleagues at the University of Mannheim, especially Rudolf Wildenmann and Uwe Schleth, and from Yola Laupheimer, director of the fieldwork, and Dorothea Reppart, assistant director, both of the Economics Research Bureau, Infratest GmbH & Co., Munich.

[4] Dorn-Dürkheim had shown relatively high support for the NPD in the late 1960s; Mannheim-Schönau had shown relatively high support during the late 1940s and early 1950s for the KPD (there has been hardly any electoral support anywhere in present-day West Germany for the new version of the Communist party – DKP); the University of Heidelberg had experienced relatively high levels of disruptive collective protest during the academic year 1972–73.

[5] These items are intended to measure the concept of diffuse political support, proposed by David Easton. For his most recent elaboration of the

part of a 14-item card-sort. The sorting sheet contained 7 boxes, labeled according to Likert-type format: strong agreement, moderate agreement, weak agreement, neutral, weak disagreement, moderate disagreement, strong disagreement.[6] Scores ranging from "0" (strong disagreement) up to "6" (strong agreement) were assigned to the different intensities of agreement. Table 1 shows the matrix of product-moment correlations computed for the political support items. Relatively strong correlations obtain almost across the board (the mean inter-item correlations is .44), indicating that these items are unidimensional.

Conventional political behavior was measured by a set of items referring to standard ways by which a person might participate in an election campaign. Respondents were asked if within the past five years they had (1) spoken with people and tried to persuade them to vote for a particular candidate or party; (2) worn a campaign button or affixed a sticker to their car; (3) attended political discussions or rallies; (4) spent money to help a candidate or a party; (5) worked for a candidate in a campaign. The Conventional Political Behavior index is the sum of the number of campaign-related activities in which a respondent had engaged.

Items selected to measure unconventional political behavior are those which exemplify aggressive political protest: action either illegal and disruptive of the normal functioning of government or illegal and explicitly violent. Unconventional political behavior was limited to illegal, aggressive action so as to avoid the possibility that a correlation between conventional and unconventional behavior could, in part, be an artifact arising from existential commonality between items in both indices. (For example, nonaggressive actions such as participating in peaceful rallies or marches, though unconventional, are nevertheless similar to electoral participation in that both involve "within-system" behavior which does not challenge the authority of the state – hence both are essentially conformative in nature.) The Aggressive Political Behavior index is the sum of the number of positive responses to questions asking whether or not a respondent had engaged in any of the following actions: (1) takeovers of factories, offices, or other buildings; (2) involvement in fights (battles with the police, battles with other demonstrators); (3) involvement with a group which wants to bring down the government by violent means; (4) destruction of public property (smashing windows, pulling down street signs); (5) involvement in a wildcat strike.[7]

The Aggressive Political Behavior index is by no means independent of the Conventional Political Behavior index; they correlate positively at $\text{Tau}_b = .365$, a value statistically significant at the .001 level. A different correlation coefficient for ordinal variables, Gamma, is sensitive to the presence of a monotonically nondecreasing relationship – as rank-order on one variable increases, rank-order on the other variable either increases or stays the same. In contrast to Tau_b, which can attain unity only when all cases fall on the diagonal of a contingency table, Gamma also reflects "corner" correlations: it can attain unity when all cases are located along two adjacent edges of a contingency table.[8] The Gamma correlation between the Aggressive Political Behavior index and the Conventional Political Behavior index is .705. The fact that the Gamma correlation is much higher than the Tau_b correlation indicates that the form of the relationship between Aggressive Political Behavior and Conventional Behavior tends to be monotonically nondecreasing rather than monotonically increasing.

The political behavior indices are categorized into Low (absence of participation), Medium (participation in one or two activities), and High (participation in three or more activities) levels and cross-classified in Table 2. As can be seen from this table, those who show no participation in conventional activity are virtually certain not to participate in aggressive activity; those who participate in conventional activity are unlikely to participate in aggressive activity; but practically all of those who participate in aggressive activity also participate in conventional activity. While one would not want to argue that this particular form of relationship is necessarily *the* most likely pat-

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concept, see Easton, "A Re-Assessment of the Concept of Political Support," *British Journal of Political Science*, 5 (October, 1975), 435–457.

[6] There is some reason to believe that the format of the cardsort sheet might have encouraged respondents with no real opinion about a given item to place it in the Neutral box, since the sheet did not contain a Don't Know box. At least, when the Neutral category is excluded, correlations with other variables go up. But there is no systematic way to determine which respondents "really" meant "neutral" and which respondents really meant "don't know."

[7] The phrasing for many of these items comes from a pretest questionnaire developed for a substantively parallel study which is being carried out in West Germany under the direction of Max Kaase and Hans Klingemann of the Zentrum für Umfragen, Methoden und Analysen, Mannheim.

[8] See Johan Galtung, *Theory and Methods of Social Research* (Oslo: Universitetsforlaget, 1967), p. 223.

tern of association, it does seem to be considerably more plausible than a number of others. For example, it is more plausible than a straight-forward monotonically-increasing relationship, since there is no causal rationale for predicting that low rank on one variable should usually occur *only* with low rank on the other, medium rank on one *only* with the medium rank on the other, and high rank on one *only* with high rank on the other. Also, any nonmonotonic patterns would be highly implausible. Thus, aside from the null hypothesis, the most plausible remaining candidate is the hypothesis of a monotonically nonincreasing relationship in which the preponderance of cases is located along the Low row on Aggressive Political Behavior and down the Low column on Conventional Political Behavior. This would be the situation if only pure modes of behavior tended to occur. But no support for it has been found in these data; and no support was found in the San Francisco Bay area survey. This leads to the conclusion that it will generally be necessary to account for mixed modes of behavior in addition to pure modes.

The cells defining a political behavior typology that includes pure and mixed modes of political behavior are shown enclosed by boxes in Table 2. This conceptualization of political-action type has been inspired especially by the theories of Finifter and of Schwartz.[9] In Finifter's dimensions-of-alienation theory the behavioral responses are apathy, conformative participation, reform-oriented protest, revolution, and withdrawal; behavioral orientations in Schwartz' theory are conformity, reformism, revolutionism, withdrawal, and ritualism.

In both theories, withdrawal refers to the absence of any kind of political action. Hence, it is logical to define a Behavioral Withdrawal type by the combination of the Low category on Conventional Political Behavior and the Low category on Aggressive Political Behavior.

Conformist behavior, in both theories, is equated with conventional activity. But since aggressive political action may often co-occur with conventional action in a person's behavioral repertoire (mixed mode), it would seem best to define *conformist* behavior as participation *only* in conventional electoral and pressure-group politics. Hence, the Conformative Participation type is defined by the combination of the Medium or High categories on Conventional Political Behavior and the Low category on Aggressive Political Behavior.

Finifter views reformism as actions of protest groups working within the institutional framework of society. Schwartz's view is somewhat vague about behavioral referents, since reformism is measured by items such as: "I'd like to help bring about major changes in American politics – a peaceful revolution in our attitudes, priorities, policies, and institutions."[10] Reformism would seem to be best conceptualized as preeminently a mixed mode: the use of some aggressive tactics in conjunction with conventional activity. Accordingly, the Reformist Action type is defined by the combination of the Medium or High categories on Conventional Political Behavior and the Medium categories on Aggressive Political Behavior.

Revolutionism, in both theories, refers to eschewal of conventional activity in favor of political aggression. In Finifter's view, revolutionism entails "fundamental, radical rejection

Table 2. Derivation of Index of Political-Action Type

Aggressive Political Behavior	Conventional Political Behavior			
	Low	Medium	High	*N*=
Low	I 122	II 48	33	203
Medium	V 1	III 9	11	21
High	0	IV 3	4	7
N=	123	60	48	231

Political-Action Type Index
- Type I: Behavioral Withdrawal
- Type II: Conformative Participation
- Type III: Reformist Action
- Type IV: Realist Revolutionary Action
- Type V: Nonconformative Opposition

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[9] See Finifter, "Dimensions of Political Alienation;" Schwartz, *Political Alienation and Political Behavior.*

[10] Schwartz, p. 163.

of established methods of accomplishing political goals."[11] Revolutionism is thus conceived as a pure rather than a mixed mode. This notion of revolutionism is best captured by the action type labeled Nonconformative Opposition, defined by the combination of the Low category on Conventional Political Behavior and the Medium or High categories on Aggressive Political Behavior. Two subtypes could further be distinguished. If one defined the High category of an aggressive political behavior index as necessarily including involvement in at least one violent action (as is the case here), then one might differentiate between Nonconformative Opposition which is not necessarily violent – an action type akin to what Schwartz calls radical reformism[12] – and a more extreme kind of Nonconformative Opposition which does involve participation in at least some political violence.

Of course, revolutionaries frequently have sought to overthrow the system from within as well as from without – e.g., during the 1920s in Germany, members of the Communist party and of the Nazi party engaged in violent political aggression *and* participated in conventional electoral politics; today, members of the Communist party and of the Neo-Fascist party in Italy have shown a similar proclivity for such a mixed mode. And this is certainly a realistic strategy for the revolutionary, though it may entail ideological compromise which can eventually deradicalize a revolutionary movement.

A mixed mode comprising participation in a high level of aggressive activity without eschewal of conventional activity is not a component of the behavior typologies proposed by either Finifter or Schwartz. But it is clearly a logical possibility which, on empirical grounds, warrants explicit consideration as a distinct action type. Labeled Realist Revolutionary Action, this type is defined by the High category on Aggressive Political Behavior and the Medium or High categories on Conventional Political Behavior.

The political behavior typology proposed here does not include apathy or ritualism as separate types. As Finifter defines it, apathy refers to very low political involvement.[13] In this sense, apathy would be subsumed under the Behavioral Withdrawal type. As a specifically behavioral orientation, Schwartz's conceptualization of the ritualism type is unclear. He defines it by items such as: "There are some public policies that I still advocate because I used to believe that they would work, even though now I am not very convinced that they would work;" "You've got to keep trying to get good laws and fairness in politics, but it's really not much use."[14] But these items would appear at face value to measure some sort of *attitude* of political futility rather than a behavioral orientation.

With respect to general definition of each of the political-action types, how one goes about differentiating between medium and high levels of conventional political behavior is immaterial, but the distinction between low and medium levels is, of course, important. Here, I would include participation limited to the act of voting under the low category of conventional political behavior and relegate participation in other conventional actions to the medium and high levels.[15] Also, under the low category of aggressive political behavior, I would include participation in any kind of unconventional behavior which is legal; and I would differentiate the high from medium aggressive behavior by defining the high category as necessarily including participation in some form of political violence.

A check on the concurrent validity of the Political-Action Type index can be made by testing predictions about relationships which should hold between action type and intention to engage in various political behaviors.[16] Given the definition of each action type, the following hypotheses should be supported:

(1) The Behavioral Withdrawal type should show a negative intention toward participation in conventional and aggressive political activity.

(2) The Nonconformative Opposition type should show a negative intention toward participation in conventional political activity.

(3) The Conformative Participation, Reformist Action and Realist Revolutionary Action types should not show a negative intention toward participation in conventional political activity.

(4) The Conformative Participation type should show a negative intention toward participation in aggressive political activity.

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[11] Finifter, p. 408.

[12] Schwartz, p. 140.

[13] Finifter, p. 407.

[14] Schwartz, pp. 163, 179.

[15] There is, of course, an enormous gap between frequency of voting and frequency of engaging in the other conventional actions. See, for example, Lester W. Milbrath, *Political Participation* (Chicago: Rand McNally, 1965), pp. 16–27.

[16] On the topic of concurrent validity, or criterion validation, see David C. Leege and Wayne L. Francis, *Political Research* (New York: Basic Books, 1974), pp. 160–162.

(5) The Reformist Action, Realist Revolutionary Action, and Nonconformative Opposition types should not show a negative intention toward participation in moderately aggressive political activity.

(6) The Reformist Action type should show a negative intention toward participation in highly aggressive political activity.

(7) The Realist Revolutionary Action type should not show a negative intention toward participation in highly aggressive political activity. Of course, since there is only one instance of Nonconformative Opposition in this sample, prediction 2 cannot be tested, and prediction 5 can be tested only in regard to Reformist Action and Realist Revolutionary Action.

Table 3 shows the results when the dependent variables are measures of intention toward participation in conventional electoral activity – working for a candidate or party in an election campaign; a moderately aggressive action – takeovers of buildings; and a highly aggressive action – use of violence against the government. The predictions about the expected pattern of behavioral intention which should be manifested by each action type receive strong confirmation across the board.

Political Support and Political Behavior

According to the political trust theory developed by Gamson and refined in certain respects by Paige, high political trust leads either to conventional behavior or to apathy ("passive adjustment"), depending upon whether sense of political efficacy is high or low. Low political trust leads either to aggressive political behavior or to alienated or resentful withdrawal, again depending upon whether felt political efficacy is high or low.[17] Finifter's dimensions-of-alienation theory utilizes the concept of political normlessness – a variable similar in meaning to political trust-distrust. Combinations of low political normlessness and the two political efficacy conditions are predicted to lead to the identical behavioral responses predicted by the Gamson-Paige theory. The predicted result of high political normlessness is a reform orientation, given high political efficacy; when political efficacy is low, the predicted result of high political normlessness is extreme disengagement, either in the form of revolutionary opposition or complete withdrawal, depending upon whether sense of personal efficacy is high

Table 3. Relationships Between Political-Action Type Index and Three Behavioral Intentions

	Political-Action Type							
	Behavioral Withdrawal		Conformative Participation		Reformist Action		Realist Revolutionary Action	
	%	N=	%	N=	%	N=	%	N=
Conventional Action:								
Work for candidate or party in election								
Would not do	79	(88)	19	(15)	05	(1)	00	(0)
Might do, would do	21	(23)	82	(66)	95	(18)	100	(7)
Totals	100%	(111)	101%[a]	(81)	100%	(19)	100%	(7)
Moderately Aggressive Action:								
Takeover offices, factories, other buildings								
Would not do	94	(104)	76	(61)	25	(5)	00	(0)
Might do, would do	06	(7)	24	(19)	75	(15)	100	(7)
Totals	100%	(111)	100%	(80)	100%	(20)	100%	(7)
Highly Aggressive Action:								
Use violence against government								
Would not do	94	(104)	88	(70)	85	(17)	29	(2)
Might do, would do	06	(7)	13	(10)	15	(3)	71	(5)
Totals	100%	(111)	101%[a]	(80)	100%	(20)	100%	(7)

[a] Rounding error.

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[17] Paige, "Political Orientation and Riot Participation," pp. 811–812.

or low.[18] The alienation theory proposed by Schwartz asserts that if estrangement from the political system is low, conventional political behavior is the likely response. If estrangement from the system is high, reformism or revolutionism are likely to result, given that variables conducive to a strong sense of personal efficacy are also high; withdrawal or ritualism are likely to occur when estrangement from the system is high and the variables conducive to a strong sense of personal efficacy are low.[19] In effect, these theories tend to conceive of high political support as a necessary but not sufficient condition of conventional political behavior, and low political support as a necessary but not sufficient condition of aggressive political behavior.

How have these theories fared empirically? None of the predicted effects of interaction between political support and other variables on types of political behavior has yet received any clear-cut confirmation. The prediction of a positive relationship between political support and conventional political behavior also has not received clear-cut confirmation, and the evidence from other studies, cited earlier, militates against it. Only the prediction of a negative relationship between political support and aggressive political behavior has received support.[20]

The hypothesis of a monotonically nondecreasing relationship between level of political support and level of conventional political behavior can be expressed as follows:

> When political support is low or medium, conventional political behavior will be low; when political support is high, conventional political behavior will be low, medium, or high.

This is a corner correlation hypothesis, appropriately tested for ordinal variables by the Gamma coefficient. The hypothesis which predicts a monotonically nonincreasing relationship between level of political support and level of aggressive political behavior is also a corner correlation hypothesis and may be expressed as:

> When political support is low, aggressive political behavior will be low, medium, or high; when political support is medium or high, aggressive political behavior will be low.

Table 4 shows the results of the test of these hypotheses for the West German sample. Categories of low, medium, and high political support are defined by dividing the Support for the Authority Structure index, which ranges from 0–30, into approximately equal thirds (Low = 0–10; Medium = 11–20; High = 21–30). The boxed-in cells are those in which all cases would fall if the hypotheses were to receive perfect confirmation.

The negative sign attached to Gamma tells us at once that the hypothesis about conventional political behavior is disconfirmed. In fact, the data provide moderately strong confirmation for the obverse of the predicted relationship – corner correlation in the opposite direction. This is evidenced by the fact that a majority – 62 per cent – of those at High SAS score Low on the Conventional Political Behavior index,

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[18] Finifter, "Dimensions of Political Alienation," p. 408.

[19] Schwartz, *Political Alienation and Political Behavior,* pp. 158–159.

[20] Finifter's dimensions-of-alienation theory has not been tested. Paige tested the political trust theory on a sample of black males interviewed some six months after the July, 1967, civil disorders in Newark. He found that among blacks high on political information, there was a statistically significant, positive linear trend between participation in the riots and level of political distrust. But a negative linear trend between being registered to vote in 1964 and level of political distrust was very slight. Since the question of whether one is registered to vote is not a very reliable (or even desirable measure of participation in conventional political activity), the test of the prediction of a negative relationship between level of *dis*trust and conventional activity probably is best ignored. Also, since the equivalence of amount of political information (used as an indicator of political efficacy) and sense of political efficacy is debatable, it probably is best not to regard the information variable as one that enables any kind of precise test of political efficacy and political trust interaction hypotheses. Schwartz's alienation theory has not been tested against measures of overt behavior. Schwartz does report correlations between a national political alienation index and measures of behavioral orientation for a sample of students and faculty at the University of Pennsylvania and a sample drawn from black ghettoes in North and West Philadelphia. In the case of the University of Pennsylvania sample, a statistically significant negative correlation obtained between the political alienation index and the behavioral orientation labeled Conformity; but this correlation is suspect because, since three of the four items in the Conformity index appear to be measuring the same thing as the items in the political alienation index, political alienation is, at least in part, just being correlated with itself (see Schwartz, pp. 269, 273). In the case of the Philadelphia ghetto sample, the meaning of the statistically significant negative correlation between political alienation and Conformity is unclear because the single item used to measure the conformity orientation does not appear to have much at all to do with conventional political behavior *per se* (see Schwartz, p. 179). The political alienation index does show statistically significant positive correlation with Revolutionism in both samples. However, tests of the hypothesis of an interaction between political alienation and sense of personal efficacy in the determination of Revolutionism are performed incorrectly (all variables are entered additively into a multiple regression analysis).

Table 4. Relationships Between Political Support and Indices of Political Behavior

Conventional Political Behavior	Support for the Authority Structure: Low %	Low N=	Medium %	Medium N=	High %	High N=
Low	09	(3)	54	(60)	62	(53)
Medium	38	(13)	26	(29)	27	(23)
High	53	(18)	20	(22)	12	(10)
Totals	100%	(34)	100%	(111)	101%[a]	(86)
			Gamma = –.445			

Aggressive Political Behavior	Support for the Authority Structure: Low %	Low N=	Medium %	Medium N=	High %	High N=
Low	55	(17)	88	(95)	100	(80)
Medium	26	(8)	(11)	(12)	00	(0)
High	19	(6)	01	(1)	00	(0)
Totals	100%	(31)	100%	(108)	100%	(80)
			Gamma = –.879			

[a] Rounding error.

while those at Low SAS tend to score at Medium or High on the Conventional Political Behavior index.

The hypothesis about aggressive political behavior is handsomely confirmed. The sign of Gamma is in the predicted direction and the magnitude of the coefficient is quite high, reflecting the fact that only 13 out of the 219 cases with scores on both variables are disconfirming instances of the hypothesis.

The answer to one of the research questions is thus negative. Different levels of political support are *not* associated with different broadly defined modes of political behavior. Political support correlates in the same direction with aggressive and conventional political behavior.

As we know, however, those who score medium or high on the *Aggressive* Political Behavior index tend also to score medium or high on the *Conventional* Political Behavior index. Therefore, in order to get a sharper test of political support hypotheses derived from the political trust, dimensions-of-alienation, or alienation theories, we need to look at the typology of political behavior which distinguishes between the pure types and the mixed types. With Political-Action Type as the dependent variable, one would hypothesize that high political support is a necessary condition of Conformative Participation, that low political support is a necessary condition of Realist Revolutionary Action and Nonconformative Opposition, that perhaps medium support would be a necessary condition of Reformist Action,[21] and that Behavioral Withdrawal could occur at all levels of support.

Though one might conceivably rank order Types I–IV of political action on the basis of something like degree of challenge to a political regime, the Political-Action Type index is essentially a nominal variable. Therefore, the strength of the relationship between SAS and Political-Action Type will be indexed by Cramer's V, a correlation coefficient appropriate for the nominal level of measurement. As can be seen from Table 5, there is a moderately strong relationship between SAS and the Political-Action Type index.[22] The percentages show that low political support is indeed very close to being a necessary condition of Realist Revolutionary Action. And Behavioral Withdrawal does occur at all levels of political support. But medium political support is not a necessary condition of Reformist Action. And most disconcerting is the finding that Conformative Participation is more likely to occur at *low* instead of high support

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[21] This hypothesis is suggested by Paige on p. 818 of "Political Orientation and Riot Participation," where he proposes that civil rights activity will be the preferred political tactic of those who are high on political efficacy and medium on political trust.

[22] The inclusion of the single case of Nonconformative Opposition in Table 5 does not affect the stability of chi-square, as evidenced by the fact that *V*, a chi-square-based statistic, is .3995 when the Nonconformative Opposition case is excluded.

Table 5. Relationship Between Political Support and Political Action-Type Index

	Support for the Authority Structure					
	Low		Medium		High	
Political-Action Type	%	N=	%	N=	%	N=
Behavioral Withdrawal	03	(1)	55	(58)	61	(48)
Conformative Participation	50	(15)	32	(34)	39	(31)
Reformist Action	23	(7)	11	(12)	00	(0)
Realist Revolutionary Action	20	(6)	01	(1)	00	(0)
Nonconformative Opposition	03	(1)	00	(0)	00	(0)
Totals	99%[a]	(30)	99%[a]	(105)	100%	(79)
	$\chi^2 = 68.48$	$p<.001$	$V = .400$			

[a]Rounding error.

Thus, even when a typology of political behavior is constructed so as to differentiate various mixed and pure behavioral types, one is still left with the problem that when support falls to a low level, it is difficult to predict with much accuracy exactly which type of behavior is likely to result. And existing theory is of no help here, because it is premised on the notion that Conformative Participation will not occur when political support is low, whereas these data show Conformative Participation to be a likely response to low support.

Could the results in some way be confounded by type of community? The three communities represent a *working-class* milieu, a *peasant* milieu, and an *intelligentsia* milieu. One plausible hypothesis of a contextual confounding effect might run as follows: In the working-class and peasant milieus, community norms may serve to inhibit radical behavior even when an attitude conducive to it, such as low political support, is present. Thus, when community type is controlled for, either Behavioral Withdrawal or Conformative Participation might be the response to low political support in the working-class and peasant milieus; whereas, in the intelligentsia milieu, where community norms that might inhibit radical behavior may be weak or nonexistent, those low on political support might either withdraw or else engage in one of the action types involving aggressive behavior.

Unfortunately, as Table 6 shows, almost all of the low political support observed in this sample comes from the intelligentsia milieu. This is not unexpected for present-day West Germany, since most of the behavioral opposition to the regime in the recent past has come from the intelligentsia sector of society rather than the working-class or peasant sectors.[23] But in the absence of low support from the working-class and peasant milieus, it is impossible to carry out a sharp test of the hypothesis of a contextual confounding effect. At any rate, in the intelligentsia milieu, where low support *does* occur, the incidence of conformative participation does not drop off substantially, thus suggesting at least partial disconfirmation of the hypothesis.

Political Support Theory of Political-Action Type

Given that withdrawal can occur at any level of support, and that one could thus only expect different levels of support to be a *necessary* condition of behavior, still, neither medium nor high levels of political support are a necessary condition of anything, and low political support, while close to a necessary condition of Realist Revolutionary Action, also is associated with other behavioral types. Therefore, to predict to behavioral consequences, it appears that interaction between political support and other variables will have to be considered.

The theoretical model for prediction of political-action type to be proposed here draws not only on the concept of diffuse support for the structure of political authority but also on the concept of belief in the efficacy of past political aggression, utilized previously in a partial theory of political violence,[24] and on

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[23]Rural communities such as Dorn-Durkheim did show rather high support for the rightist party, *Nazionaldemokratische Partei Deutschlands,* during the late 1960s; but there is some question whether this party is actually an antiregime party in the same sense as, say the German Communist Party. In this sample, three of the four NPD identifiers from Dorn-Durkheim are at the Medium level on SAS, the other is at the High level. Of the four respondents from Heidelberg who identify with the DKP (newly reconstructed Communist party), three are at the Low level of SAS, the other is at the Medium level.

[24]Edward N. Muller, "A Test of a Partial Theory of Potential for Political Violence," *American Political Science Review,* 66 (September, 1972), 928–959.

Table 6. Relationship Between Political Support and Political-Action Type Index, Controlling for Kind of Community

	Kind of Community											
	Mannheim-Schönau: Working-Class						Dorn-Dürkheim: Peasant					
	Support for the Authority Structure						Support for the Authority Structure					
Political Action Type	Low		Medium		High		Low		Medium		High	
	%	N=	%	N=	%	N=	%	N=	%	N=	%	N=
I	33	(1)	88	(38)	81	(21)	--	--	65	(20)	61	(25)
II	33	(1)	09	(4)	19	(5)	--	--	36	(11)	39	(16)
III	00	(0)	02	(1)	00	(0)	--	--	--	--	--	--
IV	-	--	--	--	--	--	--	--	--	--	--	--
V	33	(1)	00	(0)	00	(0)	--	--	--	--	--	--
Totals	100%	(3)	99%[a]	(43)	100%	(26)			101%[a]	(31)	100%	(41)

	Kind of Community					
	Heidelberg: Intelligentsia					
	Support for the Authority Structure					
Political Action Type	Low		Medium		High	
	%	N=	%	N=	%	N=
I	00	(0)	00	(0)	17	(2)
II	52	(14)	61	(19)	83	(10)
III	26	(7)	36	(11)	00	(0)
IV	22	(6)	03	(1)	00	(0)
V	--	--	--	--	--	--
Totals	100%	(27)	100%	(31)	100%	(12)

Key: Type I: Behavioral Withdrawal
Type II: Conformative Participation
Type III: Reformist Action
Type IV: Realist Revolutionary Action
Type V: Nonconformative Opposition

[a] Rounding error.

the concept of perceived personal political influence, utilized in the political trust and dimensions-of-alienation theories. Support for the authority structure, in interaction with efficacy of past aggression and personal political influence, is assumed to channel other kinds of dissatisfaction into different types of political behavior. This assumption need not necessarily be true. Relative deprivation in any of its many guises could have an impact on political-action type independently of diffuse political support.[25] If so, the model would have to be made more complex. But, for present purposes, relative deprivation will be relegated to the status of a likely direct cause only of variation in support for the structure of political authority.[26]

The model for prediction of political-action type will be formulated on the basis of interaction between low, medium, and high levels of support for the authority structure, efficacy of past aggression, and personal political influence. In addition, it is necessary under one condition in the model to invoke a dichotomous variable, the presence or absence of identification with an antiregime party, in order to specify which action type is most likely. The model can be expressed by the following propositions:

A. Given low support for the authority structure:

Ia. If personal political influence is low and efficacy of past aggression is low or medium, then Behavioral Withdrawal.

IIa. If personal political influence is medium or high and efficacy of past aggression is low, then Conformative Participation.

IIIa. If personal political influence is medium or high and efficacy of past aggression is medium, then Reformist Action.

IVa. If personal political influence is medium or high and efficacy of past aggression is high; or if personal political influence is low, efficacy of past aggression is high, and identification with an antiregime party is present, then Realist Revolutionary Action.

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[25] This is the role proposed in Ted Robert Gurr's, *Why Men Rebel* (Princeton: Princeton University Press, 1970), though not in his "A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," *American Political Science Review*, 62 (December, 1968), 1104–1124.

[26] Empirical evidence supporting such an assumption is reported in Muller, pp. 931–932.

Va. If personal political influence is low, efficacy of past aggression is high, and identification with an antiregime party is absent, then Nonconformative Opposition.

B. Given medium support for the authority structure:

Ib. If personal political influence is low, regardless of efficacy of past aggression, then Behavioral Withdrawal.

IIb. If personal political influence is medium or high and efficacy of past aggression is low, then Conformative Participation.

IIIb. If personal political influence is medium or high and efficacy of past aggression is high, then Reformist Action.

C. Given high support for the authority structure:

Ic. If personal political influence is low or medium, regardless of efficacy of past aggression, then Behavioral Withdrawal.

IIc. If personal political influence is high, regardless of efficacy of past aggression, then Conformative Participation.

The derivation of these predictions is shown pictorially in Chart 1, where support for the authority structure, personal political influence, and efficacy of past aggression are cross-classified and the behavioral prediction is listed for each set of cells subsumed under a proposition in the model.

In all but one instance, at least a modicum of personal political efficacy is hypothesized to be a necessary condition of political action. This is in line with Paige's elaboration of the political trust theory, according to which low political efficacy leads either to alienated withdrawal or passive adjustment, whereas high political efficacy leads to some form of political behavior, either conventional or unconventional. The exception to the rule is individuals who are alienated from the authority structure, believe that they have no influence over what the government does, but have come to perceive that aggressive political behavior has been quite helpful to other groups in the society. In this case, it is expected that their belief in the efficacy of aggressive action will spur them to act on their political alienation by engaging in Nonconformative Opposition. We need, however, to introduce one additional condition: Nonconformative Opposition will be the likely type of behavior only if any established political parties of a radical or revolutionary bent are not attractive. For if an individual disposed to Nonconformative Opposition by virtue of low political support, low political influence, and high perceived efficacy of past aggression comes to identify with an antiregime party,

Chart 1. Model for Prediction of Political-Action Type

<table>
<tr><th rowspan="3">Efficacy of Past Aggression</th><th colspan="9">Support for the Authority Structure</th></tr>
<tr><th colspan="3">Low
Personal Political Influence</th><th colspan="3">Medium
Personal Political Influence</th><th colspan="3">High
Personal Political Influence</th></tr>
<tr><th>Low</th><th>Medium</th><th>High</th><th>Low</th><th>Medium</th><th>High</th><th>Low</th><th>Medium</th><th>High</th></tr>
<tr><td>Low</td><td rowspan="2">Ia
BW</td><td colspan="2">IIa
CP</td><td rowspan="4">Ib
BW</td><td colspan="2">IIb
CP</td><td colspan="2" rowspan="4">Ic
BW</td><td rowspan="4">IIc
CP</td></tr>
<tr><td>Medium</td><td colspan="2">IIIa
RA</td><td colspan="2" rowspan="3">IIIb
RA</td></tr>
<tr><td rowspan="2">High</td><td>Va*
NO</td><td colspan="2" rowspan="2">IVa
RRA</td></tr>
<tr><td>**</td></tr>
</table>

*Antiregime Party Identification not present
**Antiregime Party Identification present

Key:
BW: Behavioral Withdrawal
CP: Conformative Participation
RA: Reformist Action
RRA: Realist Revolutionary Action
NO: Nonconformative Opposition

then he would presumably be ready to engage also in conventional activity – though he might not expect it to have much consequence – and Realist Revolutionary Action would be the prediction.

The conjunction of medium or high sense of personal political influence and low or medium political support provides an impetus toward political action in order to achieve desired political change. If political support is low, individuals will not be disposed to rely only on conventional tactics in order to achieve change – *except* if they believe that aggressive tactics are basically useless or detrimental. This is how Conformative Participation can occur even when political support is low. Otherwise, the likely result is Reformist Action, when past political aggression is seen as being moderately useful, or Realist Revolutionary Action, when past political aggression is seen as being highly useful.

When political support is at a medium level, dissatisfaction is presumably not sufficiently intense to engender a high level of aggressive political behavior. Therefore, if medium support occurs in conjunction with medium or high personal political influence, the likely result is Reformist Action, except when aggressive behavior is perceived as generally detrimental. Under the latter condition, only conventional tactics will be used, and Conformative Participation is the predicted result.

Those who are highly satisfied with the political system as it is will not be likely to participate in unconventional political activity. Hence, Conformative Participation is the only type of behavior predicted to result at high levels of political support. Since, however, the very supportive individual is quite satisfied with the status quo, he will probably require a high sense of personal influence to be motivated to engage even in Conformative Participation. Therefore, under the condition of high support, Conformative Participation is predicted to result only when personal political influence also is at a high level. And given high support, it is further hypothesized that the efficacy of the past aggression variable will be irrelevant, because highly supportive individuals who *do* believe that aggressive tactics have been beneficial to other groups would, nevertheless, have no motivation to take part in such action themselves.

The concept of personal political influence is measured by a question which asked respondents to give their opinion about whether they had a large influence, a fair amount of influence, a little influence, or no influence at all on how their country is governed. Showing a healthy degree of realism, no one in the sample expressed the opinion that he or she had a great deal of influence. The High category on the Personal Political Influence variable is thus defined by the response, "a fair amount of influence," the Medium category by the response, "a little influence," and the Low category by the response, "no influence."[27]

The concept of belief in the efficacy of past politically aggressive behavior is measured by summing up the number of positive responses (perception that those involved in an aggressive activity had been rewarded or had not experienced adverse consequences as a result of it) to questions about whether the following actions had been beneficial or harmful to the goals of the participants: (1) student groups which have violently disrupted university lectures; (2) groups which have seized public buildings and fought with the police; (3) groups which have engaged in guerrilla warfare tactics against the German government.[28] The Low category on the Efficacy of Past Aggression variable is defined as negative responses to all three items, the Medium category is defined by one positive response, and the High category is defined by two or three positive responses.

The Low, Medium, and High categories on the Support for the Authority Structure variable are as defined previously. The positive category of the dichotomous variable, Antiregime party identification is defined by expression of identification with the German Communist party or with one of the minor, radical splinter parties which have found small constituencies primarily in certain university communities.[29]

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[27] The text of the items is: "*Sind Sie der Ansicht, dass Sie einen grossen Einfluss, einen nennenswerten Einfluss, kaum oder keinen Einfluss darauf haben, wie die Bundesrepublik regiert wird?*"

[28] The text of the items is: "*Um Veränderungen an den Universitäten herbeizuführen, haben Studenten Lehrveranstaltungen gestört und Raume und Mobiliar beschädigt. Was glauben Sie, hat dies den Gruppenzielen im allgemeinen genützt, im allgemeinen geschadet oder weder noch?*" "*Einige Gruppen haben öffentliche Gebäude gestürmt und sich mit der Polizei geliefert, um gegen Tätigkeiten der Regierung zu protestieren. Was glauben Sie. . . .*" "*Einige Geheimorganisationen-so hauptsächlich die Baader-Meinhof-Gruppe-haben zur Guerilla Taktik gegriffen, um gegen die gegenwartigen gesellschaftlichen und staatlichen Strukturen in der Bundesrepublik zu protestieren. Was glauben Sie. . . .*"

[29] The party identification question listed all the major parties. Three respondents from Heidelberg chose the option, "other." Which party this meant was not determined; but one is on fairly safe ground in assuming that "other," in this context, referred to a radical party.

Table 7 shows the test of the ten propositions in the model. A proposition would be disconfirmed if a preponderance of cases manifested an action type other than that predicted. Cells in the table which correspond to the predicted action type for each proposition are shown enclosed by boxes. For the conditions subsumed under propositions Ia and Va, there are hardly any cases, so these propositions must be held in abeyance for the time being.

No proposition is disconfirmed, but proposition IIIa is neither confirmed nor disconfirmed. Of those with low support, medium or high sense of political influence, and belief that past political aggression has been moderately helpful, 43 per cent do manifest Reformist Action as predicted, but an equal proportion also manifests Conformative Participation under these conditions.

The other propositions receive varying degrees of confirmation: IIIb and IVa are only weakly confirmed; IIb and IIc receive moderately strong confirmation; while Ib, Ic, and IIa receive quite strong confirmation. It is noteworthy that the conditions subsumed under proposition IIa yield perfectly accurate prediction. Of course, there are only six cases; nevertheless, the perfect prediction observed here even for this small number does suggest that proposition IIa may well provide a fairly sound explanation for the phenomenon of Conformative Participation even when political support is quite low.

The model clearly predicts better to Behavioral Withdrawal and Conformative Participation than to Reformist Action and Realist Revolutionary Action. Overall, however, the fit to the model is rather good, given that the number of independent variables is small, and given that all respondents who could be scored on the variables are included in the analysis rather than limiting it to high information respondents or those high on political interest or other such variables that might be especially conducive to accurate prediction across subsamples. Cramer's *V*, the index of predictive accura-

Table 7. Relationship Between Political-Action Type Index and Conditions Hypothesized to Predict to Political-Action Type

Political Action Type	Propositions in the Model for Prediction of Political-Action Type											
	Ia		Ib		Ic		IIa		IIb		IIc	
	%	*N*=	%	*N*=	%	*N*=	%	*N*=	%	*N*=	%	*N*=
I	50	(1)	80	(41)	64	(42)	00	(0)	33	(11)	42	(5)
II	00	(0)	12	(6)	36	(24)	100	(6)	58	(19)	58	(7)
III	50	(1)	06	(3)	00	(0)	00	(0)	09	(3)	00	(0)
IV	00	(0)	02	(1)	00	(0)	00	(0)	00	(0)	00	(0)
V	00	(0)	00	(0)	00	(0)	00	(0)	00	(0)	00	(0)
Totals	100%	(2)	100%	(51)	100%	(66)	100%	(6)	101%[a]	(33)	100%	(12)

Political Action Type	Propositions in the Model for Prediction of Political-Action Type							
	IIIa		IIIb		IVa		Va	
	%	*N*=	%	*N*=	%	*N*=	%	*N*=
I	00	(0)	21	(3)	00	(0)	00	(0)
II	43	(3)	36	(5)	22	(2)	00	(0)
III	43	(3)	43	(6)	33	(3)	00	(0)
IV	14	(1)	00	(0)	44	(4)	50	(1)
V	00	(0)	00	(0)	00	(0)	50	(1)
Totals	100%	(7)	100%	(14)	99%[a]	(9)	100%	(2)

$\chi^2 = 257.03$ $p < .001$ $V = .564$

Key: Type I: Behavioral Withdrawal
Type II: Conformative Participation
Type III: Reformist Action
Type IV: Realist Revolutionary Action
Type V: Nonconformative Opposition

[a] Rounding error.

cy, is .564 – a satisfyingly high magnitude.[30] This coefficient would be unity if each proposition predicted to *any* one, but *only* one cell. Since the percentages tell us that there are no instances where a proposition predicts best to an action type contrary to that expected, *V* then tells us, on the whole, how close the propositions come to perfectly predicting each of their expected cells in the table.

Conclusion

According to the theory of political-action type proposed here, Behavioral Withdrawal may occur at any level of political support, depending upon the level of personal political influence and the level of belief in the efficacy of past collective political aggression. When support is high, the only overt behavioral result will be Conformative Participation. When support is medium, either Conformative Participation or Reformist Action are likely results. And when support is low, any of the four behavioral types is likely to occur, depending upon values of personal political influence and efficacy of past aggression, as well as, under one condition, antiregime party identification. Thus, political behavior conducive to political stability (or stagnation, depending on one's perspective) would be virtually guaranteed if political support were high. But political behavior conducive to political stability would not necessarily follow from low support.

This theory suggests an answer to the question of what is likely to happen when political support declines. To get that answer one must know what is happening to personal political influence and belief in the efficacy of past collective political aggression. So long as belief in the efficacy of past aggression stays low, as political support declines, citizens who become alienated from the system are expected either to withdraw or else to rely only upon conventional methods of participation, provided that their sense of personal political influence is high enough to motivate them toward political action. But if they come to believe that aggressive political behavior has not been detrimental to other groups in the society, they will be expected to move toward adoption of the more aggressive, mixed types of behavior. Finally, if belief in the efficacy of past aggression becomes quite high while personal political influence remains low, *and* if there are no antiregime political parties perceived as attractive, then (although this could not be tested with the data at hand) Nonconformative Opposition is expected to take place.

Successful use of aggressive tactics thus can generate a contagion effect, as was the case in the United States during the latter part of the 1960s, and also in Western Europe.[31] Eventually, as the contagion spreads, and more and more of the alienated are mobilized, the authorities normally begin to crack down more and more severely. Then, as belief in the efficacy of past aggression wanes, the contagion effect subsides, and those who are alienated either withdraw again or else shift to working for desired political change only within the established political party system. Of course, if political alienation were relatively widespread to begin with, then the contagious effect of the successful use of aggression by an initial few could well spread to unmanageable proportions, resulting in the outbreak of full-fledged internal war.

Such are some of the predictions about broad-scale behavioral consequences which can be derived from the theory. Of course at this point it is limited basically to three independent variables. Further work needs to be done to see if the addition of variables such as sense of personal efficacy or relative deprivation would increase predictive accuracy enough to warrant its purchase at the cost of reduction in the parsimony of the model.

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[30] If the different propositions predicting to the same action type are collapsed to form five general categories, each predicting to a single action type, Cramer's *V* is .540. If proposition Va and the Nonconformative Opposition type are excluded from the table for want of sufficient cases, Cramer's *V* remains unchanged at three decimal places.

[31] I am speaking, of course, not about unmotivated contagion, as in the random outburst theory of urban riots proposed by Banfield, among others. A good discussion of this appears in David O. Sears and John McConahay, *The Politics of Violence* (Boston: Houghton Mifflin, 1973), chap. 7.

The Cabal in Kabul: Great-Power Interaction in Afghanistan

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Introduction and Background

In the July, 1973 coup in Afghanistan, old and new politics were bewilderingly intertwined. On the one hand, as a *New York Times* editorial commented, the overthrow of King Zahir Shah by his cousin and brother-in-law, Lieutenant-General Mohammad Daud Khan, reflects a medieval mentality of palace intrigues and royal rivalries which might be expected in a country about to leap into the sixteenth century.[1] On the other hand, during the 'fifties and early 'sixties, Afghanistan's pivotal geopolitical features enabled this small, isolated, and traditional nation to play off the United States, the Soviet Union, and the People's Republic of China against one another in a competitive aid-giving game of the most thoroughly contemporary cast.

The purpose of this paper will be to explore the nature and extent of great-power interest and involvement in Afghanistan—first, by outlining the differing interests of the great powers in Afghanistan; second, by examining the possibility of great-power involvement in bringing about the coup; and third, by considering the impact of the coup upon Afghanistan's relations with the three great powers.

In this complex and shifting great-power interaction, no one great power has the outright control that a colonizer would exert on the colonized. Thus, this paper deals with *influence* rather than control, and multilateral *interaction* rather than unilateral or bilateral action. Nor is it within the scope of the paper to explain the genesis of the coup in terms of ethnic cleavage or interest-group activity. Rather, the coup serves as a case study of the dynamics of great-power interaction in the affairs of small states.

The period 1946–53 saw the beginning of radical transformations in the Afghan economy because of changing patterns of development and assistance. British departure from the Asian subcontinent in 1947 removed an important source of military support, leading Afghanistan to the United States for assistance. When the United States refused military aid on the grounds that no amount could make it defensible against a determined Soviet attack, the Afghans were forced to turn to the Soviet Union. Thus, the present Afghan military establishment is largely Soviet-trained and -equipped, a significant, though inconclusive, circumstance in view of the coup of July 17, 1973. During this period Afghanistan assumed a cold-war policy of neutrality and formed closer associations with the emerging Afro-Asian nations. By 1954, this attitude had resulted in a new era of foreign influence characterized by competitive foreign aid.

During Daud Khan's premiership (1953–63) both the United States and the Soviet Union launched substantial foreign aid programs to Afghanistan. Also at this time the issue of "Pakhtoonistan" emerged as a linchpin of Daud Khan's regional policy. (I shall argue that this policy has had its domestic aspects as well.) Afghan-Pakistani relations have been strained over this issue to a greater or lesser degree ever since the creation of Pakistan in 1947. The area called "Pakhtoonistan" by the Afghans consists of two of Pakistan's poorest regions – the Northwest Frontier Province and Baluchistan – both bordering on Afghanistan. Since the people of this sparsely-populated but vast area – like the majority of Afghans – are Pushtu-speaking Muslims, Afghanistan has encouraged separatist tendencies in the area, in part to weaken its southern neighbor and obtain access to the sea, and in part to stimulate internal cohesiveness among diverse tribal factions by espousing a popular foreign policy.

With Daud Khan's removal from the premiership in 1963, the "Pakhtoonistan" issue subsided. By the mid-'60s, Afghanistan had acquired a changing international perspective, perhaps reflecting the reduction in competitive assistance. Relying less on external aid, Afghanistan inevitably turned toward increased regional diplomacy in an attempt to improve relations with both Iran and Pakistan. It is not unreasonable to speculate that with Daud Khan's resumption of power in 1973, the re-emergence of "Pakhtoonistan" will again strain relations with Pakistan, and possibly also with Iran.[2]

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[1] "Upheaval in Kabul," *New York Times,* July 20, 1973, p. 30.

Interests of the Great Powers in Afghanistan

It is difficult to talk about the policy of any one of the great powers toward Afghanistan in isolation from the policy of the other two great powers. The nation, though small in population, is so strategically placed that the interests and objectives of the United States, the Soviet Union, and the People's Republic of China are not simply influenced by, but in fact are largely determined by each other, rather than by Afghanistan itself. Each of the three great powers acts and interacts with the other two, and it is the cumulative interaction of all three that is simultaneously observed.

The United States. American interests in Afghanistan stem mainly from its military strategic position both in relation to the subcontinent and its geographic location between two Baghdad Pact members, as well as its southern border with the Soviet Union. Since 1953, the United States has donated between $425 and $450 million in aid and loans to Afghanistan, as compared to $1.5 billion from the Soviet Union, and about $72 million from the People's Republic of China.[3]

In early 1970 a satellite-tracking team in Kabul began a project designed to provide more accurate maps.[4] The project was suported by the U.S. Agency for International Development, and conducted by a team from New Mexico State University in a mobile tracking station. Interestingly, this project began at a time when U.S. installations in other nonaligned countries were being phased out. The station itself opened in March, four months after Pakistan requested the closing of a U.S. satellite-tracking station in Peshawar. Both U.S. and Afghan officials stated that the installation would not jeopardize Afghan neutrality. Apparently the Afghan government had ensured that the stations in Afghanistan operate within the officially designated purposes, so that they could not undertake the functions of more complex espionage installations.

A second and more specific American concern in Afghanistan is the increasing traffic in narcotics. Because of increased surveillance by neighboring Iranian and Turkish governments, opium crops have been sharply reduced in these countries, leading to a spiraling increase in the price of opium in Afghanistan. As a result, many Afghan farmers have switched from grain and other food crops to opium, which is easy to grow and sell and seems to have an infinite price ceiling.

Opium is grown in three main regions in Afghanistan. One is in the virtually inaccessible northern province of Badakhshan, one in Nangarhar near the Pakistan border, and the third in the Helmand-Arghandab valley in the southwest. The Soviet Union has a large land reclamation project in Nangarhar, and the United States has a similar project in the Helmand Valley. Some Western officials reportedly believe that Afghan farmers have used both Soviet and American technical assistance for land development to produce more opium poppies:

> Officials here say that implicit threats have been made to cut off assistance to Afghanistan unless the Government moves against the opium trade. Experts here are convinced that Afghans in positions of power, perhaps reaching into the royal family, are involved in the lucrative narcotics traffic.[5]

Robert G. Neumann, however, the American Ambassador to Afghanistan in 1970, has denied that "there was a tie between the post-revolutionary role of America in Afghanistan and the Afghan attitude towards drug control."[6] During Ambassador Neumann's six-and-one-half-year tenure in Kabul, several agents of the U.S. Bureau of Narcotics and Dangerous Drugs (now the Drug Enforcement Agency) went to Kabul to work with the Afghan authorities on the problem of narcotics control. American-Afghan cooperation had progressed so far that an international consortium on drug control, under U.N. auspices, was scheduled to have its first meeting at Geneva in July, 1973. The coup intervened and the meeting was postponed, but Ambassador Neumann was assured of the very strong interest shared by the Daud government in narcotics control.[7]

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[2] This article was originally written in January, 1974. Subsequent events have proved this supposition to be partially correct.

[3] "Afghanistan Modernizing Hesitantly," *New York Times*, June 7, 1973, p. 16.

[4] "U.S. and Afghans in a Map Project," *New York Times*, August 23, 1973, p. 6.

[5] "Afghans Use U.S. Aid Project for Opium," *New York Times*, May 26, 1973, p. 8.

[6] Quoted from a personal letter from Ambassador Robert G. Neumann to the author, dated May 19, 1975.

[7] *Ibid.*

In summary, it can be said that Afghanistan lies outside the mainstream of American interests in both Europe and Asia. Its concern, peripheral at best, stems partly from a broad consideration of geopolitical features, such as the proximity of Afghanistan to the Soviet Union and China, and partly from a specific concern about the American domestic problem of the traffic in narcotics.

The Soviet Union. The Soviet interest in Afghanistan may be considered within the context of general Soviet goals in Asia, which in the 1970s has largely featured an increasing assertion of global presence and influence on a par with the U.S. These goals, coupled with the awareness that relations with the U.S. ought not to be excessively disturbed, are guided by: (1) a fear of Chinese expansionist ambition in Asia. This is evinced in the June 1969 Brezhnev proposal at the International Communist Conference about a new collective security system in Asia, implicitly directed primarily against the Chinese; (2) the wish to replace the anti-Soviet SEATO in which Pakistan is nominally a member; and (3) a desire to establish Soviet leadership among developing nations and within the international socialist movement.[8]

More crucially, the Soviet Union has considered Afghanistan a focal part of its foreign policy in Asia because (1) Afghanistan shares a long southern border with the Soviet Union; (2) Afghan tribesmen in the north occasionally indicate an interest in joining with their Soviet kinsmen across the border, and such occurrences, if uncontrolled by Afghan authorities, could stir unrest in Soviet Central Asia; (3) Afghanistan is an important source of natural gas for the Soviet Union; and (4) it acts as a buffer to the pro-American Iran and Pakistan. In view of these circumstances, the Soviet Union has consistently, though subtly, supported Afghanistan in its running dispute with Pakistan over the issue of "Pakhtoonistan" (an attitude which, of course, is in line with the Soviet pro-India policy).

On February 28, 1921, the Soviet Union and Afghanistan signed a Treaty of Friendship. The 50th anniversary of this Treaty was noted with approval in both *Pravda* and *Izvestia.* A commentary by A. Fillipov loudly applauded the "Fruit of friendship and cooperation":

> The historical significance of the Treaty of 1921 lies in the fact that it embodies in itself entirely new principles of international relations – equality, mutual respect, non-interference in internal affairs of each other, good neighborliness and mutually beneficial cooperation.[9]

Another commentary in *Pravda* on June 24 cited Soviet-Afghan friendship as an example to other nations of "good relations." The September, 1971 visit of the King and Queen of Afghanistan to the Soviet Union was greeted with appropriate fanfare in the press.[10] This was followed by a brief unofficial visit by King Mohammad Zahir Shah to Moscow in December, 1971.[11] Prime Minister Abdul Zahir also paid a visit to Moscow in March, 1972, which resulted as before, in friendly joint communiqués and declarations of cooperation and friendship.[12] Shortly afterward, political commentator Victor Maevski reiterated Brezhnev's 1969 proposal for a collective security system in Asia.[13] Adoption of such a system would strengthen Soviet ties with participating Asian nations and accordingly erode Chinese and American influence. It must be kept in mind, however, that these Moscow visits by the Afghan royal family and Prime Minister do not necessarily indicate that Afghanistan was being drawn into the Asian Collective Security Treaty, despite such hints in the Soviet press. While cultivating friendly relations with its northern neighbor, Afghanistan has astutely managed to stay clear of formal commitments to any great power.

In its attempts to build and maintain friendly ties with Afghanistan, the Soviet Union has become that nation's chief trading partner and absorbs all its exports of natural gas, which accounted for 14.6 per cent of the total value of Afghanistan's exports in 1973. The Soviet Union also buys more than 90 per cent of Afghan wool exports and is the biggest buyer of skins, hides, clothing, and cotton.[14]

An agreement was signed in 1972 in which the Soviet Union was to assist in expanding the refining plant for natural gas at Khawaja Gogerdak so that production would increase to 5,000 cubic meters annually. Similarly, with Soviet help a pipeline was built to carry gas from the production centers in northern Afghanistan to the Soviet Union. These are important because goods traded between Afghanistan and West Germany, when not carried by air, go by road via the Soviet Union.[15]

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[8] Shaheen F. Dil, "The Extent and Nature of Soviet Involvement in the Bangladesh Crisis," *Asia Quarterly,* No. 3 (1973), 245.

[9] *Pravda,* February 28, 1971, p. 5.

[10] *Pravda,* September 15, 1971, pp. 1, 4, and September 20, 1971, pp. 1, 3.

[11] *Pravda,* December 14, 1971, p. 1.

[12] *Pravda,* March 22, 1972, pp. 1,4.

[13] *Pravda,* June 21, 1972, p. 4.

[14] "Mobilising Afghan Resources Receives Priority," *Middle East Economic Digest,* June 29, 1973, p. 738.

In summary, the Soviet Union has found that befriending Afghanistan is beneficial in two ways. On the domestic level, friendly relations ensure the uninterrupted flow of natural gas into the Soviet Union and minimize the possibility that Afghan minority groups in the north will cause unrest among their kinsmen across the border. On the international level, Soviet backing of Afghanistan is indirectly an anti-Pakistan move in light of the Afghan-Pakistan conflict over the "Pakhtoonistan" issue. This move in turn strengthens India (a Soviet ally since the signing of the Mutual Friendship Treaty in 1970), and thus may also be interpreted indirectly as a move against China, Pakistan's ally. Finally, close relations between Moscow and Kabul lessen American influence, always an important consideration for Soviet foreign policy makers.

The People's Republic of China. China's relations with Afghanistan are even more fragmentary than are those of the United States or the Soviet Union, even though Afghanistan was one of the earliest nations to recognize the People's Republic. Indeed, the Afghan diplomatic note was sent to Premier Chou En-lai more than two months before the People's Liberation Army reached the Sinkiang-USSR-Afghanistan vicinity. Nevertheless, China ignored these friendly overtures for four years, because of what it termed "large-scale United States construction of military projects in Afghanistan."[16] Afghanistan's continuing neutralist orientation finally assuaged Peking's suspicions, and formal diplomatic relations were established between Kabul and Peking in January, 1955. Trade relations began to be worked out between the two countries in 1956–57. A trade agreement was signed in February, 1957, generally approving the exchange of Afghan raw materials for Chinese light machinery and industrial products.

Some three years after the signing of the China-Afghanistan Treaty of Friendship and Mutual Nonaggression, the China-Afghanistan Friendship Association was organized in Peking in 1963. By December, 1963, Afghanistan reportedly concluded an air link agreement with China, just as a similar agreement was being reached between Pakistan and the Soviet Union. The tenth anniversary of the signing of the Friendship Treaty was marked by a reception at the Chinese embassy in Kabul and received suitable coverage in the Chinese press.[17]

In the early 1970s relations between Afghanistan and China appeared to be developing smoothly. Chinese aid, though limited in volume (totaling only $72 million by the end of 1973), was highly visible and well publicized in the Chinese press. King Zahir Shah reportedly commented that the Bagrani Textile Mill, built with Chinese assistance, was the most successful aid project in Afghanistan.[18] In 1970 Afghanistan reiterated its recognition of the People's Republic of China as the sole legitimate representative of "the great Chinese people," and declared that it firmly opposed the "two Chinas" policy.[19] Trade delegations were exchanged, and there were official and semiofficial exchanges of dignitaries.

At the embassy reception on the occasion of the 53rd anniversary of Afghanistan's independence, Ambassador to Peking Osman Sidky remarked that "Afghanistan as a peace-loving country based her foreign policy on the principles of active neutrality, non-alignment, non-participation in military pacts, independent judgment in world issues, equality with all nations – large and small, and right of self-determination."[20] This statement is quite interesting in that it tactfully touches two Chinese concerns. On the one hand, Sidky reasserts Afghan neutrality, both in terms of East-West détente and in terms of the Sino-Soviet rivalry; on the other hand, he carefully guards the Afghan position on the question of "Pakhtoonistan" by emphasizing the "right of self-determination." These are, of course, the two main issues which could cause friction between China and Afghanistan. In this instance, by emphasizing its neutrality, and downplaying "Pakhtoonistan," Sino-Afghan cordiality, or at least its appearance, is preserved.

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[15] "Afghanistan Faces an Uncertain Future," *Middle East Economic Digest,* May 5, 1972, p. 493.

[16] Shen-yu Dai, "China and Afghanistan," *The China Quarterly,* No. 25 (January–March, 1966), 216.

[17] "Peking Reception Marks Tenth Anniversary of Signing of China-Afghanistan Treaty of Friendship and Mutual Non-Aggression," August 25, 1970, in U.S. Consulate General, Hong Kong, *Survey of China Mainland Press,* August 31–September 4, 1970, pp. 86–87.

[18] "Afghan King Inspects Chinese-Aid Projects," December 9, 1970, in *China Press,* December 14–18, 1970, p. 80.

[19] "Chinese Minister of Foreign Trade Gives Banquet in Honor of Trade Delegation of Afghanistan," December 23, 1970, in *China Press,* December 28–31, 1970, p. 157.

[20] "Afghan Ambassador to China Gives National Day Reception," May 27, 1971, in *China Press,* June 7–11, 1971, p. 81.

In summary, the Chinese interest in Afghanistan is principally a function of Afghanistan's relations with the Soviet Union, Pakistan, India, and to a certain extent the United States. Increasing Soviet friendliness to Afghanistan aroused some negative feelings in Peking as the Sino-Soviet rift widened. Soviet-Afghanistan disaffection with Pakistan and friendliness to India ran counter to China's own preferences. It was to China's benefit to prevent virtual Indian hegemony in the subcontinent by supporting Pakistan, just as the dismemberment of Pakistan – of benefit to both India and Afghanistan – was disadvantageous to China. As the Sino-Indian border conflicts continued, China tilted more and more towards Pakistan, and away from Afghanistan and India.

The Coup

Circumstances. The Republic of Afghanistan was proclaimed on July 17, 1973, while King Zahir Shah took the mud baths at Ischia, Italy. Lieutenant General (Prince) Mohammad Daud Khan, cousin and brother-in-law of the King, led the coup, assisted by a small number of young army officers. Pakistan Press International reported that army and political officers loyal to the King had been executed and that members of the royal family and scores of senior government officials had been arrested.[21] Spokesmen of the coup denied these charges, claiming that there was little or no resistance, that only eight people had been killed, two of them accidentally. General Daud Khan announced that he overthrew the monarchy for domestic rather than international reasons.[22] He accused King Zahir Shah of having fostered a "pseudodemocracy," and promised in its stead to institute a "genuine democracy."[23]

With two notable exceptions, the Royal family was allowed to join the King in Italy. The exceptions were Prince Abdul Wali, who was imprisoned, and his father, Marshall Shah Wali, the King's Uncle, who refused to leave without his son. As Abdul Wali was once considered the strong man of the regime, and both men were personalities to be reckoned with in Afghani politics, this exception is of some significance. Neither of their names have been mentioned in connection with two subsequent counter-coup attempts, which resulted in several highly placed individuals being sentenced to death by a military tribunal. Some American officials feel there is reason to believe that both are still in Kabul, comfortably confined to their respective quarters.

It seems fairly clear that there was no effective protest from the population at large. Although martial law was proclaimed, ordinary life resumed in Kabul within a matter of days after the military takeover. The army appeared to be in full control throughout the country, and there were few signs of armed resistance. This is not altogether surprising in a country where the vast majority of the population is illiterate, government institutions and functions are rudimentary, and life goes on much as it did several centuries ago. For the common person the coup was just a family matter and signified no great social revolution. One old vendor in the market-place at Kabul summed it up with the placid comment, "Ah, yes, we have a new king."[24]

This is not to say that there is no attentive public in Afghanistan. There is, but it is essentially a small one, consisting of interest groups clustering around a few significant individuals, usually associated both with the army and with the former royal family. Ethnic identifications are strong, but resulting cleavages do not always reflect upon the situation in Kabul. Pathans constitute a clear majority, but there are sizable minorities, such as the Tadzhiks, Uzbeks, and Hazaras. These are traditionally suppressed groups with little interest in or allegiance to the central government. If they are interested in mobilizing at all, it would be directed toward affiliations with their kinsmen in the north, i.e., in the Soviet Union. Thus, any elite in Kabul must mainly secure the support of the Pathans, which in practice means that of Pathan tribal leaders. Viewed from this perspective, Daud Khan's "Pakhtoonistan" policy is eminently reasonable for a regime wishing to remain popular.

Some Afghans have voiced the suspicion that two counter-coup attempts were the result of instigation by CIA-Pakistani agents, Moslem extremists, and pro-Chinese elements. The ease with which these attempts were suppressed indicates both the probable lack of foundation for these fears, and the essential strength of the new regime. Either these accusations were indicative of how Afghans viewed the interaction of great-power, regional, and domestic conflict, or they represented an attempt by the new regime to discredit its opponents.

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[21] "Executions in Afghanistan are Reported," *New York Times,* July 19, 1973, p. 8.

[22] "Afghan Pledges No Subservience," *New York Times,* July 26, 1973, p. 9.

[23] "Executions," p. 8.

[24] Bernard Weinraub, "Two Weeks After Coup, Afghan Government Remains Paralyzed and Public Seems Mostly Apathetic," *New York Times,* August 1, 1973, p. 13.

The new dictatorship – even the leaders do not claim that it is anything more at present – declared that the coup "not only enjoyed the complete cooperation of all the branches of the army but also the support of all the people, particularly the intellectuals and youth . . . We undertook the revolution solely to fulfill the wishes and aspirations of the people." Yet under questioning by the press, Daud Khan admitted that the people may have to forego some of the basic freedoms temporarily since the future constitution was not prepared.[25] He was also emphatic about abandoning Zahir Shah's policy of rapprochement with Pakistan. He has openly revived the old Pathan ambition of absorbing the Pushtu-speaking northwestern section of Pakistan – "As regards our relations with Pakistan, we have a political dispute with that country. . ."[26] – a policy which is virtually a foregone conclusion, based as it appears to be on the premise of building a meaningful national identity for Afghanistan as a Pathan State.

Resurgence of border conflict between Afghanistan and Pakistan could have serious implications for Iran as well. Iranian relations with Afghanistan have been anything but simple in the past several years. In 1973 a draft Treaty on the Use of the Helmand River Waters had been concluded between the Durrani monarchy and Iran, but not ratified. The Daud regime alleged that such a treaty was treasonous on the part of the previous government. In addition, Iran is concerned that separatist movements along the Afghan-Pakistani border could lead to similar disturbances in Baluchistan, which falls on the Iranian-Pakistani border. As a result of Daud's statement and Iran's concern, relations between the two countries temporarily cooled. At one point the Shah of Iran indicated his determination to use his American-equipped forces if necessary to prevent any further dismemberment of Pakistan.[27] More recently, however, the Shah has made a major effort to develop friendly relations with Afghanistan, offering perhaps as much as $2 billion in assistance. The Iranian government appears to have decided that the stability and independence from foreign control of the Daud regime was worth paying for.[28]

Possible Involvement of the Great Powers. One question that arises whenever a coup d'état occurs in a small state is whether any of the three great powers was involved. Since great-power interaction in local crises is complex and difficult to differentiate, it may be most efficient to consider first whether it would have been in the *interests* of an outside nation to intervene. If so, was there an opportunity for intervention or influence? And finally, did such participation actually take place?

Daud Khan has vigorously denied any Soviet involvement in the coup: "Anyone who says this coup d'état was helped by a foreign country – I will deny that emphatically and it is a great mistake. The people who have taken the coup chose their future with no help from a foreign country."[29] Such a forceful denial was necessary because virtually the entire officer corps which participated in the coup was Soviet-trained and -equipped. Furthermore, whereas Daud Khan's statements generally extol neutrality, democracy, and independence, they are specifically directed toward weakening and breaking up Pakistan – a policy not displeasing to the Soviet Union in view of growing Indo-Soviet friendship. It should also be remembered that during Daud Khan's premiership, the Soviet Union became Kabul's chief source of military and economic aid, after the Eisenhower administration had first declined the same opportunity. The portent of this fact is lessened, of course, as the substantial American aid program to Afghanistan also began at this time. Yet the Soviet Union and India have consistently supported Daud Khan's position on the issue of "Pakhtoonistan" – one of the chief reasons Zahir Shah removed him from power in 1963. India and the Soviet Union were also the first two nations to recognize the new regime – both extending formal diplomatic recognition on July 19, just two days after the coup.[30]

Although there is no evidence to indicate that any nation had prior knowledge or complicity in the coup, Daud Khan's policies probably made the takeover a diplomatic victory for the Soviet Union. Yet if the new regime lives up to its promises of independence, neutrality, and democracy, the Soviet "diplomatic victory" may prove to be just that – a psychological success.

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[25] "Afghan Pledges No Subservience," p. 9.

[26] "New Afghan Chief Revives Dispute," *New York Times,* August 2, 1973, p. 6.

[27] C. L. Sulzberger, "New Fuse Burning in Asia," *New York Times,* July 21, 1973, p. 27.

[28] "Promise of Substantial Aid from Iran Buoys Military Regime in Afghanistan," *New York Times,* February 3, 1975, p. 4.

[29] "Afghan Pledges No Subservience," p. 9.

[30] "Kabul After Coup Quiet, Patroled," *New York Times,* July 20, 1973, p. 5.

Some preliminary conclusions may be drawn from this schematic outline of the nature and circumstances of the coup. By strengthening Soviet influence in Kabul, it may be said to have worked significantly against the interests of China and the United States. Insofar as Daud Khan resumed his anti-Pakistani stance, the coup appears to have operated significantly against the interests of Pakistan. Nor does the coup appear to have benefitted Iran, both because the new regime refused to ratify the Helmand River Water Draft Treaty and because Iran could become entangled in the event of conflict in Baluchistan. Furthermore, the coup has resulted in marginal benefits for India and the Soviet Union, which seems to indicate that it was not engineered or encouraged by China, the United States, Pakistan, or Iran. The two states which would have benefited from it are India and the Soviet Union. India had neither the resources nor the influence to engineer the coup. This leaves only the Soviet Union. Since the Soviet Union is an ally of India, one of the principal beneficiaries of Daud Khan's "Pakhtoonistan" policy, it is not unreasonable to speculate that any Soviet complicity may well have had Indian proclivities in mind. Having been for more than two decades Afghanistan's chief aid donor and trade partner, the Soviet Union was likely to have had the means, the opportunity and sufficient motives to take a hand in the coup.

Whether or not the Soviet Union was actually involved, however, as is occasionally hinted in the Western press,[31] is another matter altogether. To determine this we must consider yet another factor – namely, the constraints upon the Soviet Union *not* to act under the given circumstances. First, Afghanistan under Zahir Shah was sufficiently friendly to the Soviet Union so that it was not necessary for the Soviets to act out of reasons of state. Second, neither China nor the United States had sufficient influence in Kabul to be able to reduce Soviet leverage. Third, the only significant shift in Afghanistan's foreign policy after the coup has been Daud Khan's intransigent attitude toward Pakistan. This affects the Soviet Union only peripherally and is not sufficient justification to warrant any significant Soviet outlay in planning, directing, or aiding the coup. Fourth, there is no concrete evidence that the Soviet Union even had definite prior knowledge of the coup, let alone a hand in bringing it about. Thus, if one keeps these considerations in mind, it seems advisable not to speculate rashly on the possibility of Soviet involvement in the overthrow of the Durrani monarchy.

Indeed, the complexities inherent in any revolutionary situation preclude the possibility of a simplistic, unicameral view of the underlying factors which led to the coup. Thus the dynamics of great-power influence were felt only in complex interaction with many facets of the situation. No doubt there was factional activity related to dissatisfaction with the monarchy's handling of important social and economic issues. Afghanistan's development was slow, and democratic institutions fragmentary. Daud Khan, among others, is known to have felt that the royal government took inadequate action to combat the effects of a two-year drought which exacerbated already-low living standards. Furthermore, the Durrani monarchy failed to take sufficient account of ethnic cleavages and the resultant dissipation of national identity. Daud Khan's commitment to Pathan nationalism made a potentially formidable popular force. Another factor which cannot be ignored is Daud Khan's personal ambition and a possible residue of bitterness over his earlier dismissal from the premiership. This multiplicity of compelling domestic factors and the lack of convincing contrary evidence suggest that little, if any, great-power influence was brought to bear upon the coup.

Impact of the Coup

The reaction of each of the three great powers to the coup was quite different.

American reaction to the coup was reflected in extensive coverage in *The New York Times.* While such reportage does not necessarily reflect government policies, comments by some newspaper columnists often portray a fairly accurate image of American concerns and interests. C. L. Sulzberger's summary of the American reaction is representative:

> This week's coup d'etat in Afghanistan might well change the ultimate power balance in South and South-west Asia. The Soviet position is certainly convenienced even if there is no known evidence that Moscow played any role in the ouster of King Mohammad [sic] Zahir Shah and the creation of the first Afghan

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[31] For example, Bernard Weinraub, "Two Weeks After Coup," p. 13.

> republic by his cousin, Prince Mohammad Daud.[32]

In the midst of a cautious wait-and-see attitude there were openly expressed fears that the coup had been engineered by the Soviet Union, that the new regime would actively work for the further fragmentation of Pakistan, and consequently, that Iran might be pushed into a conflict. What was not expressed, but may prove equally significant, was the continuing American fear that each absolute increase in Soviet influence implied a reduction of American power. The vision of Soviet paramountcy in a part of the world which the United States is not willing to acknowledge as within the Soviet sphere of influence is not comforting to American foreign policy makers.

The Soviet reaction to the coup was also cautious. Initially it rated only a medium-sized headline on page 5 of *Pravda*. The article that followed reported that Afghanistan had been proclaimed a republic, that the army was patrolling the streets of Kabul, and that everything was under control. Daud Khan's statements on the new regime's policy of continuing neutrality and democracy were also published.[33] The following several days saw equally muted reports on the situation in Kabul. The commentary was generally favorable toward the new regime, but not obstrusively so. No concern was expressed over the fate of the royal family or the future of the fledgling republic.

There is every reason to believe that the Soviet Union was pleased with the turn of events. Press coverage on Afghanistan remained on the fifth page and concentrated on positive aspects of the coup, such as the move toward democracy and away from monarchy. Reports from Pakistan Press International about the alleged assassinations of several high officials who refused to participate in the coup were not published. Such a low-keyed reaction (which is nonetheless indicative of satisfaction) can be interpreted in three ways. It might indicate that the coup was indeed a surprise, and that Soviet reporters were uncertain how they should treat the subject. Or it could mean that having foreknowledge of the coup, the Soviet media were instructed to underplay the issue. Finally, it could mean that the coup was simply a pleasant surprise and that Soviet reporters were instructed not to react in any way which could suggest that the Soviet Union had been guilty of complicity.

Unlike the Americans and the Soviets, the Chinese chose to ignore the coup altogether. Thus not a single reference to the overthrow of the monarchy or the establishment of a Republic under Daud Khan appears in the Chinese press. Yet relations between the two countries were not terminated, nor was there any indication of permanent withdrawal of ambassadors, abrogation of trade agreements, or dissolution of the Chinese-Afghan Friendship Association.[34] The first reference to Afghanistan in the Chinese press after the coup came on October 2, 1973, more than two and one-half months after the event. The report had an air of studied tepidity:

> Chinese ambassador to Afghanistan Kan Yeh-tao gave a reception at the Chinese embassy here yesterday evening to celebrate the 24th anniversary of the founding of the People's Republic of China...
>
> At the reception, an official of the protocol department of the foreign ministry handed to the Chinese ambassador a message of greetings from President Mohammad Daoud [sic] on the Chinese national day.
>
> The reception proceeded in a friendly atmosphere.[35]

The report was accompanied by the publication of two congratulatory letters from Daud Khan – one to Premier Chou En-lai, and the other to acting Chairman Tung Pi-wu. Both were friendly and stressed Afghanistan's "most sincere wishes for the further consolidation of the cordial neighbourly relations that so happily exist between our two countries and for the happiness and prosperity of the People's Republic of China."[36]

Apparently China ignored the coup, declining to comment on the situation, and simply carried on state relations as before. It remains to be seen whether the new regime's policy of verbally supporting separatist action in Pakistan will cause China to take some action on behalf of its ally. As this has not happened in the intervening two years, it does not seem probable. Chinese influence in Kabul is minimal at any rate, and so it is unlikely that the PRC could do very much to change Kabul's views on the issue of "Pakhtoonistan."

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[32] Sulzberger, "New Fuse Burning in Asia," p. 27.

[33] *Pravda*, July 18, 1973, p. 5.

[34] It was reported in Peking on October 10, 1973 ("Afghan Ambassador Leaves Post," *China Press*, October 23–26, 1973, p. 71), that "Mohamed Osman Sidky, Ambassador extraordinary and plenipotentiary of the Republic of Afghanistan to China, left his post for home today." No further explanation was offered. As a new ambassador arrived in due course, it appears that this was not a formal act of diplomatic portent, but a routine change of representatives.

[35] "Chinese Ambassador Holds National Day Reception in Kabul," October 2, 1973, in *China Press*, October 15–19, 1973, p. 115.

[36] "Head of State of Afghanistan M. Daud Sends Message to Acting Chairman Tung Pi-wu on Chinese National Day," October 3, 1973, in *China Press*, October 15–19, 1973, p. 116.

Conclusions

Projections about the future of Afghanistan in relation to the three great powers must remain speculative at best. The policies of the new regime will prove advantageous to both India and the Soviet Union. No concrete evidence suggests that the Soviet Union played a role, however small, in bringing about the coup, although it did have the opportunity, influence, and interests to do so. It is also clear, however, that the Soviet Union did not need to instigate the coup since its interests were adequately considered by the Durrani monarchy.

The Soviet gain in Afghanistan probably occurred at the expense of both American and Chinese interests. What little influence the United States and China may have had in Kabul has been weakened further as the interests of the new regime move closer toward Soviet goals. This trend affects both China and the United States in terms of global politics, since the strengthening of the Soviet sphere of influence inevitably reduces Chinese and American strength. The United States is also affected on the more specific issue of limiting the narcotics traffic, because the new regime has not yet taken up the problem in cooperation with the proposed international Consortium of Drug Control, as agreed to by the Durrani monarchy.

Finally, it will be increasingly difficult for Pakistan to avoid conflict with Daud Khan's regime, which is sensitive on the issue of "Pakhtoonistan." Pakistan is determined to go to war if necessary to prevent secession movements in the Northwest Frontier Province and Baluchistan. Indeed, the issue may well erupt without Afghan intervention. Iran has occasionally spoken out strongly in support of Pakistan, but has recently stepped up aid to Afghanistan, and it now appears to be busy cultivating cordial relations with both countries. Iran's reaction to a possible Afghan-Pakistani confrontation is unpredictable. Whether Afghanistan will nonetheless feel sufficiently confident of Soviet and Indian support to trigger such a conflict remains uncertain.

Partisan Dealignment in the Postwar South*

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Strong winds of political change have swept through the American South during the last two decades.[1] The parameters of this change are clear: a sharp decrease in Democratic presidential voting; gradual increases in Republican fortunes in important contests below the presidential level; the mobilization of blacks into politics; and the emergence of a new breed of southern politicians. At the same time, some of the region's traditional political habits seem to have endured stubbornly, particularly in state and local voting. In essence, the politics of the post-World War II South combines elements of both continuity with the past and change. The purpose of this study is to increase our understanding of this complex pattern of postwar southern politics by examining the partisanship of important groups in the southern electorate between 1952 and 1972.[2]

The most conspicuous political change in the postwar South lies in the pattern of presidential voting. The once solidly Democratic South became highly competitive at the presidential level as early as 1952 and has been the region least supportive of Democratic presidential candidates in recent years.[3] For all the appeal of Republican candidates to southerners in 1952, 1956, and 1960, the region still supported Democratic candidates more than did the rest of the nation. The sharp decreases in Democratic presidential voting since then would seem, at first glance, to reflect a more traditional force in southern politics – the racial question. As the national Democratic party emerged as the champion of black civil rights, the southern electorate seems to have deserted it.

Republican fortunes have improved also in the most salient subpresidential contests—races for Senator, Congressman, and Governor. Increased success here has lagged about a decade behind that at the presidential level,

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[1] For useful examinations of various aspects of these changes, see Bruce A. Campbell, "Patterns of Change in the Partisan Loyalties of Native Southerners: 1952–1972," paper presented at the Annual Meeting of the Midwest Political Science Association, Chicago, May 1–3, 1975; William C. Havard, ed., *The Changing Politics of the South* (Baton Rouge: Louisiana State University Press, 1972); Everett Carll Ladd, Jr., with Charles D. Hadley, *Transformations of the American Party System* (New York: W. W. Norton and Company, 1975), pp. 129–177; E. M. Schreiber, "'Where the Ducks Are': Southern Strategy versus Fourth Party," *Public Opinion Quarterly*, 35 (Summer, 1971), 157–167; Donald S. Strong, "Further Reflections on Southern Politics," *The Journal of Politics*, 33 (May, 1971), 239–256; and Raymond E. Wolfinger and Robert S. Arseneau, "Partisan Change in the South," paper presented at the Annual Meeting of the American Political Science Association, Chicago, August 29–September 2, 1974.

[2] The data used in this study come from national surveys conducted by the Survey Research Center and, more recently, the Center for Political Studies (hereafter SRC/CPS) of the University of Michigan in each presidential election year since 1952. For purposes of this study, the South is defined as the eleven former Confederate states *minus* Tennessee. (While it would have been highly desirable to add Tennessee to the analysis sample, the original sample design's specification of that state as a "border state" does not permit it. Interviews conducted in Tennessee were meant to be representative of border states in general and not the state itself. Thus, the addition of Tennessee to the other states of the Confederacy does not yield an unbiased sample of those eleven states.) Sample estimates are very similar to the census statistics, where available, on the relative sizes of the various groups analyzed in this study. The sample does tend, however, to exaggerate somewhat the number of southerners who moved from outside the South.

[3] Democratic presidential candidates won the following percentages of the *two-party vote* in the ten southern states in the sample: 66% in 1948, 52% in 1952, 48% in 1956, 51% in 1960, 51% in 1964, 48% in 1968, and 29% in 1972. In 1948 and 1968, when third-party totals were substantial, Democratic candidates polled 50% and 31%, respectively, of the total vote.

with the first noticeable Republican inroads appearing in the 1960s. Since then, there has been a steady growth in the number of Republicans holding these offices until 1974, when the GOP suffered a slight setback. In 1973, for the first time since Reconstruction, every southern state had a Republican occupying at least one of these three important offices. Nonetheless, the Democratic party retains a heavy majority of these offices throughout the region, and it may be some time before the GOP becomes fully competitive.

A third political change has been just as important as increased political competition for southern politics. Over the last two decades, the legal pillars of the dual society have been demolished. In the areas of social segregation, the actions of the Supreme Court in Brown and subsequent cases have been the hammer of destruction. Changes in the social sphere have been paralleled by an equally dramatic, and perhaps more important in the long run, mobilization of blacks into politics. Initially blacks entered the political process through voting as barriers erected in the previous century were stripped away. Black voting registration rose from a scant five per cent of the adult black population in 1940 to a robust 59 per cent by 1971 – only about six per cent less than the comparable figure for whites.[4] Following quickly upon the heels of black enfranchisement has been the recruitment of blacks into the ranks of political office holders. The number of black elected officials jumped from only 72 as recently as 1965 to 1398 in 1974,[5] and the region gained in 1973 its first black United States Representatives since 1901.

A fourth recognizable change in southern politics has been the emergence of a new breed of politicians, styled and often self-styled as racial moderates. The most successful member of this new breed, Jimmy Carter, now occupies the White House – to no small degree because of his racial moderation. Not only is the cry of white supremacy used less frequently by modern southern politicians, but it appears to be far less frequently heeded by southern voters. In large part, this change in southern politicians may be traced to the surge in black participation. Open appeals to racial prejudices, relatively costless when the target group could not vote, must be weighed now against the very real possibility that they will repel more voters than they attract. The growth of political competition in general elections also has contributed to this change. Few southern politicians now seem willing to repudiate openly the support of black voters, who have come to hold the balance of power in many contests. Strom Thurmond's hiring of a black assistant and George Wallace's appeals to black audiences for support and appointment of blacks to high positions in his administration signify that the badge of "segregationist" is no longer coveted by even the most ardent erstwhile spokesmen for the dual society.

Change has been a conspicuous feature of the southern political landscape during the past several decades. Yet strong elements of continuity with the past lie just below the surface, particularly in subpresidential voting, and they have dampened the amount and rate of change. The South has continued to send a disproportionate share of Democrats to Washington: in the 95th Congress, three-quarters of the Senators and of the Representatives from these ten states are Democrats – a slight gain from the preceding year. The party has retained also a major share of governorships (8 of 10 in 1977). Finally, the Democrats continue to monopolize southern state legislatures.[6] All in all, subpresidential politics remains heavily Democratic, placing severe limitations on the potential for a dynamic Republican party in the region.

Probing Below the Surface: Patterns of Partisanship in the South

On the surface, elements of stability and change appear side by side in contemporary southern politics. These elements combine to produce the puzzling electoral response of the region – the coexistence of weak support for Democratic presidential candidates and strong support for most other Democrats. This variable electoral response cannot be understood through an analysis of voting patterns themselves. Rather, one must look below the surface at the changing partisan loyalties of southerners and at the changing role these loyalties have played in voting behavior. Enduring partisan loyalties have been observed to have special significance in American voting behavior.[7] One would be well advised to begin with these loyalties in seeking to explain postwar electoral politics in the South.

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[4] Both registration figures apply to the eleven states of the old Confederacy. See David Campbell and Joe R. Feagin, "Black Politics in the South: A Descriptive Analysis," *The Journal of Politics*, 37 (February, 1975), 133–134.

[5] These figures on black elected officials cover the eleven-state South and come from *The New York Times*, March 4, 1974, and March 6, 1975.

[6] The proportions of this monopoly are staggering. Democrats occupied 88 per cent of the seats in the lower houses and 92 per cent of the seats in the upper houses in the ten-state South in 1975 – both totals slightly above those in the previous year. Only in Florida (72%) and Virginia (66%) did the Democratic percentage drop below 86 per cent in lower houses. Only in Florida (68%) did it drop below 88 per cent in the upper houses. The Democratic advantage has grown even larger with the 1976 election.

The partisan affiliations of southerners are portrayed in Table 1 for each presidential election year since 1952.[8] At the beginning of this period, more than two-thirds of all southerners regarded themselves as Democrats, and there were fewer Republican identifiers than either Independents or Others. The distribution of party identifications twenty years later had altered considerably. The most striking changes in the twenty-year interval were the increase in Independents and the decrease in Democrats. Nonetheless, these partisan changes were not as pronounced as one might expect from presidential voting in the region, nor as slight as one might expect from the region's subpresidential voting.

The distributions of party identification in the intervening years tell a more complicated tale of partisan change in the South. The increase in Independents and decrease in Others (most of them apolitical) have been steady throughout. Discontinuous changes in the two partisan categories, however, suggest two separate phases. Between 1952 and 1960, the Republican party experienced moderate growth while the Other category declined. Since then support for both parties, and especially the Democrats, has declined and the percentage of Independents has increased dramatically.

These changes are partially reflected in the pattern of stability and change in southern voting. The decline of Democratic identifiers, halted temporarily only in 1964, should have produced a gradually weakening but still dominant majority party during this time period. The increase of Independents should have brought substantial volatility to the region's electoral response, since Independents naturally exhibit little partisan regularity in their voting. This volatility should have been greatest in contests for the more salient offices, especially the presidency. Taken together, then, these partisan changes should have lessened the region's traditionally solid support for Democratic candidates, particularly at the top of the ticket.

Alterations in the distribution of party identification cannot explain adequately the region's support for Republican presidential candidates. To handle this phenomenon, one must turn to voting defections by Democratic identifiers. Although party identification remains a good predictor of the presidential vote throughout the period, defection rates among Democrats were high – ranging from 19 per cent (1964) to 39 per cent (1968) through 1968 and then rising to 54 per cent in 1972. By contrast, Democratic defections in congressional voting exceeded 7 per cent only once in the two decades – 15 per cent in 1968. While presidential-level defections require extensive study in their own right, the fact remains that such defections are a regular feature of American politics – and can occur without disturbing a more enduring distribution of partisanship.[9]

Table 1. Party Identifications of Southerners, 1952–1972 (Percentages)

	Democrats	Independents	Republicans	Others	Total *N*
1952	68.3	12.4	9.0	10.3	(388)
1956	62.2	14.0	15.2	8.6	(421)
1960	56.6	16.8	20.7	5.9	(489)
1964	65.5	18.7	12.6	3.2	(342)
1968	57.5	29.6	11.2	1.7	(358)
1972	49.8	32.7	15.1	2.4	(676)

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[7] See Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley and Sons, 1960), pp. 120–145.

[8] For purposes of this analysis, the original SRC/CPS party identification measure was collapsed into four categories. Democratic and Republican identifiers include both weak and strong identifiers from the initial coding, but not the Independent leaners. Independents are all those who responded "Independent" to the first party identification question and include pure Independents as well as leaners. The Other category contains minor party identifiers and those classified as apolitical. Nonresponses have been eliminated as missing data.

[9] The classic illustration of defection without partisan change comes from the two presidential elections in which Eisenhower was elected President while the overwhelmingly Democratic loyalties of the electorate were maintained. See Angus Campbell, "A Classification of the Presidential Elections," in *Elections and the Political Order*, ed. Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes (New York: John Wiley and Sons, 1966), pp. 69–74.

This distribution should be the focus in explaining the more general, and more lasting, patterns of electoral change.

The mixed pattern of continuity and change in southern politics can be linked to the changes in party identification of the southern electorate. Furthermore, the most prominent explanations of postwar southern politics are inadequate because they ignore these *partisan* changes. Those who have prophesied "an emerging Republican majority" in the region cannot account for the paucity of Republican identifiers there as recently as 1972, not to mention Republican failures in subpresidential electoral contests.[10] Those who had seen Republican growth as a "spinoff" of modernization must admit bewilderment also at the small number of Republican identifiers, particularly since 1960, as the pace of modernization has quickened.[11] Neither of these theories of impending realignment is suported by the data because realignment requires pronounced growth in Republican affiliations. On the other hand, those who have attempted to minimize the extent of partisan change in the South must now revise their notions to take into account the decline of Democrats and the increase in Independents.[12] The data presented here do not square with a theory of stability either.

Instead, the data on party affiliations presented here suggest that the southern electorate is engaged in a secular *dealignment*. This term describes a process in which traditional party coalitions dissolve without new party coalitions being formed to take their place.[13] That a dealignment of the southern electorate has occurred seems obvious. *Why* it has taken place is more difficult to discern. The South has experienced several different kinds of change during the last two decades which may have affected its politics. Dealignment may be the product of population migrations which have increased "Yankee" influence on the region's electoral politics. It may be the result of the mobilization of blacks into politics. Finally, the roots of dealignment may be located within the traditional white electorate: adult southerners may have converted to independence, new Independent-oriented generations may have entered the electorate, or both phenomena may have occurred. Let us now turn to an examination of each of these hypotheses concerning the roots of partisan dealignment in the South.

Three Migrations and Partisan Dealignment in the South

The postwar South has experienced three important population "migrations." Two have involved interregion population exchanges – the movement of people (mostly northerners) into the region and the departure of native southerners for the North and West. The third migration is not a geographical movement but rather the continuing mobilization of blacks into the mainstream of southern political life. Each of these migrations seems likely to have had a profound effect on the politics of the region and, as a result, warrants examination in its own right. Table 2 displays distributions of partisanship for each of the migrant groups during the 1952 to 1972 time period.[14]

Immigration. The immigrants seem to have brought with them partisan loyalties characteristic of their mostly northern region of origin and their higher socioeconomic status. These loyalties were shown elsewhere to have been affected little by the pervasive Democratic

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[10] The strongest proponent of this thesis is Kevin P. Phillips, *The Emerging Republican Majority* (New Rochelle, N.Y.: Arlington House, 1969), pp. 187–209. See also Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton and Company, 1970), pp. 135–174; and E. M. Schreiber, "'Where the Ducks Are'. . ."

[11] See Donald S. Strong, *Urban Republicanism in the South* (University, Alabama: University of Alabama Bureau of Public Administration, 1960).

[12] See Philip E. Converse, "On the Possibility of a Major Political Realignment in the South," in *Elections and the Political Order*, pp. 212–242. For a more recent assessment, see Converse, "Change in the American Electorate," in *The Human Meaning of Social Change*, ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), pp. 307–317.

[13] See Ronald Inglehart and Avram Hochstein, "Alignment and Dealignment in France and the United States," *Comparative Political Studies*, 5 (October, 1972), 343–372.

[14] Interregion migrants are defined as individuals who lived in a different region or nation (since some foreign-born, mostly Cubans and Mexicans, are involved) from the one in which they had grown up. Because there is no other way to measure migrations from the SRC/CPS survey, each year's group of migrants contains those who had migrated through the previous presidential year *plus* those who had migrated since. There is no way to determine the actual date when the migration took place. Black migrants have been counted as migrants and excluded from the "native" black category. Thus, the three subgroups contained in Table 2 plus the subgroup of native whites to be analyzed later are mutually exclusive; they are also exhaustive of the entire southern subsample except where region of origin was not ascertained or where race was "other."

Table 2. Party Identifications of "Migrants" in the South, 1952–1972 (Percentages)

	Democrats	Independents	Republicans	Others	Total *N*
Immigrants					
1952	59.3	18.5	22.2	0.0	(27)
1956	43.1	21.5	32.3	3.1	(65)
1960	28.3	25.0	42.4	4.3	(92)
1964	43.1	21.5	35.4	0.0	(65)
1968	42.1	31.6	22.4	3.9	(76)
1972	31.7	35.8	31.7	0.8	(123)
Emigrants					
1952	63.2	15.8	18.4	2.6	(76)
1956	56.5	18.8	18.8	5.8	(69)
1960	52.1	16.9	31.0	0.0	(71)
1964	68.5	16.3	14.1	1.1	(92)
1968	69.0	17.9	10.7	2.4	(84)
1972	56.2	21.3	21.3	1.1	(89)
Native Blacks					
1952	50.0	10.9	8.7	30.4	(92)
1956	45.5	5.2	16.9	32.5	(77)
1960	40.5	13.9	20.3	25.3	(79)
1964	76.0	12.0	4.0	8.0	(75)
1968	96.6	1.7	0.0	1.7	(58)
1972	64.0	27.2	6.6	2.2	(136)

loyalties of their region of destination.[15] Immigration has weakened the Democratic party while providing a solid base for the development of Republicanism. Since 1960, from one-third to one-half of all southern Republicans have grown up outside the region, giving the southern GOP a distinctly "Yankee" flavor. At the same time, the Democratic party has continually enrolled less than its proportionate share of immigrants.

While it is difficult to assess the overall partisan consequences of the immigration because the partisanship of the immigrants varies considerably from year to year, one can discern faint traces of two patterns. During the 1950s and early 1960s, as the proportion of immigrants in the electorate increased, the Republican party grew as well, drawing a considerable portion of its strength from this new source. Since 1968 the percentage of immigrants has not grown, and the contribution of immigrants to the partisan distribution in the region has stabilized. In short, the major partisan impact of immigration seems to have been registered in the 1960s, and additional immigration has played only a minor role in partisan change since then.[16]

Emigration. During the last two decades, many southerners have left the region, usually in search of jobs and a better way of life. Much of this emigration has flowed from rural areas, and it has contained healthy admixtures of blacks and whites. While the emigrants have not been numerous enough to affect significantly the distribution of partisanship in the North, their departure has had a pronounced effect on partisanship in the South. The emigrants have been about as Democratic as those they left behind. In fact, in recent years, they have been a bit more likely to espouse Democratic loyalties. The effect of their departure has been to dilute Democratic party strength in the South, particularly in the mid–1960s when the emigrants showed unusually strong Democratic loyalties.

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[15] See Anne Permaloff, "Partisan Change in the American Electorate," paper presented at the Annual Meeting of the Southwestern Political Science Association, Dallas, March 28–30, 1974, p. 20.

[16] It should be reiterated that the SRC/CPS sample seems to overestimate the number of southerners who are immigrants. The United States Census for 1960 and 1970 shows fewer immigrants. Part of this difference, though, surely lies in the different definitions of immigrants in the sample and the Census. In the sample, immigrants are southerners who had *grown up* outside of the *ten-state* region. In the census, immigrants had to be defined as southerners *born* outside of the *Census South* (15 states plus the District of Columbia) because no other data were readily available.

Black Mobilization. The third "migration" is the mobilization of increasing numbers of blacks into the political life of their region. This mobilization has been far from smooth. Initially it faced the hostility of a unified white community working through white political authorities. In more recent years, it has been slowed by black apathy – conditioned by centuries of exclusion from politics – as well as continued fear and discrimination.[17] The partisan change caused by this mobilization is reflected most clearly in the steady decline in the percentage of blacks classified as "Others." The decline has been dramatic – from a high of almost one-third of southern blacks in 1956 to a total as low as that for whites in 1972 – and represents movement almost exclusively from an "apolitical" orientation. This politicization of blacks, though, understates the overall impact of black mobilization. Because few blacks were permitted to exercise the franchise in the South until the 1960s, only in recent years have their partisan orientations been translated into votes. Blacks have also played an important role in the changing distributions of partisanship in the region. From 1952 through 1960, they exhibited an increase in Republican loyalties and a parallel decrease in Democratic loyalties. Since then, black Republicans have all but vanished, and blacks have identified heavily with the Democratic party.

It seems likely that these variations in party support are keyed to the parties' positions on racial issues. During the 'fifties, neither party championed the black cause. Yet the behavior of the Eisenhower Administration, however reluctant, in sending federal troops to Little Rock to facilitate school desegregation in 1958 was an important symbolic gesture and may well have attracted numerous blacks to the GOP. What governmental action there was at the time on behalf of black civil rights emanated from a Republican-led administration and a Supreme Court presided over by a prominent Republican. With the Democratic championing of the civil rights cause in the 'sixties, black perceptions of which party was more favorable to them changed.[18] One result seems to have been the conversion of blacks to Democratic loyalties, although a cause-effect relationship is (as always) impossible to prove from cross-sectional surveys.

Given black attraction to the Democrats because of racial issues, the substantial decline in black Democrats and growth in black Independents between 1968 and 1972 is somewhat puzzling. This change may be an artifact of the unusually high number of new voters who entered the electorate in 1972 as a result of the enfranchisement of 18-to-21-year-olds. Or perhaps differences between the parties on civil rights matters were less perceptible that year. Even with the patently obvious "southern strategy" of the Nixon Administration, the greatest gains yet in integration of southern schools were registered during the Nixon years. At the same time, the national Democratic party was treading lightly on civil rights matters in order to minimize that issue's divisive impact on the old Democratic coalition. McGovern's weak identification with the battle for black rights may also have dimmed black recognition of party differences on the issue in 1972. Alternatively, the increase in black Independents may have marked the dawning of a new era of black political strategy. Many blacks may now feel that their influence will be maximized if they remain independent of the two parties and retain the prerogative of bargaining for policy concessions from each. Only future distributions of black partisanship can show whether 1972 represented an anomaly or the launching point for a new pattern of black political behavior.

The Impact of the Three Migrations. The differences in partisanship between the entire southern electorate (Table 1) and each group of migrants suggest that each of these population processes has affected partisanship in the South to some degree. The data are far too complex, however, to permit any immediate conclusions about the magnitudes of these effects. Not only will the effects vary depending upon the years chosen for comparison (especially for blacks), but the unequal sizes of the groups must be taken into account as well.

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[17] See Lester M. Salamon and Stephen Van Evera, "Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation," *American Political Science Review*, 67 (December, 1973), 1288–1306.

[18] In 1960, most blacks had no opinion on school integration. Of those who did, about half perceived no party differences on the issue. Of those who saw party differences, four times as many believed the Republicans to be less anti-integration than the Democrats. By 1964, these perceptions had undergone dramatic change. Three-fifths of *all blacks* perceived the Democratic party as more favorable to school integration. The remainder either perceived no difference or reported no opinion. Similar figures appear in 1968, when three-quarters of all blacks perceived the Democrats as more favorable to school integration. The question on perceived party differences was not asked in 1972 and was not asked in comparable form in 1956.

In order to present a clearer picture of the effect of the migrations, the 1972 electorate has been reconstructed under the assumption that each migration *had not taken place.* An index of effect for each migration is defined as the signed percentage difference between the 1972 electorate and the reconstructed electorate. It is the difference between "what is" and "what would have been without migration." Negative differences indicate that the migration reduced the relative size of a partisan category. Positive differences indicate that the migration increased its size. When the absolute index scores are averaged across the partisan categories, the result is an index score which measures the total impact of the particular migration. The cumulative effect of the three migrations can be estimated also by reconstructing the 1972 electorate under the assumption that none of the migrations had taken place and by calculating analogous index scores. Table 3 displays the results of these operations.[19]

These results lead to several conclusions concerning the impact of population "migrations" on southern partisanship. Overall, the effect of no single migration is large. The largest (black mobilization) is responsible for only a 5.6 per cent decline in the "Other" category – a change that has benefitted the Democrats especially. Moreover, the total absolute impact of black mobilization is nearly as large as that of the two interregional population exchanges combined. Black mobilization was largely a phenomenon of the 'sixties, and it completely reversed the Republican gains from black changes in the 'fifties. The impact of emigration was virtually negligible but would have been larger had not the Democratic hold on these migrants weakened somewhat after 1968. Although its impact was slight, emigration hurt both parties and enhanced independence. Immigration has had a far greater influence on partisan change than emigration and, as expected, incremented Republican strength while reducing that of the Democrats.

The most interesting finding from these data concerns the effect of the three migrations, taken together, on the strength of the two parties. In essence, the net advantage accruing to any one party is small because the Democratic losses arising from interregional population exchanges are largely counterbalanced by

Table 3. Impact of Migrations on 1972 Southern Electorate (Percentages)

	Democrats	Independents	Republicans	Others	Index of Total Effect
Base: 1972 Electorate	49.8	32.7	15.1	2.4	
Base + Emigrants	50.6	31.4	15.8	2.2	
Effect of Emigrants	–0.8	+1.3	–0.7	+0.2	.75
Base – Immigrants	53.9	32.0	11.4	2.7	
Effect of Immigrants	–4.1	+0.7	+3.7	–0.3	2.20
Base – Black Mobilization	46.2	31.1	14.7	8.0	
Effect of Black Mobilization	+3.6	+1.6	+0.4	–5.6	2.80
1972 Electorate Without Migrations	50.4	28.9	12.3	8.4	
Effect of Migrations	–0.6	+3.8	+2.8	–6.0	3.30

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[19] The base for these operations is the partisan distribution of the southern electorate in 1972. The impact of emigrants was estimated by simply adding them to this base. The result is a new distribution of partisanship in the South, assuming that no one had left the region. The impact of immigrants was estimated by removing them from the 1972 electorate. The effect of each interregional migration, then, is the difference between each new distribution and the base. The impact of black mobilization was determined by rearranging the 1972 distribution so that 30.4% of blacks fell into the "other" category (as was the case in 1952) with the remainder distributed proportionally as in 1972, and comparing the result to the base. This counts only movement of blacks out of the "other" class and ignores the changes in black identification between 1952 and 1972.

Democratic gains from the mobilization of blacks. The principal effect of the migrations overall is the movement out of the "Other" category into "Independent" and "Republican." These three population migrations, then, have disturbed only marginally the net balance of partisanship in the South. To explain the dealignment of the southern electorate, one must turn away from these exogenous factors to the major endogenous factor – the behavior of native whites.

Native Whites and Partisan Dealignment in the South

The story of partisan change in the South must be largely a tale of the changing partisan loyalties of whites who have grown up in the South and remained there ever since. This suggests that the Democratic party, that "coalition of the whole" which has dominated southern politics for nearly a century, has eroded from within. The distributions of party identification for native white southerners from 1952 to 1972 are presented in Table 4. This group exhibits marked partisan change during the last two decades. Support for the Democratic party has fallen steadily from more than three-quarters of all native whites in 1952 to barely one-half of them by 1972. At the same time, the percentage of Independents has virtually tripled in generally continuous growth throughout the period.

The principal transformation in the native white electorate occurred between 1964 and 1968, accounting for more than half of the change in both the Democratic and Independent columns. This trend provides circumstantial evidence for the inference that 1968 was a critical election to many southerners. Yet this election did not produce a realignment favoring the Republican party among native white southerners. Their Republican loyalties increased only slightly after 1964 and have not yet returned to their 1960 peak. Instead, changes in the partisan distribution of native whites – the movement from Democratic affiliation to Independent orientation – suggest *de*-alignment. In other words, a sizable segment of the traditional southern electorate has adopted a stance of neutrality towards the two-party system.

On the surface it is plausible that the third-party candidacy of George Wallace in 1968 and his continued flirtation with third-party politics since catalyzed the partisan changes after 1964. Wallace's campaign undoubtedly legitimized a neutral position toward the major parties for many southerners, who accepted his contention that there "wasn't a dime's worth of difference" between the Democrats and the Republicans. Nonetheless, among native whites Wallace received greater support from Democratic identifiers (41 per cent of their votes) than from either Independents or Republicans. Fewer than one-third of the Independents, in fact, voted for Wallace in 1968. Thus, political independence and Wallace support are far from synonymous. More important, the level of independence among native whites was on the increase well before the third-party activity of the late '60s and was maintained in 1972 even though there was no significant third party alternative. While the Wallace movement was no doubt important in changing the partisan predispositions of *some* native whites, it does not provide a satisfactory explanation of large-scale partisan change among native white southerners.

Toward an Explanation of Change among Native Whites. In order to understand contemporary southern politics, one must look to the long-term processes which have eroded the partisanship of the native white electorate. Two distinctly different processes are prime contenders. On the one hand, the changes may have resulted from *generational replacement.* Those native whites who entered the electorate in recent years may have been more Independent and less Democratic than their elders – even than their elders had been at a similar

Table 4. Party Identifications of Native White Southerners, 1952–1972 (Percentages)

	Democrats	Independents	Republicans	Others	Total *N*
1952	76.0	11.6	7.9	4.5	(267)
1956	71.3	14.7	10.8	3.2	(279)
1960	68.9	15.1	14.5	1.6	(318)
1964	68.7	20.4	8.5	2.5	(201)
1968	52.7	36.0	10.4	0.9	(222)
1972	51.0	33.5	13.1	2.4	(412)

stage in the life cycle. An alternative hypothesis is that these changes were the result of *conversion* to political independence by Democrats at all ages.

The empirical test of these alternative hypotheses is quite straightforward. If native white southerners are divided into four-year age cohorts by date of entry into the electorate, their partisan affiliations can be traced neatly across the quadrennial time series. When the change patterns which depict dealignment are found predominantly among the younger cohorts, generational replacement would seem to be the more appropriate explanation. Young voters are likely to be less partisan generally, as a result of their life stage rather than their generation. Thus, these patterns must be shown to persist among the younger cohorts as they age – at least as far as this can be followed in the data. On the other hand, when the change patterns which depict dealignment are spread evenly across all age groups, the conversion hypothesis would seem to be more appropriate.

Table 5 contains the data required for testing these hypotheses. Since the focus is on dealignment, only the totals for Democrats and Independents are presented. Evidence in support of both hypotheses appears, but the magnitude of generational replacement change far outweighs that of change by conversion. Some support for the conversion hypothesis is found by tracing the partisanship of each cohort over time: the percentage Democratic generally declined and the percentage Independent generally increased between 1952 and 1972. The conversion explanation is undermined, however, because this dealignment-type change is consistent and large *only* among the younger age cohorts. The really significant changes are found in those cohorts which entered the electorate since the end of World War II. Each cohort for which we have pre-1968 data exhibits at least a ten per cent decline in Democrats and a nine per cent increase in Independents from time of entry to 1972. These totals are matched only twice among the seven older cohorts for which full data are available. The several cohorts which entered the electorate after 1960 showed an even greater tendency toward independence: rather than "dealigning" as they aged like other post-war cohorts, they began with unusually low Democratic support and unusually high independence which was maintained as they matured.

All in all, those voters who entered the electorate in the postwar period were much more Independent and much less Democratic

Table 5. Democrats/Independents among Native White Southerners by Entry Cohort, 1952–1972[a] (Percentages)

Entry Cohorts	1952	1956	1960	1964	1968	1972
1973–1976						31/56[b]
1969–1972						35/44
1965–1968					27/53	38/46
1961–1964				50/33	13/78	40/43
1957–1960			100/0	46/36	50/50	29/54
1953–1956	100/0[b]	70/25	61/22	75/12	44/38	56/35
1949–1952	75/0	56/35	44/22	57/29	70/25	52/33
1945–1948	71/21	75/8	75/19	53/41	56/44	56/30
1941–1944	58/26	67/25	84/16	53/29	78/22	69/15
1937–1940	72/7	69/17	60/19	91/0	50/43	65/19
1933–1936	70/20	76/8	42/0	80/15	60/25	80/10
1929–1932	96/0	83/0	71/15	61/22	58/33	64/27
1925–1928	70/17	88/4	71/16	73/20	59/35	67/33
1921–1924	82/9	67/20	80/13	94/0	65/15	67/20
1917–1920	75/10	64/9	82/9	100/0	64/18	52/12
1913–1916	83/11	79/7	75/25	50/20	63/16	
1909–1912	88/0	67/11	75/0	87/13		
1905–1908	91/0	82/9	94/6			
1901–1904	73/27	50/8				
Before 1901	88/0					
N	(263)	(279)	(318)	(201)	(222)	(411)

[a] The percentages are underlined where they are based on fewer than ten respondents.

[b] Contains 18-to-20 year olds eligible to vote – in Georgia in 1952 and in all southern states in 1972.

by 1972 than their predecessors. While the youngest postwar voters showed the sharpest movement in this direction, final judgment on their contribution to dealignment must be suspended until they have had more time to "age." That the dealignment pattern is much sharper among the older postwar cohorts (in their late thirties and forties by 1972) who have had time to age than among still older voters signifies a sharp generational break in patterns of partisanship with the beginning of the postwar period. Such a break is exactly what one would expect from change which was the product of generational replacement.

To explain this sharp generational break within the southern electorate – postwar cohorts leading the trend toward dealignment, older voters largely resisting it – two related theoretical reasons may be offered. The first explanation lies within the context of southern politics. The postwar period has been marked by a change in national Democratic party policy on the racial question, traditionally the single most important issue in the South. After the war, most dramatically at its 1948 nominating convention, the Democratic party violated an implicit compact with its southern wing to ignore the racial issue and could not be counted on any longer as the unequivocal defender of white supremacy in the South. The ensuing period of doubt about the party's position ended rather abruptly, as perceptions of party differences on the racial question demonstrate, with the Johnson Administration. As a result of these policy changes on a highly salient issue, southerners entering the electorate in the postwar period found themselves in a significantly different political climate from the one their elders had experienced at entry.[20] As in other periods of change in the meaning of the parties,[21] this change in climate undoubtedly had its greatest impact on young voters – even causing them to cut their inherited partisan ties.

A second explanation for this sharp generational break seems even more compelling because it adds, to this change in policy climate, the entry into the electorate of young people available for mobilization in new directions. It seems likely that the partisanship of the postwar cohorts was unusually fragile to begin with owing to weak childhood partisan socialization. These young voters had not experienced directly the New Deal politics which gave meaning to and thus strengthened the partisanship of earlier cohorts. As a result, the affiliations of the postwar cohorts may have been more malleable. That the period after World War II was a time of changing party appeals to the southern electorate allowed this malleability to be exploited. This theory[22] also accounts for the even greater independence of the youngest generations of southern white natives. Not only had they not witnessed the New Deal, but most of their parents had not witnessed it either. The "reality testing" which undergirds the partisanship of older native whites was two generations removed from them.

Now that the division of the electorate into postwar cohorts and their elders has been justified on empirical and theoretical grounds, the respective cohorts may be combined into two groups for ease of analysis.[23] Those native whites who entered the electorate after World War II are combined in the Postwar Generation. Those who entered the electorate prior to this time are combined in the Solid South Generation – a name chosen to connote continuity in the meaning of Democratic party loyalty (i.e., white supremacy) since the time of Reconstruction. The data for these two generations are presented in Table 6.

The patterns in Table 5 appear in Table 6 with heightened visibility. A decline in Democratic identification within the Postwar Generation has occurred in each successive survey

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[20] Ladd and Hadley concur with this contention. They show convincingly that the South steadfastly supported New Deal policies through the war. Indeed, southerners were found to support the New Deal more fully than residents of any other region. Postwar changes, they argue, were the result of changing Democratic party positions on the racial issue and, more recently, the increasing general conservatism brought about by the modernization of the region. See Ladd with Hadley, *Transformations of the American Party System*, pp. 130–151.

[21] See Paul Allen Beck, "A Socialization Theory of Partisan Realignment," in *The Politics of Future Citizens*, ed. Richard G. Niemi (San Francisco: Jossey-Bass Publishers, 1974), pp. 199–219.

[22] See Beck, "A Socialization Theory," for a full statement of the theory, as well as empirical support for it on a nation-wide scale.

[23] Analysis of the full cohort table is hampered by the large sampling errors which accompany most of the cell estimates. A more detailed analysis of the native white electorate – to be initiated shortly – necessitates some combination of cohorts. For the theoretical and empirical reasons I have cited here, the combination I employ seems to be the most reasonable. To make the two generations used in this study consistent with my earlier work, however, I have divided the sample into those who became 21 years old before 1946 and those who reached this age in 1946 or after. Thus, the 1945–1948 age cohort in Table 5 is broken up in Table 6. This makes no difference in subsequent analysis.

Table 6. Party Identification by Generation of Native White Southerners, 1952–1972 (Percentages)

	Democrats	Independents	Republicans	Others	Total *N*
Postwar Generation					
1952	74.4	12.8	5.1	7.7	(39)
1956	67.2	22.4	9.0	1.5	(67)
1960	64.1	19.6	14.1	2.2	(92)
1964	54.4	32.4	10.3	2.9	(68)
1968	42.0	49.0	7.0	2.0	(100)
1972	41.1	42.7	13.3	2.8	(248)
Solid South Generation					
1952	76.8	11.6	7.6	4.0	(224)
1956	72.6	12.3	11.3	3.8	(212)
1960	70.8	13.3	14.6	1.3	(226)
1964	75.9	14.3	7.5	2.3	(133)
1968	61.5	25.4	13.1	0.0	(122)
1972	65.6	19.6	12.9	1.8	(163)

since 1952. (From Table 5, it is clear that a large part, but certainly not all, of this change is produced by the entry of new age cohorts in each year.) This decline in Democratic strength was sizable between 1964 and 1968. Postwar Generation Democratic affiliation over the entire twenty-year period dropped by a staggering 33 per cent. This change is paralleled by almost as dramatic a gain in the Independent category. By 1972, the Postwar Generation was less Democratic than Independent – a fact which portends a volatile future for southern politics. That only 13 per cent of this generation was Republican by 1972, furthermore, implies that GOP domination of the region is a highly unlikely prospect.

By contrast, net Democratic change for the Solid South Generation is small. Although the Democratic total varies somewhat from year to year, the range of variation for the older voters is only 15.3 per cent – between 1952 and 1968. This compares with the 1952–1972 range of 33.3 per cent for the Postwar Generation. Similarly, there is no apparent trend and relatively slight variation in the Republican loyalties of the older generation. Even the 1968 increase in Independents from a previously stable level and the maintenance of a part of this increase four years later is slight in the face of that recorded for the younger generation.

That the partisan loyalties of the Solid South Generation show only modest change during such a turbulent period is testimony to the insulation from current events of older voters' partisanships. The members of this generation reached political maturity at a time when the Democratic party and white supremacy seemed virtually synonomous. Even severe shocks to this tradition have done little to shake loyalties hardened over years of consistent political behavior. These loyalties have been maintained, it should be added, in the face of sizable defections from the Democratic banner in presidential voting.[24] The tremendous change within the Postwar Generation, by contrast, shows the malleability of partisanship among voters who pass through their formative years at a time of political upheaval. Almost all members of the Postwar Generation of native whites must (because of the distribution of party identification in the Solid South Generation) have come from Democratic homes, but this has not prevented their surge toward Independent orientations.

Most of the partisan change in the native white electorate, then, is attributable to the behavior of its younger members – many of them in their late thirties and forties by 1972. Whether it is reflected in their forsaking of inherited partisan dispositions during their formative early adult years or in their arrival in the electorate with already strong Independent affinities, there is no doubt that a generational replacement process is at work in the South Even if the astonishingly high levels of independence among the youngest members of the Postwar Generation wither with age, there is little reason to believe that the current dealignment will be reversed.

The Impact of Partisan Change among Native Whites. Substantial partisan change within the native white southern electorate appears in the 1952–1972 period. This change derives principally from the behavior of those who entered the electorate after World War II. Only a general sense of the relative impact of changes in these two generations, however, may be gained from the data presented above. Additionally, the contributions to change in the entire southern electorate made by these two generations of native whites remain to be estimated.

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[24] For example, only a quarter of the Solid South Generation Democrats (of native white southern voters) supported McGovern in 1972. About half supported Humphrey in 1968.

Table 7 contains estimates of the relative contributions of three phenomena among native whites – strict generational replacement, change among the Postwar Generation, and change among the Solid South Generation – to partisan changes in the entire southern electorate. The method by which these estimates were derived is the same as that used above to disentangle the effects of various migrations on the 1972 electorate. As applied to native white southerners, it reconstitutes the 1972 electorate as if generational replacement and change within the generations, in turn, had not taken place.

Several different "as if" electorates are supplied. First, the 1972 electorate was reconstructed to yield the partisan distribution which would have resulted had generational replacement in native whites not taken place. This was accomplished by weighting the 1972 partisan distribution for each generation by the proportion of the electorate that generation constituted in 1952. The difference between the reconstructed electorate and the 1972 electorate is the effect of generational replacement of native whites. Second, the electorate was reconstituted for each generation separately to produce the partisan division which would have resulted had there been no partisan changes within the generation. This was accomplished by utilizing the 1952 distributions of partisanship and the proportion of the 1972 electorate each generation constituted. The difference between the 1972 electorate and the reconstructed electorates is the effect of changes in the partisan cast of the generation. The total effect of each type of change was estimated by averaging the absolute values of the effects across partisan categories.

Table 7 shows unambiguously that a substantial portion of the 1952–1972 change in partisanship in the South is due to the behavior of postwar native white entrants into the electorate. The mere replacement of older voters by this new generation has greater effects than the total of the population migrations discussed above. But the principal effect of these younger voters derives from *the erosion of their own partisanship* during the last two decades. This dealignment dwarfs both strict generational replacement and the total impact of the population migrations by more than 2-to-1. By contrast, the impact of partisan change among the Solid South Generation is quite small. Postwar partisan change in the South, then, is largely a tale of the entry into the electorate and the changing partisan dispositions of a new generation of native whites.

Table 7. Impact of Partisan Changes by Native Whites on 1972 Southern Electorate (Percentages)

	Democrats	Independents	Republicans	Others	Index of Total Effect
BASE: 1972 Electorate	49.8	32.7	15.1	2.4	
1972 Electorate without Generational Replacement	56.6	26.4	15.0	2.0	
Effect of Generational Replacement	−6.8	+6.3	+0.1	+0.4	3.40
1972 Electorate without Partisan Change in Postwar Generation	61.9	21.8	12.2	4.1	
Effect of Partisan Change in Postwar Generation	−12.1	+10.9	+2.9	−1.7	6.90
1972 Electorate without Partisan Change in Solid South Generation	52.4	30.8	13.8	3.0	
Effect of Partisan Change in Solid South Generation	−2.6	+1.9	+1.3	−0.6	1.60

Of course, the analysis which has been performed here does not tell the entire story of partisan change in the postwar South. Its focus has been restricted to four noticeable phenomena – immigration, emigration, black mobilization, and generational change. Neglected are such important factors as partisan change among immigrants and emigrants and the contribution of those few respondents who failed to provide usable information on their party, race, or region of origin. Furthermore, attention has been focused primarily on change between the extremes of this 20-year period, not on variability *within* the period. Even with these qualifications, the preceding analysis provides a well-defined picture of partisan change in the postwar South.

Issue Partisanship among Native White Segregationists

The most obvious explanation for dealignment among native white southerners, especially those in the Postwar Generation, is the shift of Democratic party policy on "the racial issue." At the same time that the party has changed its position – first by repudiating an implicit intraparty compact to leave racial policy to its southern wing and then by championing the cause of civil rights – traditional members of its coalition have deserted in droves to political independence and in trickles to Republicanism. It seems but a short logical step to conclude that the desertions *were caused by* Democratic party policy changes on racial issues. The following analysis will attempt to test empirically the hypothesis that racial attitudes provide the basis for partisan change within the native white electorate.

At first glance, ample evidence supports the view that casts racial attitudes in a leading role. As measured by attitudes toward school integration – probably the most salient component of the racial question – the basically segregationist position of native whites declined very little between 1956 and 1972.[25] In fact, largely through an emptying of the "no opinion" categories, the percentage of native white southerners expressing opposition to school integration *rose* slightly from about 63 per cent in 1956 and 1960 to almost 69 per cent in 1968. The South surely contains enough "segregationists" to account for the decline in Democratic support. During much of the same period, perceptions of the party positions on the question of school integration were altered drastically.[26] In 1960, only one-third of all native whites saw party differences on the issue, and of this group, more than two-thirds perceived the Democratic party as more segregationist. These views underwent rapid change in the 1960s. By 1968, a majority of native white southerners perceived party differences and, by an almost 4-to-1 margin, they saw the Democratic party as more integrationist. That this perceptual change paralleled dramatic partisan change *suggests* a relation between the two. One might be led to infer that increasingly widespread perceptions of the Democratic party as pro-integration have caused the sharp decline in Democratic loyalties in a basically segregationist electorate. But aggregate relationships only *imply* individual ones, they do not prove them. It is to the matter of proof that I shall now turn.

The importance of racial attitudes in southern dealignment depends upon the number of native white southerners who are "racial issue partisans." Three conditions must be satisfied for issue partisanship to exist.[27] First, the issue must be salient to the individual; it must be sufficiently salient for him or her to have an opinion on it. Second, the individual must have a distinct perception of party positions on this issue. Third, the individual's opinion, perception or party image, and party identification must be mutually consistent. Satisfaction of all three conditions is necessary for issue partisanship to exist. It is not sufficient, though, because an issue may not be the *cause* of partisan loyalties even where opinion, party image, and partisanship are in perfect balance with one another. The following analysis examines the extent of racial issue partisanship among native white segregationists.[28]

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[25] In 1956 and 1960 respondents were asked to agree or disagree with the statement: "The government in Washington should stay out of the question of whether white and colored children should go to the same school." From 1964 through 1972, the question was: "Do you think the government (in Washington) should see to it that white and Negro children go to the same schools or stay out of this area as it is none of its business?" Segregationists were defined as those selecting the "stay out" alternative in each case. A school integration question was not asked in the 1952 survey. The focus on federal government intervention probably accounts for both lower support for school integration and much lesser growth in support than are reported by Greeley and Sheatsley for whites in the South and border states in 1963 and 1970. See Andrew M. Greeley and Paul B. Sheatsley, "Attitudes toward Racial Integration," *Scientific American*, 225 (December, 1971), 13–19.

[26] The question asked in 1960 was: "Which party do you think is more likely to stay out of the question of whether (the government sees to it that) white and colored children go to the same school; the Democrats or the Republicans, or wouldn't there be any difference?" In 1964 and 1968, the question was: "Which party do you think is more likely to want the government to see to it that the white and Negro children go to the same schools? The Democrats, and Republicans, or wouldn't there be any difference between them on this?" No party image question on school integration was asked in 1972, and the party image question asked in 1956 is not strictly comparable with the others.

[27] See Richard A. Brody and Benjamin I. Page, "Comment: The Assessment of Policy Voting," *American Political Science Review*, 66 (June, 1972), 455–457, for a clear statement of the criteria for issue voting. The criteria I have used represent a modification of those suggested by Brody and Page.

The Elections of 1960 to 1968. Figure 1 displays the relationship between party image and party identification for native white segregationists.[29] In 1960, party image and party identification showed some linkage. Segregationists who saw the Republican party as the party more likely to support segregation (REP CON) were more Republican than those who saw the Republican party as less favorable to their position (DEM CON). Furthermore, the highest percentage of Independents is found among those perceiving no difference. On the whole, however, the message of Figure 1 is that racial attitudes had little impact on Democratic loyalties in 1960, since more than 65 per cent of the segregationists in each party image category were Democrats. In 1964, this conclusion appears to be even more appropriate. Partisan differences among those with various party images were slight, with Democratic affiliations a clear majority everywhere. Al-

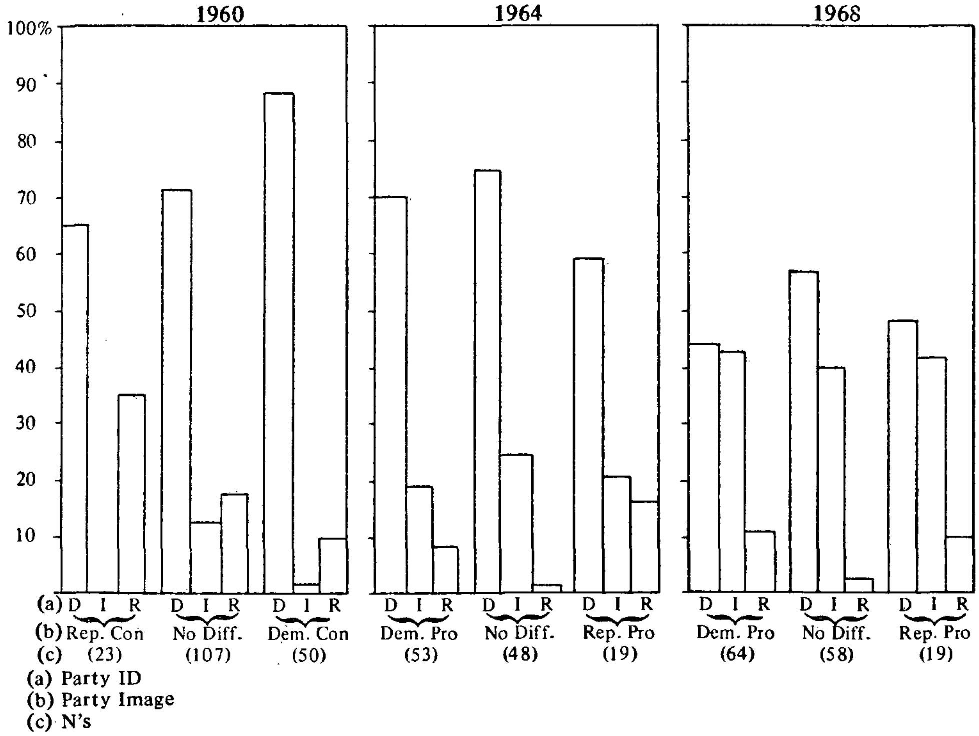

Figure 1. Party Identification and Party Image on School Integration Issue (Native White Segregationists Only)

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[28] Analysis may be restricted to segregationists – those opposing governmental intervention to promote school integration (1960–1968) or to aid blacks and other minorities (1972) – because of the nature of the hypothesized impact of racial attitudes. The principal change in southern partisanship has been the decline in Democratic loyalties at a time when that party was emerging as the champion of the civil rights cause. Thus, it is anticivil-rights attitudes which should be looked to as the possible source of change.

[29] Because of changes in question wording between 1960 and 1964, the perceived party position categories are different. Respondents in 1960 were asked to choose the more anti-integrationist party, while in 1964 and 1968 they were asked to identify the more integrationist party. Hence, in Figure 1, "con" positions are entered for 1960 and "pro" positions for the later years. Because of the possibility that no party differences were perceived, "con" positions in 1960 are *not* the reciprocal of "pro" positions.

though party images had undergone considerable change between these two elections, with the Democrats replacing the Republicans as the party perceived to be pro-integration, image alteration had little *net* impact on party identification. If anything, more inconsistency among opinion, image, and loyalty appeared, particularly in voters who perceived the Democrats as pro-integration.

This cognitive inconsistency continued into 1968. Democratic affiliation fell off and independence increased among those holding each party image. While the lowest percentage of Democratic identifiers appeared where the Democratic party was linked perceptually to integration, the difference between this figure and that for those regarding the Republican party as pro-integration is negligible. This is surely not the stuff of issue-based partisanship! At least through 1968, there is no evidence that the dealignment of the native white electorate resulted principally from the racially based partisan changes. Unfortunately, this analysis cannot be continued into 1972 because of the absence of a party image question on school integration from the SRC/CPS 1972 survey.

One can posit several reasons why segregationist attitudes, party perceptions, and party affiliations remained inconsistent for native whites through 1968. Surely the most obvious to students of public opinion is that civil rights might not have been an issue of overwhelming salience to most southerners. Mass electorates can tolerate many inconsistencies in their cognitions, particularly when the elements lack salience. Before we can draw the surprising conclusion that the civil rights issue was not instrumental in the dealignment, though, two problems with the preceding analysis must be solved. First, analysis of the entire native white electorate neglects a principal premise of earlier analysis: that changes in partisanship to produce attitudinal consistency are unlikely among older voters but would seem to be possible for the younger generation. Thus, we should look to the younger generation for the bulk of civil rights issue partisanship. Second, the SRC/CPS party image question is not adequate for the task assigned it: by gauging only perceptions of which party is "con" in 1960 and "pro" in 1964 and 1968, the question does not determine precisely whether either party is perceived by the respondent as reflecting his or her own views on racial matters.

The Election of 1972. A question in the SRC/CPS survey satisfies the second problem with the preceding analysis by permitting a full examination of the relationship between party image and party for opponents of black civil rights. Respondents were asked to position themselves and *both* parties on a scale of approval of government efforts to improve the positions of blacks and other minority groups.[30] As expected, most native white southerners who had opinions located themselves on the "conservative" side of this scale – advocating that minority groups help themselves rather than be helped by the government. Fewer than 12 per cent of all native whites were favorable to government intervention on behalf of minorities. Furthermore, the modal image of the Democratic party was interventionist, while the modal image of the Republican party cast it in a middle-of-the-road position on the issue.

The relationship between party image and party identification for all native white conservatives on the minority aid issue is displayed in Figure 2. Racial attitudes are clearly related to party identifications. What was either hidden in the incomplete distinction between party images in the school integration question or had not yet developed by 1968 is present here. Among those native white conservatives who perceive the Democratic party as supporting their own positions (the Democrat and Both categories), Democratic party loyalties approach their 1952 level. If these respondents comprised the entire native white electorate, there would have been no noticeable partisan changes between 1952 and 1972. In reality, only 29 per cent of all native white conservatives fell into these two categories. This amounts to a surprisingly high amount of what might be labeled misperception, and this misperception seems responsible for a fair portion of the staying power of Democratic loyalties. It was far more common, though, for respondents to see neither party as sharing their views on minority aid (49 per cent) or to perceive the Republican party as a partner in these views (22 per cent).[31] Both of these images, especially the latter, were accompanied by considerable erosion in Democratic strength. Indeed, Democratic loyalties for the group which perceives the Republican party as sharing its position were less common than was political independence.

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[30] The scale contains seven opinion categories, ranging from "government should help minority groups" at one extreme to "minority groups should help themselves" at the other. For the purposes of this analysis, responses were trichotomized into neutral (category four), liberal or pro-aid, and conservative or anti-aid. The distribution of respondent opinion on the minority aid question approximates that for school integration in 1972, with government intervention a bit less popular for minority aid than for school integration. Attitudes towards government intervention in minority aid and school integration exhibit a gamma correlation of .34. While the items clearly share a domain of content, this correlation suggests also some empirical distinctiveness between the items – making it impossible to determine whether the relationships to be discussed below are unique to the minority aid question or would appear for school integration too in 1972. Had perceptions of party positions on school integration been elicited in 1972, of course, I would have been able to use both items in analysis. A focus on school integration would have provided continuity with earlier years; a focus on minority aid would have contributed the full picture of party images which the school integration question did not.

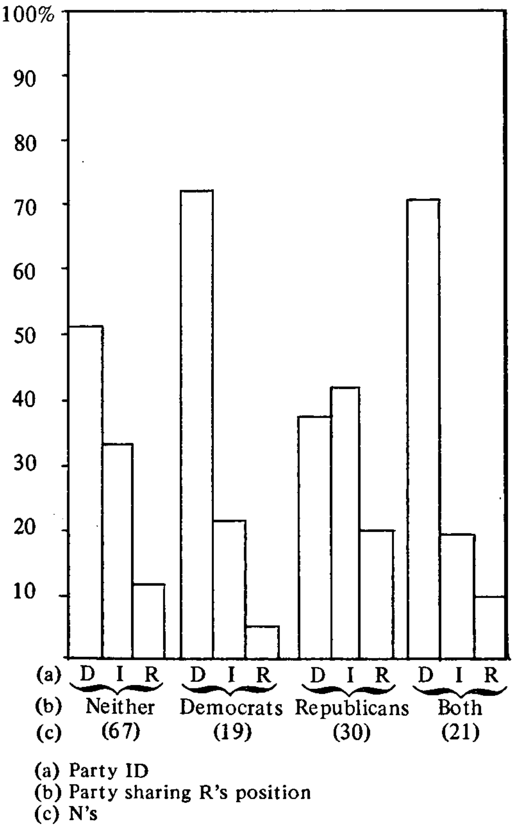

Figure 2. Party Identification and Party Image on Minority Aid Issue, 1972 (Native White Conservatives Only)

Upon closer examination, another interesting pattern may be detected. Even when favored by party image, the Republican party achieved little success in attracting new loyalists. The surprising lack of Republican party loyalty among native whites shown in Table 4 recurs where conditions would seem to be most propitious for its development. Whether this lack of loyalty was due to an inadequate gestation period for the emergence of a party-image/party-loyalty linkage or the failure of the Republicans to attract voters for other reasons, these data *do not* augur well for a pro-Republican realignment. Rather, the data suggest that dissatisfaction with Democrats – no matter what stance is ascribed to the Republican party – commonly resulted in partisan neutrality or independence.

An even clearer picture results when generational controls are applied. Given the theory of partisan change outlined above and the data on the party identifications of the different generations, it should come as no surprise to find party loyalties related only weakly to party images for older opponents of minority aid, but strongly related for younger conservatives. Figure 3 shows these relationships for the Solid South and the Postwar Generations separately. For the older generation, while lower levels of Democratic loyalty are registered where the Democrats were perceived to have a position incompatible with the respondent's own views, Democratic loyalties remain the most common in every category. In the younger generation, on the other hand, the Democrats suffer considerable erosion among those native whites whose views on minority aid were opposite to the perceived Democratic position. Indeed, when only the Republican party was perceived to share a conservative viewpoint, the majority were Independents, and equal (and small) numbers were Democrats and Republicans. These relationships are surely of the direction and magnitude one would expect for a basic transformation in partisan strength arising from the civil rights attitudes of the Postwar Generation.

Finding a relationship between party image and party identification for younger white conservatives, however, is only the first step towards demonstrating that racial attitudes made a substantial contribution to partisan dealignment in the South. The second step involves estimating the relative number of native white conservatives who satisfy the three conditions for issue partisanship. The final step is an assessment of the impact conservative civil rights issue partisans have had on the distribution of partisanship in the region.

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[31] Virtually a majority of the respondents had no perceptions of either party's position on this question. Well over half of these nonresponses are due to the absence of opinions on the initial minority aid issue. Whatever the reason for their appearance, nonresponses detract from the extent of issue partisanship.

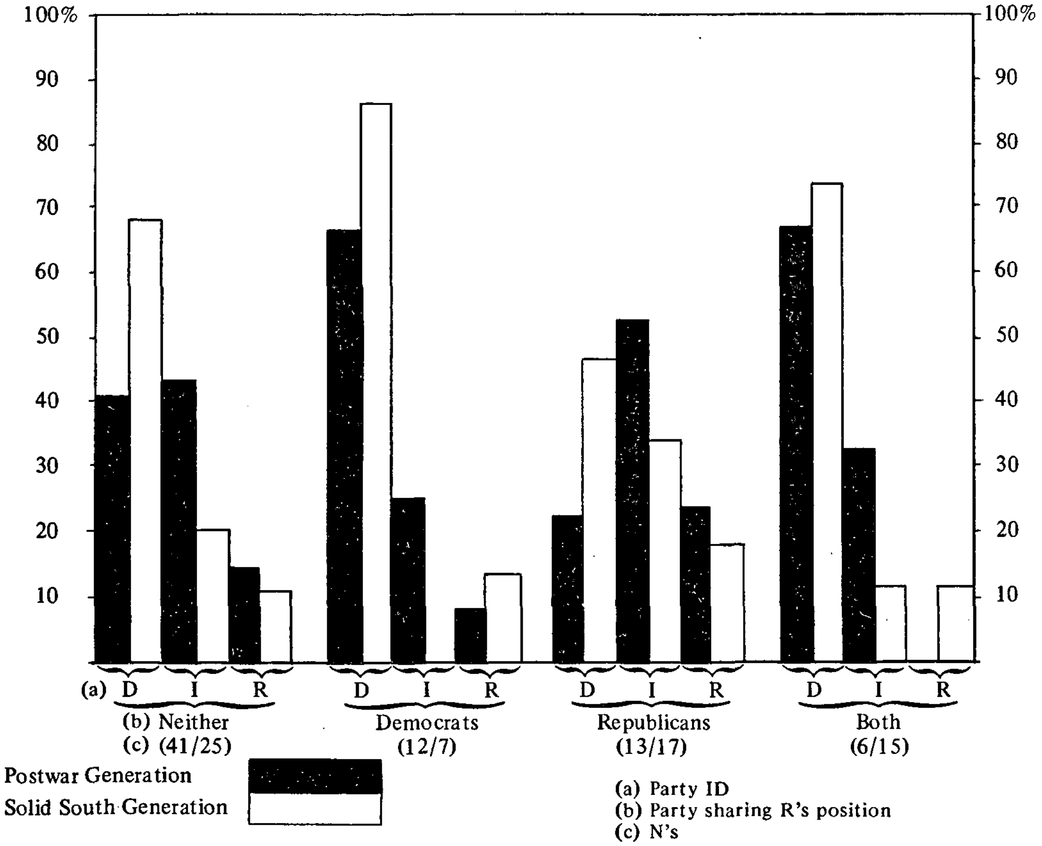

Figure 3. Party Identification and Party Image on Minority Aid Issue by Generation in 1972 (Native White Conservatives Only)

At a maximum, only 15.7 per cent of all native whites qualify as conservative issue partisans. This estimate is derived from the relative number of those opposing aid to minority groups who either perceive the Democratic party as not sharing their opinion and fail to identify as Democrats (12.3 per cent) or perceive the Democrats as opposed to aid and call themselves Democrats (3.4 per cent).[32] The 12.3 per cent figure is the upper limit on the number of native white conservatives who *could have deserted the Democratic party* because of the racial issue. The number of Democratic identifiers would have to decline even more than it has for this limit to rise. The total of conservative issue partisans is no higher as a result of both the high number of respondents without opinions or perceptions on the issue and the hardened Democratic loyalties among the Solid South Generation. When the two generations are examined separately, the greater capacity of younger voters for changing their party identification to produce cognitive consistency becomes apparent. The Postwar Generation contains a higher proportion of "correct perception" issue partisans and slightly fewer respondents whose cognitive consistency depends upon *(mis-)* perceiving the Democratic party as segregationist.

The preceding total (15.7 per cent) was attained by a highly generous coding of issue partisans: where the Democratic party was perceived as favorable or neutral toward minority aid, any non-Democratic loyalties were counted as consistent with issue partisanship regardless of the Republican party image. A more reasonable coding would take into account the images of both parties and would

[32] The task of identifying segregationist issue partisans is complicated by the virtual absence of any trend toward Republican affiliations. The GOP may not yet have been perceived by many southerners as a viable alternative for long-run commitments of loyalty. In the initial coding of issue partisans, this possibility will be considered, and only perceptions of Democratic party positions will be utilized.

count Independents as issue partisans among these conservatives only if neither party was accorded an antiblack position. *Following this rule, the relative number of conservative issue partisans among all native whites drops to 10.4 per cent,* about one-third of whom misperceive party positions. Members of the Postwar Generation remain more likely (by a 3-to-2 margin) to be issue partisans under this strict rule. When those issue partisans who perceive the Democratic party as anti-aid are removed, the ratio of Postwar Generation to Solid South Generation issue partisans reaches almost 2-to-1.

On the whole, however, the extent of issue partisanship among native white conservatives is surprisingly low. The major constraint on this figure, given the widespread segregationist attitudes in the region, is the high number of Democratic loyalists – especially older voters. That no more than one of ten have brought their partisan loyalties into line with their racial attitudes and party images belies the popular image of whites motivated largely by their racial prejudices in establishing their long-term loyalties. Perhaps this image was once accurate but is no longer. The presumptive decline in the importance of race may be another significant feature of the postwar pattern of southern politics.[33] Yet to students of mass publics, the low level of issue partisanship found here should not be surprising at all. Where politics is concerned, few people seem compelled to bring their cognitive structures into consistency. Politics seems to have such a low degree of saliency for most Americans that inconsistency is easily bearable. Perhaps this is why Democratic loyalties have held up under even adverse issue conditions.

The preceding discussion should not be interpreted to imply that antiblack issue partisanship has made little contribution to native white dealignment. If all conservative issue partisans are assigned Democratic loyalties, assuming that they would have been Democrats except for their racial attitudes (a highly dubious assumption), Democratic partisanship increases to 58.1 per cent among native whites in 1972, while Independent orientations decrease to 27.8 per cent. When the more generous coding for issue partisans is used, to reflect Democratic dealignment rather than a realignment in a specific direction, the totals are 63.4 per cent and 24.7 per cent, respectively. Thus, *a significant share of the partisan change within the native white electorate may be attributed to racial conservatives' desertions of the Democratic party because of its racial policies.* The facts that an even larger share cannot be traced to the racial question and that there has not been more partisan change among native whites suggest, however, that this issue is not the sole concern of native whites.

In conclusion, much of the dealignment in the South remains open to an explanation by other factors. Among them may be forces which are *national,* rather than *regional,* in scope. Dealignment has occurred among the northern electorate as well as the southern, for reasons which are largely unrelated to racial issues. The socialization explanation for dealignment, thus, seems more useful than one which focuses solely on issue partisanship.[34] Furthermore, given all of the opposition to civil rights policies in the South, it is significant that there has not been more dealignment. This tendency too is testimony to the power of the partisan socialization process for older voters – their partisanship, hardened long ago, is largely undisturbed by current issues. Of course, issue *voting* rather than issue *partisanship* may be widespread in the South and may well account for the electoral response of the region in recent elections. As long as the former does not turn into the latter, however, dealignment will continue to be the operative process.

Conclusion

Analysis of changes in the party identifications of southerners during the last twenty years has exposed the roots of both continuity and change in southern politics. Change, the manifest mark of the region, has several sources. Interregional population migrations have diluted Democratic strength and slightly increased that of the GOP by exchanging strongly Democratic "rebels" for more Republican and less Democratic "Yankees." Although these migrations continue into the 1970s, their major impact was absorbed by the mid–'60s.

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[33] Additional evidence of a decline in the importance of racial issues in the South comes from analysis of the questions in the SRC/CPS survey concerning likes and dislikes of the two parties. See Merle Black and George Rabinowitz, "An Overview of American Electoral Change: 1952–1972," paper presented at the Annual Meeting of the Southern Political Science Association, New Orleans, November 8, 1974. Also see Richard J. Trilling, *Party Image and Electoral Behavior* (New York: Wiley, 1976), pp. 149–173.

[34] Partisan dealignment is a nationwide phenomenon, although it is less pronounced outside the South. See Inglehart and Hochstein, "Alignment and Dealignment. . . ," for regional comparisons. See Beck, "A Socialization Theory," for the socialization explanation of the dealignment process.

Since then, their pace has ebbed and their impact has become more marginal. Other change-producing forces have become more important in recent years. One of these other forces is the mobilization of black southerners into politics. As the pace of this mobilization has quickened, there has been a renewal of Democratic loyalty – overwhelming the legacy of the population exchanges between the regions. The net effect of these three changes, then, has been partisan continuity.

Another source of political change in the postwar South is the changing partisan orientations of the native white electorate. Many of the native whites who entered the electorate after World War II, in particular, have taken up the mantle of political independence. A substantial number, though hardly a majority, of these desertions appear to have been motivated by racial attitudes. Many native whites opposed to better treatment of blacks have left the Democratic party for political independence and in rare cases for the Republican party. Such changes are concentrated in the Postwar Generation of native whites and provide an excellent illustration of change through the generational replacement process.

If the roots of change in southern politics lie in the generational newcomers to the southern political arena, the dampening of this change may be traced to the vestiges of the prewar southern electorate. The members of the Solid South Generation formed their partisan loyalties in a different political era, when the Democratic party was the protector of the status quo in the South. Over the years, these loyalties seem to have hardened as if partisanship was a primary pillar of political identity for these older voters–resistant to even an erosion in cognitive support for the party. In fact, so strong are these loyalties, that the older voters have supported candidates of the opposite party often without changing their partisanship.

At least three important implications for the future of southern politics may be drawn from these findings. First, the evidence points consistently away from a forecast of partisan realignment. Gains in identification with the Republican party in the last two decades have been meager indeed, and the few which appeared through 1960 have been reversed since. With the potential for widespread black Republicanism gone, the impact of interregional population exchanges largely absorbed, and the lack of Republican support among the young, GOP prospects for the future seem dim. While Democratic prospects are somewhat better, there is reason for that party as well to be highly concerned about its standing in the South. The core strength of the Democratic party in the native white electorate has suffered very damaging shocks, particularly since 1964. The low level of Democratic support among young native whites suggests little promise of recovery. In short, the future seems bright for neither party. Too large a share of the southern electorate has repudiated party ties. The politics of dealignment are upon the South and should remain dominant for quite some time.

The second implication for the future of southern politics is merely an extension of the first. Partisanship will play a restricted role in the future, just as it has in the past few years. As this happens, the region's politics will become more unstable. There is substantial irony in this trend: In the past, political conflict showed little continuity or structure in the South because all battles were fought out among factions in the dominant Democratic party. In the future, the same result should occur because neither party will be strong enough to organize politics around interparty strife. Key and Schattschneider, among others, have argued that viable *party* competition is the essential ingredient of democracy.[35] It remains doubtful that popular influence on public policy – the core element of political democracy – can be high in the South if political conflict is not structured by competitive parties.

But the picture is not entirely gloomy. There are grounds for optimism about the South's political future for while antiblack attitudes have contributed to the process of dealignment in the South, their contribution was not as pronounced as might have been expected.[36] Only about one in ten native whites qualified as anticivil-rights issue partisans in 1972. At least an equal number of southerners – most but not all of them black – met the conditions for procivil-rights issue partisanship. Furthermore, after years of presumably high salience, there is good reason to believe that the racial question is losing its position as an important issue to southern voters. The issue may well have had already its maximum impact on the region's politics. This is a message which seems to have been heard by even the most segregationist of politicians. It can only be a healthy sign for the future of both races in the South.

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[35] See V. O. Key, *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949) pp. 298–311; and E. E. Schattschneider, *Party Government* (New York: Holt, Rinehart, and Winston, 1942).

[36] It is quite possible, of course, that students of southern politics have overemphasized the importance of the racial question in the past. This would not be the first time that elite images of mass publics fell wide of the mark. Yet traditional southern politics seems to have been designed so as to maximize the impact of even a small segregationist issue public. The cost of segregationist rhetoric to politicians was low because of the disenfranchisement of the target group. Traditionally low rates of voting participation in the region's elections also increased the potential impact of an issue public. Finally, the absence of *structured* competition in the region's only competitive elections (the primaries), if only because of the uncertainty about outcome it imposed, made intense issue publics very valuable and, hence, strongly courted as potential members of an electoral coalition.

Epilogue

Since this article was written, a southern Democrat has been elected president by sweeping his home region. Jimmy Carter's victories in nine of the ten southern states may appear at first glance to have disrupted the patterns of electoral change depicted in this article. An examination of the sources of his support in the region dispels this notion. While Carter received overwhelming backing from blacks, he apparently finished behind Ford among whites. This suggests that the movement of white southerners away from the Democratic party held up under its most difficult test yet – in which the Democratic standard bearer was a southerner. Thus, it seems likely that the 1976 election will provide further evidence of partisan dealignment in the South when survey data have been analyzed fully. The real impact of the 1976 election on dealignment in the South will depend upon how effectively the new president can capitalize on future opportunities to shape the agenda of southern politics, not on the circumstances which placed him in the White House in the first place.

Contextual Models of Electoral Behavior: The Southern Wallace Vote*

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Introduction

Since V. O. Key, Jr.'s, seminal work, *Southern Politics,* the relationship between southern white support for racist politics and candidates and black concentration in the immediate political context has been frequently studied by social scientists.[1] The candidacy of George Wallace for president in 1968 presented a particularly promising opportunity to test further Key's black concentration hypothesis. Researchers taking this opportunity, however, have done so with rather inconsistent results. This paper seeks to resolve these inconsistencies through a revised methodological strategy for contextual analyses of electoral behavior. This two-part strategy includes the synthesis of multiple-level data sets and systematic construction and comparison of alternative models of contextual effects, each representing explicit theoretical assumptions. The need for a different approach is pointed up by a brief overview of studies on the relationship between percentage black and percentage voting for Wallace.

Relying on strictly aggregate data, Schoenberger and Segal in a Southwide study found that percentage black was the strongest correlate of the Wallace vote at the congressional district level and suggested that this "demonstrates a contextual effect, *viz.,* the greater the concentration of blacks in a congressional district, the greater the propensity of whites in that district to vote for Wallace."[2] The hypothesis receives further support from studies of the bases of Wallace support in individual states.[3]

Other studies of southern states, however, have not found strong support for the relationship between black concentration and Wallace

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[1] V. O. Key, Jr., *Southern Politics* (New York: Knopf, 1949), pp. 5–10, 317–344. A suggestive, but by no means exhaustive list of these studies in the area of voting would include: Earl Black, "The Militant Segregationist Vote in the Post-*Brown* South," *Social Science Quarterly,* 54 (June, 1973), 66–84; Thomas F. Pettigrew and Ernest Q. Campbell, "Faubus and Segregation: An Analysis of Arkansas Voting," *Public Opinion Quarterly,* 24 (Fall, 1960), 436–447. On Negro voter registration, see: Hugh D. Price, *The Negro and Southern Politics* (New York: New York University Press, 1957); John H. Fenton and Kenneth N. Vines, "Negro Registration in Louisiana," *American Political Science Review,* 51 (September, 1957), 704–713; Donald R. Matthews and James W. Prothro, "Social and Economic Factors and Negro Voter Registration in the South," *American Political Science Review,* 57 (March, 1963), 24–44; Charles F. Cnudde, *Democracy in the American South* (Chicago: Markham, 1971). And on resistance to racial integration, see: Thomas F. Pettigrew, "Demographic Correlates of Border-State Desegregation," *American Sociological Review,* 22 (December, 1957), 683–689; Pettigrew, "Regional Variations in Anti-Negro Prejudice," *Journal of Abnormal Social Psychology,* 59 (July, 1959), 28–36; Thomas F. Pettigrew and M. Richard Cramer, "The Demography of Desegregation," *Journal of Social Issues,* 15 (October, 1959), 61–71; Donald R. Matthews and James W. Prothro, "Stateways Versus Folkways: Critical Factors in Southern Reactions to *Brown v. Board of Education,*" in *Essays on the American Constitution,* Gottfried Dietze, ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1964), 139–156; James W. Prothro, "Stateways Versus Folkways Revisited: An Error in Prediction," *Journal of Politics,* 34 (May, 1972), 352–364.

[2] Robert A. Schoenberger and David R. Segal, "The Ecology of Dissent: The Southern Wallace Vote in 1968," *Midwest Journal of Political Science,* 15 (August, 1971), 585.

[3] Robert D. Wrinkle and Jerry L. Polincard, "Populism and Dissent: The Wallace Vote in Texas," *Social Science Quarterly,* 54 (September, 1973), 319; Earl Black and Merle Black, "The Wallace Vote in Alabama: A Multiple Regression Analysis," *Journal of Politics,* 35 (August, 1973), 730–736; Stephen S. Bridsall, "Preliminary Analysis of the 1968 Wallace Vote in the Southeast," *Southeastern Geographer,* 9 (November, 1969), 61–62.

[4] Donald S. Strong, "Further Reflections on Southern Politics," *Journal of Politics,* 33 (May, 1971), 249;

voting.[4] Wasserman and Segal take explicit exception to the earlier analysis of Schoenberger and Segal.[5] Using county level data, they first find a moderately strong positive southwide correlation (.38) between county percentage black and percentage voting for Wallace, but when counties are separated into the "upper" and the "lower" Souths, the two regional correlations actually have opposite signs. The upper South correlation is positive (.43), while the lower South correlation is negative (–.31). According to the authors: "This negative correlation of the Wallace vote and the per cent black in the county in the lower South is caused by the higher concentration of nonwhites in the counties of the lower South."[6] Wrinkle and Polincard interpret this particular statement to mean that Wasserman and Segal ". . . thus specifically reject the contextual effect hypothesis."[7] The results of studies looking at the relationship between percentage black and voting for Wallace, then, are not consistent.

Problems in the Use of Aggregate Data

The inability of previous studies clearly to validate or reject the black concentration hypothesis stems from ambiguities inherent in their reliance solely on aggregate data. The hypothesis of white support for Wallace as a positive function of black concentration has been examined previously by correlating percentage voting for Wallace with percentage of the population that is black. One problem with this procedure is highlighted in the following decomposition of the proportion of voters casting ballots for Wallace (allowing the letter p to stand for proportion):

> p for Wallace = (p of white voters for Wallace) (p of voters who are white) + (p of black voters for Wallace) (p of voters who are black)

or

$$Y = (A)\,(B) + (C)\,(D) \qquad (1)$$

The aggregate proportion of all votes for Wallace (Y) equals $A \times B$ + $C \times D$, but the intention of previous studies has implicitly been to assume a simple identity between Y and A. The value of C, the proportion of black voters supporting Wallace, can be assumed to be zero so the equation simplifies to $Y = A \times B$. The assumption that $Y = A$, is correct, however, only under the very restrictive – and discriminatory–situation in which all voters are white, i.e., $B = 1.0$. This means that Y underestimates A, actual white support for Wallace, to the extent that blacks are a proportion of all voters. Also, evidence that black turnout is related to percentage black in the population, suggests that measurement error in the dependent variable, percentage voting for Wallace, may be systematically related to the independent variable, percentage black.[8] Hence, the aggregate percentage of all voters casting ballots for Wallace is a rather poor measure of support for Wallace among whites.

The discussion by Adam Przeworski of specification of contextual effects from aggregate data also merits attention.[9] He suggests a procedure whereby individual and contextual effects may be estimated using aggregate data, if certain assumptions can be justified. In the present case it is reasonable to assume that blacks do not support Wallace, regardless of levels of black concentration. This assumption of no populationwide contextual effect would seem to allow identification of the Przeworski equations.[10] Unfortunately, the usefulness of

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Glenn R. Parker and Stephen H. Wainscott, "Political Consequences of Racial Prejudice: A Causal Interpretation." Paper presented at the Annual Meeting of the Southern Political Science Association, New Orleans, November 8, 1974.

[5] Ira M. Wasserman and David R. Segal, "Aggregation Effects in the Ecological Study of Presidential Voting," *American Journal of Political Science,* 17 (February, 1973), 177-181.

[6] *Ibid.,* p. 180.

[7] Wrinkle and Polincard, "Populism and Dissent," p. 311.

[8] The relationship between black electoral participation and black concentration appears to be changing over time, and perhaps varies in different areas of the South. The relationship was negative throughout the South in the 1950s (Matthews and Prothro, "Social and Economic Factors and Negro Voter Registration in the South"), positive in Alabama in the 1960s (Johnnie Daniel, "Negro Political Behavior and Community Political and Socioeconomic Structural Factors," *Social Forces,* 47 (March, 1969), 274–280, and curvilinear in the late 1960s in Mississippi (Sam Kernell, "Comment: A Re-evaluation of Black Voting in Mississippi," *American Political Science Review,* 69 (December, 1973), 1311–1315.

[9] Adam Przeworski, "Contextual Models of Political Behavior," *Political Methodology,* 1 (Winter, 1974), 44–50. See also, Tapani Valkonen, "Individual and Structural Effects in Ecological Research," in Mattei Dogan and Stein Rokkan, eds., *Quantitative Ecological Analysis in the Social Sciences* (Cambridge, Mass.: The MIT Press, 1969), pp. 66–68.

[10] *Ibid.,* pp. 47–49.

that approach is vitiated by also having to assume that the racial composition of voters (the base for percentage for Wallace) is the same as the racial composition of the population (the base for measuring black concentration). Since the proportion of blacks turning out to vote within each unit is unknown, the problem of testing the hypothesis with aggregate data again evades solution.

Neither can we rely on orthodox survey methods. Whereas ecological studies stress context but rarely contain individual-level statements, survey methods have a tendency to focus attention so completely on individual-level variables that the influences of contextual factors are typically ignored. Since the essence of contextual models generally, and the black concentration hypothesis in particular, is to relate environmental characteristics to individual behavior, the optimal strategy is one which combines contextual information with individual-level survey data.

A Method for Contextual Analysis

Analysis of survey respondents in the socioeconomic contexts in which they live dictates the synthesis of multiple data files; contextual information must be integrated with data on individual attitudes and behavior. Once the decision to use multiple data files is made, the appropriate contextual level and units of analysis must be selected. Following V. O. Key and many others, I shall use the county initially as the contextual unit. The county is particularly meaningful in the politics of the South, where local political activity so often focuses on the office of county sheriff. The county is also closer to our notion of local "community" than any alternative unit for which systematic census and electoral data are available. Finally, using the county as the contextual unit allows the greatest comparability with previous research. Contextual information used in this study is drawn from standard census sources.[11] It is not advisable, however, to assume automatically that all contextual influences operate from only one level. For reasons made clear below, contextual influences are also considered at the state level.

The unit of analysis in a study employing multiple data files may be either the contextual unit or the individual survey respondent. Most of the previous studies combining aggregate and individual-level data have averaged individual responses up to the level of the contextual unit, leaving the context as the unit of analysis. Here the survey respondent is maintained as the unit of analysis, and the contextual data file is expanded to match cases at the individual level. That is, characteristics of the individual's environment (percentage black and measures of urbanization) are added to the survey data file and treated as individual-level variables for purposes of analysis.

The survey data examined in this study are from the Comparative State Election Project (CSEP) of the University of North Carolina. The sample design of the CSEP survey included representative cross-sections of the adult voting-age populations of thirteen states as well as supplemental interviews in the remaining states and the District of Columbia. A total of 7,673 interviews were completed in the weeks following the 1968 election.[12] Our interest here is in the weighted sample of southerners (N=3,126) and particularly the subsample of southern white voters (N=1,292). The southern CSEP respondents were drawn from 604 sample points distributed across 309 southern counties. The characteristics of these 309 counties are linked as local contextual variables to the 3,126 southern survey respondents. This merging of contextual information on percentage black with individual voting behavior data permits us to determine the effects of black concentration on southern white responses to George Wallace while controlling for possible sources of spuriousness at both the individual and contextual levels.

The dependent variable of this analysis, vote for or against George Wallace for president, is unambiguous. If a person voted for president in 1968, he voted either for Wallace or for some other candidate. Hence, the dependent variable is treated as a simple dichotomy. Proper measurement of the independent variable, black concentration, is less obvious. Local black concentration is measured initially at the county level, but which census year should be chosen for measuring that variable? Price has suggested that racial attitudes develop during the formative years, and that such attitudes may well persist through substantial changes in the racial composition of the local population.[13] Examining such a hypothesis, Matthews and Prothro found that percentage Negro in 1900 produced almost as high a correlation with black voter registration as did 1950 percentage black.[14] If the earlier data had an independent relationship with black registration while controlling for recent data, then one could argue that history, or earlier socialization experiences, were having an independent influence on present behavior. Matthews and Prothro, however, found no association between black registration and 1900 percentage black once 1950 percentage black was controlled.

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[11] County-level variables are taken from U.S. Bureau of the Census, *County and City Data Book, 1949* (Washington, D.C.: G.P.O., 1952), Table 3; *County and City Data Book, 1972* (Washington, D.C.: G.P.O., 1972), Table 2. State-level variables discussed below are taken from U.S. Bureau of the Census, *Statistical Abstracts of the United States: 1942,* 64th edition (Washington, D.C.: G.P.O., 1943), p. 13; *Statistical Abstracts of the United States, 1973,* 94th edition (Washington, D.C.: G.P.O., 1973), p. 29.

[12] For a more complete description of the CSEP data set, see Elizabeth M. Fischer and David M. Kovenock, *Guide to the Comparative State Elections Project Survey Data* (Chapel Hill, N.C.: Institute for Research in Social Science, 1975).

For the present study, measures of black concentration were gathered for 1970 as the closest census year to the 1968 elections, and for 1940, because it predates the Civil Rights Movement and catches much of the adult population in the formative years of adolescence and early adulthood. If racial attitudes and behavior are strongly rooted in the past, then the data on 1940 percentage black should have an independent influence on support for Wallace in 1968.

In support of the black concentration hypothesis, Table 1 shows that both the 1940 and 1970 measures of state and local black concentration have moderate positive correlations with individual southern white voting for and against Wallace for president. The partial correlations in Table 1, however, indicate that black concentration in 1940 has the only independent effects. Thus, individual behavior in this instance appears to be largely a product of past contextual influence. Throughout the remainder of this study, therefore, 1940 proportion black will be used as the measure of black concentration at both the local and state levels.[15]

Local Black Concentration and Region

The correlations just presented relating the contextual variables of per cent black to the individual-level variable of voting for or against Wallace pertain only to the entire South. As discussed above, Wasserman and Segal use aggregate data to find a similar positive correlation throughout the South, but they find correlations of opposite signs in the upper and lower Souths.[16] Our immediate interest, therefore, is to see if these different signed relationships appear when voting is measured at the individual level.

The different signs for the regions do appear when *all* southern presidential voters, black and white, are included in the analysis – which is analogous to the data obtained using ecological correlations. The Deep South correlation between county black concentration and voting for and against Wallace is negative ($r=-.094$), and the Marginal South correlation is positive ($r=.240$).[17] When the analysis includes *only*

Table 1. Black Concentration in 1940 and 1970 and Voting in 1968 for Wallace Among Southern Whites at the County and State Levels

Vote for Wallace with	County Level	State Level
Simple Correlations		
1940 black concentration	.38	.42
1970 black concentration	.33	.33
Partial Correlations		
1940 controlling for 1970 black concentration	.20	.28
1970 controlling for 1940 black concentration	.001	–.06

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[13] H. D. Price, *The Negro and Southern Politics: A Chapter of Florida History* (Westport, Conn.: Greenwood Press, 1957), pp. 35–54. For a discussion of the political socialization literature on the effects of early learning for later adult attitudes and behavior, see Donald S. Searing, Gerald C. Wright, Jr. and George B. Rabinowitz, "The Primacy Principle: Attitude Change and Political Socialization," *British Journal of Political Science*, 6 (January, 1976), 83–113.

[14] Matthews and Prothro, "Social and Economic Factors," p. 28.

[15] Repeating the analysis that follows using the 1970 percentage black data does not affect the conclusion reached. The only difference is a consistent pattern of lower contextual effects. Also state percentage black, measured at each decennial census from 1900 through 1970, was correlated with white support for Wallace in 1968. The 1940 data yielded the highest of these correlations.

[16] Wasserman and Segal, "Aggregation Effects," p. 179.

[17] Two regions are used in the present study. Deep South states include: Alabama, Georgia, Louisiana, Mississippi, and South Carolina. The Marginal South (called the Peripheral South in CSEP publications) includes: Arkansas, Florida, Kentucky, Maryland, North Carolina, Oklahoma, Tennessee, Texas, Virginia, West Virginia, and Washington, D.C. For a discussion

white voters, however, the black concentration hypothesis is supported with positive correlations in both the Marginal South (r=.315) and in the Deep South (r=.188).

This pattern is also reflected in the simple regression coefficients which are more appropriate for comparing relationships across populations.[18] The slope of voting for Wallace on local black concentration is stronger in the Marginal South (b=.984) than in the Deep South (b=.547).[19]

These data solve one problem, and at the same time they create another. The problem solved is the inconsistency in support for the black concentration hypothesis: whites' support for Wallace in 1968 varied as a positive function of the proportion black in the local social environment. The problem created is the difficulty of explaining why white support for Wallace in the Marginal South is more responsive to differences in black concentration than is the case in the Deep South.

If these differences are significant, then there is a need to determine what it is about the regions that make them different. The position taken here is that "region" *per se* is not an explanatory variable but rather that it is a global concept, the components of which must be specified to obtain a firm understanding of the behavioral processes at work. The next task, therefore, is to assess the importance of the differences between the Marginal South and the Deep South in overall levels of support for Wallace and in their relative responsiveness to differences in local black concentration.

To assess the differences between the regions, and to facilitate the remaining analysis, a somewhat more formalized data analysis strategy will be used. The approach consists of constructing a series of "models" with each model representing an explicit set of theoretical assumptions about the nature of contextual influences on support for Wallace.[20] Alternative models are evaluated by means of tests for the significance of differences in the relative explanatory power of models. Two sets of models will be evaluated, the first set shows that there are significant regional differences in the relationship between white support for Wallace and local black concentration, and the second set of models determines whether these regional differences are a function of contextual effects at the state level.

Model I has only one explanatory variable – local black concentration. This is compared to Model II, which has local black concentration and a dummy variable for the effects of region, i.e., Deep South versus Marginal South. We then move to Model III, which allows the slopes of Wallace voting regressed on local black concentration to vary within each of the regions. The assumptions and equations for each of these models may be expressed as follows:

Model I Assumption: Wallace support is a linear function of local black concentration.

$$W = a_1 + b_{11}C + e_1$$

Model II Assumptions: Wallace support is a linear function of local black concentration with additive regional differences.

$$W = a_2 + b_{21}C + b_{22}R + e_2$$

Model III Assumptions: Wallace support is linearly related to local black concentration differently within the Deep South and Marginal South.

$$W = a_3 + b_{31}C_D + b_{32}C_M + b_{33}R + e_3,$$

where W = 1 if voted for Wallace, 0 otherwise;

C = proportion black at county level for all voters;

C_D = proportion black at county level for Deep South voters, 0 otherwise;

C_M = proportion black at county level for Marginal South voters, 0 otherwise;

R = 1 if in Deep South, 0 otherwise.

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of the reasons underlying this division, see Merle Black, David M. Kovenock and William C. Reynolds, *Political Attitudes in the Nation and the States* (Chapel Hill, N.C.: Institute for Research in the Social Sciences, 1974), pp. 6–8.

[18] Hubert M. Blalock, Jr., "Causal Inferences, Closed Populations and Measures of Association," *American Political Science Review*, 61 (March, 1967), 130–136; Gerald C. Wright, Jr., "Linear Models for Evaluating Conditional Relationships," *American Journal of Political Science*, 20 (May, 1976), 349–373.

[19] The slopes here, with a dichotomous (1,0) dependent dummy variable measuring voting for and against Wallace, may be interpreted as the average change in percentage voting for Wallace that would be expected from a change of one per cent in the local percentage black. Alternatively, the regression coefficients may be interpreted as the average difference in the probabilities of voting for Wallace between whites living in all-white areas and whites living in all-black areas. The latter interpretation is sometimes not quite as desirable because the estimated coefficients can imply probabilities greater than 1.0 or less than zero.

[20] Joe H. Ward, Jr. and Earl Jennings, *Introduction to Linear Models* (Englewood Cliffs, N.J.: 1973). For an exposition of the regression techniques employed here using political science examples see Gerald C. Wright, Jr., "Linear Models for Evaluating Conditional Relationships."

If there are important regional differences in the responsiveness of white support for Wallace to local black concentration then Model III should provide a significantly better fit to the data than either of the simpler models, I or II.[21] In Table 2, the results of this first set of models are summarized.[22]

The simplest model, Model I, has just one regression line for county black concentration.[23] Conceptually, Model II yields two parallel regression lines, one for each region. Table 2 shows that Deep South whites had a .193 greater probability of voting for Wallace than Marginal South whites did, and this difference, which is statistically significant, occurs with local black concentration controlled.

Model III allows those regression lines for each region to vary. The estimated parameters for Model III indicate the same substantial differences in the slopes of local black concentration between the Deep South and the Marginal South as noted above. In the Deep South white support for Wallace appears a good deal less responsive to differences in local black concentration (.547) than does Wallace support in the Marginal South (.984). Beyond just different slopes, the coefficient of the region variable increases from .193 in Model II to .318 in Model III. Hence the regression line for the Marginal South appears steeper but lower within the observed range than that for the Deep South as depicted in Figure 1. The F-test for the difference between the estimated models suggests that difference in the slopes and effects of region in Model III are not due to chance.

Table 2. Three Models Relating Local Black Concentration to Support for Wallace and Tests for the Significance of Differences Between Models

Model	Regression Coefficient	Multiple *R*	Significance of Difference Between Models
Model I: Intercept (a_1)	.069	.3821	
Overall Slope (b_{11})	1.075		Model I versus Model II $F_{1,1289} = 40.54$ $p < .01$
Model II: Intercept (a_2)	.074		
Overall Slope (b_{21})	.821	.4147	
Region Slope (b_{22})	.193		Model II versus Model III $F_{1,1288} = 6.72$ $p < .01$
Model III: Intercept (a_3)	.046		
Deep South Slope (b_{31})	.547	.4199	
Marginal South Slope (b_{32})	.984		
Region Slope (b_{33})	.318		

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[21] The criterion here for comparison of models will be whether a difference in their explained variance is statistically significant. The strategy followed is to first postulate a "full" model with K parameters to be estimated. Restrictions are then put on these parameters (i.e., assuming one or more is equal to zero, or that two or more parameters are equal to each other). These restrictions are incorporated into a "restricted" model with L parameters (where $K > L$). The test for the difference in models is explained in Ward and Jennings, and Wright, *ibid.*

[22] The use of dichotomous dependent variables in multiple regression analysis is a matter of some controversy. The regression mode of analysis is chosen for its familiarity and for the nice substantive interpretation of the regression coefficients. Each of our models was estimated using multiple discriminant analysis and the results in terms of models retained, the relative importance of variables, and the significance of differences between models are identical throughout. See George W. Ladd, "Linear Probability Functions and Discriminant Functions," *Econometrica,* 34 (October, 1966), 873–885; Fred Kort, "Regression Analysis and Discriminant Analysis: An Application of R. A. Fisher's Theorem to Data in Political Science," *American Political Science Review,* 67 (June, 1973), 555–559. Peter H. Lemieux, "Communications," and Fred Kort, "Communications," *American Political Science Review,* 68 (March, 1974), 202–205.

[23] The data presented in Table 2 and throughout the remaining analysis are derived from linear correlation and regression (e.g., $W = a + b_1C + e$, where W is vote for Wallace and C is proportion black in the county; a and b_1 are to be estimated). Two nonlinear forms are hypothesized by Hubert Blalock, *Toward a Theory of Minority Group Relations* (New York: John Wiley and Sons, 1967), pp. 143–186. The first is based on a theory of economic competition and posits a positive monotonically *decreasing* slope between racial discrimination and percentage black (i.e., $W = a + b_1 \log C$); the second is based on a theory of political threat and predicts a positive monotonically *increasing* slope (i.e., $W = a + b(C/1-C)$). Both of these models were tested and found to yield no higher correlations than did the simpler linear model.

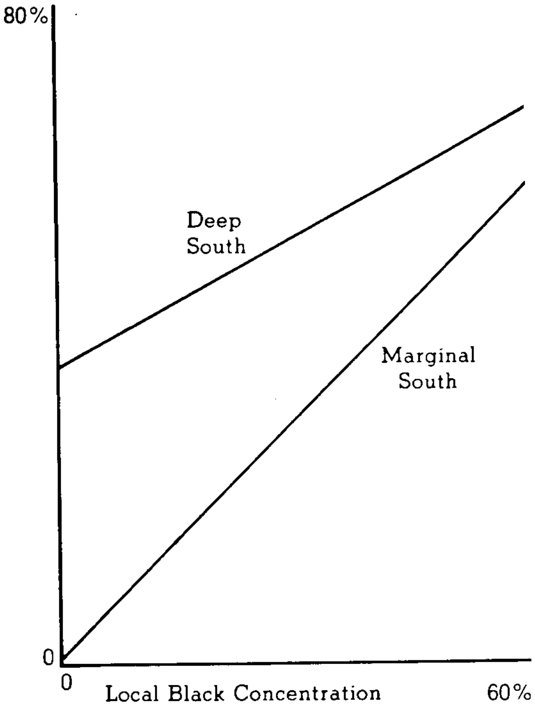

Figure 1. Model III: Relationship of White Voting for Wallace to Local Black Concentration in the Deep South and the Marginal South

State Black Concentration

The purpose of this section is to explain the regional differences indicated in Model III. The explanation for the greater slope of local black concentration in the Marginal South and for the size of the regional effect is to be found in acknowledging that competition between the races can be perceived at the state as well as the community level. State political systems may well be influenced by the proportions of blacks at that level, just as smaller social and political systems are believed to be sensitive to differences in the size of the black population at lower levels. This is suggested by Key in his discussion of the impact of the Negro on southern politics. "If the whites of the black belts give the South its dominant political tone, the character of the politics of individual states will vary roughly with the Negro proportion of the population."[24] A probable mechanism for state contextual influence is exposure to the levels of racism displayed in the election campaigns and leadership of statewide candidates and office holders. Earl Black presents data showing that from 1950 through 1969 64 per cent of the Deep South governors' campaign stands on racial segregation were "militant segregationist" whereas during the same period only 24 per cent of what he calls the Peripheral South's gubernatorial campaign stances were so classified.[25] Thus, many whites residing in communities with few blacks nevertheless have had substantial exposure to a segregationist political culture – transmitted through race as a statewide political concern. Since the Deep South states generally have had larger black populations than the Marginal South states, we might explain the regional effect noted in Models II and III in terms of differences in the relative sizes of the black populations at the state level.

To test fully the arguments concerning the importance of the state contextual factor, as well as to test for spuriousness in the Wallace voting/black concentration relationship, another series of models incorporating additional assumptions is compared. The symbols for the equations expressing these models, besides those defined above, are as follows:

S = 1940 proportion nonwhite in state of residence;

U = urbanization, 1 if the county is in a Standard Metropolitan Statistical Area (SMSA), 0 otherwise;

E = education, 1 if the respondent is a high school graduate, 0 otherwise;

A = Wallace home state advantage, 1 if the person was living in Alabama, 0 otherwise.

The next model adds the proportion black at the state level as well as including all the variables in Model III.

Model IV Assumptions: Wallace support is a function of local black concentration differentially related within each region, state black concentration, and region.

$$W = a_4 + b_{41}C_D + b_{42}C_M + b_{43}S + b_{44}R + e_4$$

The results in Table 3 strikingly support our argument for the importance of an independent contextual effect at the state level. The data reveal that including the state contextual variable in Model IV significantly adds to the explanatory power of Model III.

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[24] Key, *Southern Politics,* p. 5; Blalock, *Toward a Theory of Minority Group Relations,* pp. 174–175.

[25] Earl Black, "Southern Governors and Political Change: Campaign Stances on Racial Segregation and Economic Development," *Journal of Politics,* 33 (August, 1971), p. 713.

Model IV also solves both of the "problems" of Model III. First, the addition of state proportion black in Model IV virtually equalizes the local black concentration slopes of the Deep South (.546) and the Marginal South (.605) and thereby solves that explanatory problem of unequal slopes noted in Model III. Second, the large region effect in Model III of .318 is reduced to just .064 in Model IV. Hence, the inclusion of state black concentration appears to have explained most of the differences between the Deep South and the Marginal South in the relationship between white support for Wallace and local black concentration. This is tested by examining Model V, which removes the region variable from the analysis and forces a single local black concentration slope for both regions.

Model V Assumptions: Wallace support is a linear function of local and state black concentration.

$$W = a_5 + b_{51}C + b_{52}S + e_5$$

Excluding region and forcing a common slope does not result in any significant loss of explanatory power once state black concentration is in the model. Table 3 shows that the difference between Models IV and V does not approach statistical significance, thus indicating that there are no remaining significant regional differences in white support for Wallace. Although the Deep South did give higher support to Wallace, this can be explained in terms of white responses to black concentration at the local and particularly state levels. Thus, rather than relying on the nebulous concept of "region" to account for Wallace support, contextual factors operating at two levels are found to completely account for its effects. Moreover, as can readily be seen in the relative sizes of the regression coefficients, white support for Wallace was substantially more responsive to differences in black concentration at the state level than at the local level, although the latter does make an important independent contribution.

Before concluding that the black concentration hypothesis is strongly supported, let us briefly examine three possible sources of spuriousness. First, there is the "populism" of the Wallace candidacy with its presumed nonurban bases of support.[26] Differences in urbanization of the voters' environments might account for the association between Wallace voting and black concentration. The measure of urbanization used here is whether or not the voter's county of residence was in a Standard Metropolitan Statistical Area (SMSA). This measure produces a higher simple correlation with vote for Wallace than does either the CSEP size-of-place classification or the Census percentage urban in the county.

Second, better-educated voters have been reported to be less likely to have voted for Wallace in 1968.[27] Since education is likely to

Table 3. Three Models Relating State and Local Black Concentration to Support for Wallace and Tests of Significance Among the Models

Model	Regression Coefficient	Multiple R	Significance of Difference Between Models
Model IV: Intercept (a_4)	−.091		Model III versus Model IV
Deep South Local Slope (b_{41})	.546		$F_{1,1287} = 45.09$ $p < .01$
Marginal South Local Slope (b_{42})	.605	.4519	
Overall State Slope (b_{43})	1.053		
Region Slope (b_{44})	.064		Model IV versus Model V
Model V: Intercept (a_5)	−.107		$F_{2,1287} = .866$ not signif.
Overall Local Slope (b_{51})	.595	.4507	
Overall State Slope (b_{52})	1.173		Model V versus Model VI
Model VI: Intercept (a_6)	.092		$F_{3,1286} = 19.75$ $p < .01$
Overall Local Slope (b_{61})	.535		
Overall State Slope (b_{62})	1.031	.4881	
Urbanization Slope (b_{63})	−.085		
Education Slope (b_{64})	−.078		
Alabama Slope (b_{65})	.282		

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[26] Wrinkle and Polincard, "Populism and Dissent," and the literature reviewed therein.

[27] Irving Crespi, "Structural Sources of the George Wallace Constituency," *Social Science Quarterly*, 52 (June, 1971), 124–125; Seymour Martin Lipset and Earl Rabb, "The Wallace Whitelash," *Transaction*, 7 (December, 1969), 23–25.

be related to black concentration, the respondent's education (whether or not he graduated from high school) is controlled to determine its effects on the slopes for black concentration.

Third, Wallace has long had very strong support in his home state of Alabama, and because he was listed there, but not in other states, as the candidate of the Democratic party, the inclusion of Alabama with the other states may have substantially influenced the results above. The possibility of substantial spuriousness in the relationship between black concentration and Wallace voting due to Wallace's probable advantage in his home state is checked through adding a dummy variable for whether a voter lived in Alabama versus another southern state.[28]

The effects of these factors are estimated with the following expanded model.

> Model VI Assumptions: Wallace support is a linear function of black concentration at the state and local levels, urbanization, the voter's education, and Wallace's home state advantage.

$$W = a_6 + b_{61}C + b_{62}S + b_{63}U + b_{64}E + b_{65}A + e_6$$

Table 3 shows that, not unexpectedly, urbanization and education are negatively related to voting for Wallace while residing in Alabama shows a stronger positive relationship. Model VI does explain significantly more variance than Model V, and additional tests (data not shown) demonstrate that each of the added variables has a significant independent effect. The important point for the present study is to notice that the respective slopes for state and local black concentration are hardly affected. The effects of black concentration, both at the state level and at the county level are almost totally independent of differences in urbanization, respondent's education, and Wallace's home-state advantage. The final model for the overall influence of proportion black on voting for Wallace could be Model V, or if a more inclusive one is desired, it could be Model VI.[29]

Primary Groups and Issue Proximity

The models just discussed, while not invalid, fail to show an important interpretive linkage. Conditions in one's environment do not influence behavior directly, but rather indirectly through social processes and attitudes. There are two likely mechanisms linking contextual factors to voting behavior. The first is peer-group support for Wallace. James Coleman has argued that contextual effects imply social relational models; context influences behavior through processes of interpersonal influence.[30] Coleman's view is substantiated in work reported by Putnam showing that partisanship of the environment has the greatest impact on those individuals who are active in social organizations, and furthermore, that all of such contextual influence is transmitted through primary group preferences.[31] Hence in this view, contextual characteristics operate by influencing the types of people with whom one is likely to interact.

A related, but conceptually different, interpretation of contextual influences sees context as affecting one's attitudes and values. This perspective is implicit in most discussions of the linkage between black concentration and white support for Wallace: increases in the relative size of the black population are believed to lead to greater racial hostility among whites which in turn explains their higher levels of Wallace voting. This is simply an instance of the more general position that contextual differences impact on issue attitudes and through them on voting choice.

Both of these linkages, primary group support for Wallace and issue preferences, will now be examined as intervening mechanisms linking black concentration (and the other variables of Model VI) with voting choice. The measure of primary group support which was developed by the Comparative State Elections Project, taps the support for Wallace in the primary group environment relative to the support for Humphrey and Nixon. Support from three types of groups are incorporated in this index: the respondent's friends, his fellow workers (if currently employed), and his spouse or family, depending on whether or not the respondent was married.[32] The role of issues as intervening between context and voting choice is assessed through a composite measure of relative proximity to Wallace on five issues which include items on open housing, controlling riots and violence, better jobs and wages, taking a stronger stand in Vietnam, and presidential personality.[33] Relative issue proximity seeks to measure how close to Wallace the voter saw himself on these five issues as compared to how close he saw himself to Nixon and Humphrey on the same issues. Each of the issue proximities is given equal weight.

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[28] The impact of Wallace's home state advantage was also checked in two additional ways. First, we began by regressing Wallace voting on the Alabama dummy variable and then added the variables of Models I through VI, as outlined above. Beginning with "Alabama" in the equation did not influence the results presented above in any notable way. Second, the analysis was repeated with Alabama voters completely excluded; the results were not changed.

Models I–VI were also re-run with Republicans and Independent leaning toward the Republican party excluded to determine if the larger percentage of Republicans in the Marginal South could be influencing the regional differences. The results, in all important respects, again remained unchanged.

[29] The interaction between the state and local contextual effects was checked and the addition of a multiplicative interaction term does not add significantly to Model VI.

[30] James S. Coleman, "Relational Analysis: The Study of Social Organization with Survey Methods," *Human Organization,* 17 (Winter, 1958–1959), 28–36.

[31] Robert D. Putnam, "Political Attitudes and the Local Community," *American Political Science Review,* 60 (September, 1966), 640–654.

The independent variables of Model VI are expected to operate on voting choice through their effects on primary group support and issue proximity to Wallace as depicted in Figure 2. If this intervening model is correct, then the following should be observed: (1) the independent variables of state and local black concentration, education, urbanization, and residence in Wallace's home state of Alabama will have substantial effects on relative primary group support for Wallace and relative issue proximity to Wallace; (2) with relative primary group support and relative issue proximity controlled, the independent variables will have no direct influence on voting choice; and (3) primary group support and issue proximity will have substantial direct effects on voting choice.[34] In short, the independent variables of Model VI

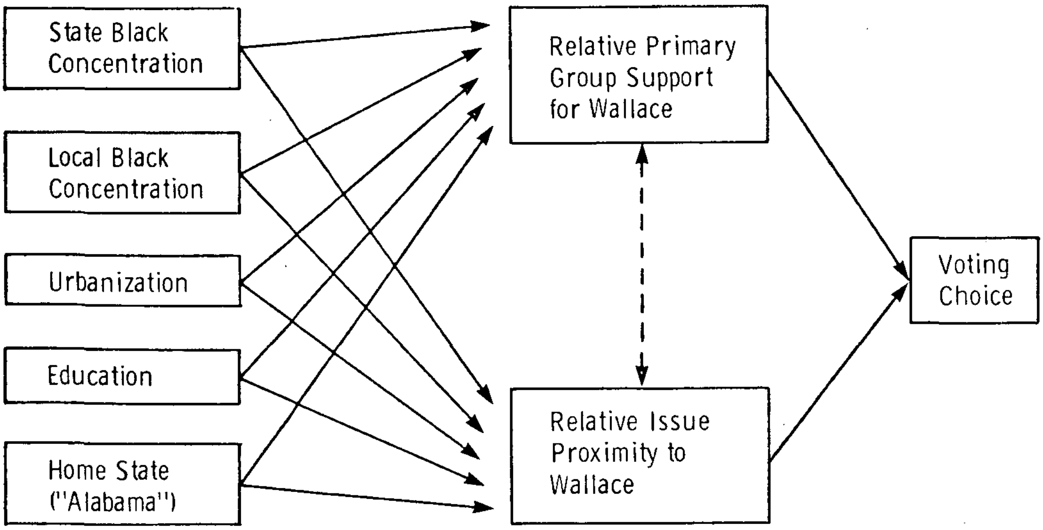
State Black Concentration
Local Black Concentration
Urbanization
Education
Home State ("Alabama")
Relative Primary Group Support for Wallace
Relative Issue Proximity to Wallace
Voting Choice

Figure 2. Relative Primary Group Support and Relative Issue Proximity as Intervening Variables[35]

are expected to influence relative primary group support and relative issue proximity, and these in turn to influence voting choice.

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[32] For a complete discussion of the procedures for constructing the relative primary group support index see Philip Beardsley, "The Methodology of Electoral Analysis," in *Explaining the Vote*, David M. Kovenock, James W. Prothro and Associates (Chapel Hill, N.C.: Institute for Research in Social Science, 1973), Part I, pp. 53–55.

[33] Beardsley, "The Methodology of Electoral Analysis" provides a rather complete discussion of the construction of relative issue proximity measures. A more thorough description and analysis of the particular items used here to construct the measure of relative issue proximity is contained in Gerald C. Wright, Jr., "Community Structure and Voting in the South," *Public Opinion Quarterly*, 40 (Summer, 1976), 201–215.

[34] These three expectations follow from path analysis definitions of intervening models. For example, see Hubert M. Blalock, Jr., "Theory Building and Causal Inferences," *Methodology in Social Research*, ed. H. M. Blalock, Jr. and A. M. Blalock (New York: McGraw-Hill, 1968), pp. 175–176; Kenneth C. Land, "Principles of Path Analysis," in Edgar F. Borgatta (ed.), *Sociological Methodology 1969* (San Francisco: Jossey-Bass, 1968), pp. 29–33.

[35] The dotted line in Figure 2 indicates an expected relationship, but it is one that will not be explored here. Fortunately, for our purposes, it is not necessary to disentangle the effects of primary group support and issue proximity on one another; it will be quite sufficient if together these variables are found to be major mechanisms intervening between contextual factors and voting choice.

The data necessary to test the adequacy of an intervening model are presented in Table 4. Because comparisons are not being made across populations – Deep South versus Marginal South – standardized partial regression coefficients, which are most useful for evaluating relative explanatory power, are now most appropriate. Overall the data of Table 4 provide striking support for the argument that primary group support and issue proximity are the mechanisms intervening between the five independent variables of Model VI on the one hand and voting choice on the other.

The first two columns of the table show that each of the independent variables has an independent influence on both primary group support and on issue proximity. Notice that the relative support for the social relational and issue interpretations of contextual effects appear to depend on which contextual unit is examined: local black concentration has a greater impact on issue proximity, while state black concentration has a greater effect on relative primary group support. The important point, however, is that both hypothesized intervening variables are quite responsive to the five independent variables.

Further support for the intervening model is found in comparing the magnitude of the coefficients for the five independent variables in columns three and four of Table 4. In both columns, voting for or against Wallace is the dependent variable, but without the intervening variables in column three and with them in the equation in column four. Column three shows the standardized version of the coefficients from Model VI; column four shows that these all drop very substantially, or even change signs, with the addition of primary group support and issue proximity. Hence there are no major direct effects from the independent variables once the intervening variables are held constant.

The final evidence for the intervening model is the very substantial impact of relative primary group support and relative issue proximity on voting choice when they are added to the model in column four. While the direct explanatory contribution of primary group support is greater, each of these variables has a substantial and significant direct influence on voting choice. Moreover, their incorporation in the model increases the multiple *R* from .48 to .82 – a rather hefty increment.

The data presented in Table 4, therefore, provide a sound basis for the position that context does not influence behavior directly but does so through intervening social and attitudinal processes. The measure of relative primary group support for Wallace and relative issue proximity to Wallace appear to tap rather nicely these respective processes, indicating very strong support for the intervening model.

Summary

V. O. Key's black concentration hypothesis has received considerable attention from social scientists, but for the George Wallace candidacy for president in 1968 the hypothesis has not been consistently supported in a number of studies. Correcting certain problems in the methodology of previous studies and taking a contextual rather than simple aggregate data approach led to the first conclusion of the present study: the data presented give firm and unambiguous support to the black concentration hypothesis. In 1968, southern white voting for Wallace for president increased with the relative size of the black population.

Table 4. Issue Proximity and Primary Group Support as Intervening Variables Between Contextual Factors and Voting Choice[a]

Independent Variable	Issue Proximity	Primary Group Support	Dependent Variable Wallace Vote (Without Issues & Primary Group)	Wallace Vote (With Issues) & Primary Group)
Local black concentration	.201	.141	.180	.040
State black concentration	.169	.364	.259	–.014
Urbanism	–.136	–.164	–.086	.052
Education	–.098	–.063	–.082	–.018
Alabama	.118	.158	.140	.001
Primary Group support	––	––	––	.635
Issue proximity	––	––	––	.251
Multiple *R*	.436	.578	.478	.821

[a] Values in the table are standardized partial regression coefficients.

Second, there is some indication that attitudes may lag considerably behind changes in the objective social environment. The last thirty years have seen very substantial rural-to-urban and South-to-North Negro migration, thus changing the racial composition of many southern communities. Nevertheless, measures of black concentration using the 1940 census for both county and state percentage nonwhite are more closely related to 1968 Wallace voting than the more proximate 1970 data. This finding gives credibility to the assumption made in much of the literature about how important early political socialization is for later adult political behavior. It also suggests – although very tenuously – that racial attitudes may respond rather slowly to changes in the context of racial relations.

Third, race has a contextual effect at the state as well as at the community level. The Deep South states, with their relatively large nonwhite populations, gave higher support to Wallace, even when differences in local black concentration are controlled. These regional differences are completely explained when differences in the black concentration at the state level are included in the analysis. This responsiveness to statewide contextual conditions was interpreted as a function of differences in the types of appeals, campaigns, and political leadership voters have had in the various southern states. The finding of independent contextual effects at more than one level indicates the need for greater attention to the level and meaning of the social and political units employed in contextual analyses.

A fourth point worth noting is the technique of positing hypotheses as alternative models and then empirically testing the differences in the models in a systematic search for parsimonious and theoretically meaningful explanations. Particularly useful is the flexibility of the general linear model approach to data analysis in which different assumptions are tested in models applying to all observations, rather than being estimated separately for different groups of subjects. In the present study, expressing differences between the Deep South and Marginal South in separate operations does not easily permit an assessment of the importance of the differences which may be observed. Expressing the Deep and Marginal South differences as parameters of a single equation (Model III) permitted assessing the importance of these regional differences in subsequent models (IV–VI). Of special substantive significance in this regard is the satisfying explanation of "region" effects in terms of black concentration at the state level.

Fifth, the study finds primary group processes and issue proximities are major mechanisms through which black concentration – and our other independent variables – influence voting choice in the South.

Finally, this study has illustrated the utility of combining multiple data files in a single analysis. Such combined data files are crucial when, as in the present case, one wishes to test hypotheses asserting that contextual factors at one level influence attitudes and behavior occurring at another level. Although a bit tedious, the cost of constructing such combined data files is minuscule compared to the costs of collecting the original data sets. The integration of aggregate or contextual data with individual-level survey data is a most promising means of bridging the gap – and hence resolving many of the substantive inconsistencies–between ecological and survey data studies.

The Stability and Reliability of Political Efficacy: Using Path Analysis to Test Alternative Models*

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Political efficacy is an important concept in the analysis of American political behavior, having been used to explain a wide variety of political activities and attitudes.[1] Most of those who use the concept have assumed that efficacy is a relatively deep-seated, stable orientation rather than a superficial and transient attitude.[2] Recently, however, doubt has been cast on the stability of one commonly applied measure of efficacy – the SRC four-item index.[3] Cross-tabular and correlational analysis have indicated that over a period of months an individual's

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*The data for this paper were provided by the Inter-University Consortium for Political Research. Neither the Consortium nor the original collectors of the data are responsible for the analysis and conclusions presented here.

[1] Most observers agree about the general nature of political efficacy. It is a feeling that individual political action does have an impact upon the political process. See Angus Campbell, Gerald Gurin, and Warren Miller, *The Voter Decides* (Evanston, Illinois: Row, Peterson, 1954), p. 187. The authors of the *American Voter* used the concept to explain voting rates, but efficacy has been used since to try to explain a wide variety of kinds of political participation. Operationalizations have been diverse, but the four items developed by the Survey Research Center that are examined here are certainly the most widely used. See also Angus Campbell, Philip E. Converse, Warren Miller and Donald Stokes, *The American Voter* (New York: John Wiley and Sons, 1964); Donald Matthews and James Prothro, *Negroes and the New Southern Politics* (New York: Harcourt Brace and World, 1966); Gabriel Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963); Jeffery M. Paige, "Political Orientation and Riot Participation," *American Sociological Review* 36 (October, 1971), 810–820. For an extensive bibliography on political efficacy see David Easton and Jack Dennis, "The Child's Acquisition of Regime Norms: Political Efficacy," *American Political Science Review,* 61 (March, 1967), 25–38.

[2] Campbell et al., *The American Voter,* p. 516. See also Robert E. Lane, *Political Life* (New York: The Free Press, 1959), pp. 149–151. The child socialization literature also contributes to this belief by suggesting that attitudes about efficacy formed in childhood carry over into adult life. See Easton and Dennis, p. 33–38; and also Kenneth Langton and M. Kent Jennings, "Political Socialization and the High School Civics Curriculum in the United States," *American Political Science Review,* 62 (September, 1968), 852–867. Some of the assumptions of this kind of socialization research have been challenged on grounds relevant to the problems under consideration here. Donald D. Searing, Joel J. Schwartz and Alden E. Lind show, for example, that attitudes of political efficacy as well as other "political orientations" have hardly any relationship at all to issue beliefs in adults. "The Structuring Principle: Political Socialization and Belief Systems," *American Political Science Review,* 67 (June, 1973), 415–432.

[3] Converse found that three of the four items in the SRC efficacy scale significantly increased during the 1950s then significantly decreased in the 1960s, while the other showed a continuous increase between 1952 and 1968. More detailed analysis indicated that these aggregate statistics substantially *understate* the instability of efficacy feelings because individual changes cancelled each other out to a significant degree. See Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change,* ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), pp. 327–332.

Using data from an SRC panel study, Asher found that for the 1956–1960 period, when only very marginal changes occurred in the aggregate efficacy scores, correlations between an individual's response to each question at the beginning and end of the study showed much greater inconsistencies as their temporal correlations averaged only .36. Even for the short period between the pre- and postelection survey of 1968 the temporal stability of efficacy feelings is hardly greater than for the four-year span. See Herbert B. Asher, "The Reliability of the Political Efficacy Items," *Political Methodology,* 1 (Spring, 1974), 45–72. Welch and Clark, investigating the relationships among the efficacy items and their changes over time, found a multidimensionality as well as substantial instability. Stability was only slightly greater for scores on separate dimensions as for individual items. See Susan Welch and Cal Clark, "Political Efficacy as an Underlying Political Orientation: The Problem of Attitudinal Instability" (paper presented at the Midwest Political Science Association Meeting, April, 1974).

The high temporal variability of the efficacy items is underlined by a comparison with other political attitudinal sets and opinions about specific political level of efficacy is fairly unstable. The cross-time correlations of individual items fall between .30 and .44, while the total index is correlated with itself at approximately .50.[4] Furthermore, there is a lack of consensus about the dimensionality of the four efficacy items. Some have argued that the four items comprise two discrete two-item subscales distinguishing "subjective competence" from "governmental responsiveness."[5] Others have found that three of the four items in the SRC index cluster together reasonably well to form a single subscale.[6] The discussion of the dimensionality of the component efficacy items raises the question of the reliability of each item in measuring the concept "efficacy"; the observed lack of stability casts some doubt on the validity of the efficacy construct.

For a variety of reasons, the arguments about the stability and reliability of efficacy have not been particularly fruitful. Different methodologies have yielded different results. Some believe that arguing about the stability and reliability of the SRC efficacy items is like arguing over the number of angels dancing on the heads of pins. On the other hand, serious questions have been raised about one of the important and widely used concepts in political behavior. Questions raised about efficacy are, furthermore, probably applicable to most concepts used in the study of political behavior. Evaluating the stability and reliability of the most common measure of efficacy more accurately should have consequences for researchers using indices not only of efficacy but of other political attitudes as well.

The difficulties in evaluating and extending research on stability and reliability of efficacy lie partly with the methods used to examine these characteristics. In addition to examining change in aggregate responses to individual items, simple over time correlations have been most widely used to examine stability.[7] Aside from the question of what constitutes a "high" over time correlation, there exists the more basic problem that relatively low over time correlations may be to causes other than stability. Simple cross-time correlations may be diminished by random or systematic measurement error, or other sources of unreliability. None of these possibilities, however, reflect on the true stability of the concept. Real change (instability) in individual efficacy may also depress the over-time correlation, but it, in turn, does not reflect inadequacy of the measures themselves. Neither correlational nor cross tabular analysis is adequate to measure stability, since in interpreting low over-time associations, one cannot untangle the effects of item unreliability and measurement error from true change.

The measurement of the reliability of the items in the efficacy scale has been approached in at least three ways: external validation (i.e., comparing the variables's relationships with other items),[8] factor analysis,[9] and analysis of differences in over-time change in aggregate responses to each of the four items.[10] External validation of items is, of course, a useful approach, but it cannot give precise measures of reliability and unidimensionality. The same holds for the analysis of aggregate response to the items. One item may move in a direction independent of the other items, but this gives no measure of reliability. Factor analysis does give an indication of dimensionality, but traditional factor analysis is subject to the same limitations as simple correlation analysis with respect to measurement error. What is needed, then, is a method of separating reliability of the items from their over-time stability, and estimating the extent of measurement error in index items.

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issues. One would expect that deep-seated orientations should be more stable than positions on political issues. However, responses to the four efficacy items were on the average less consistent between 1956 and 1960 (Pearson's r averaging from .30 to .42) than attitudes toward a range of current issues (Pearson's r between .20 and .53) and much more variable than such a basic orientation as party identification (r = .90).

[4] Welch and Clark.

[5] Converse, "Change in the American Electorate;" Balch calls the dimensions "Internal" and "External" efficacy, as cited in George Balch, "Multiple Indicators in Survey Research: The Concept 'Sense of Political Efficacy,' " *Political Methodology*, 1 (Spring, 1974), 1–43.

[6] Welch and Clark, "Political Efficacy," factor analyzed the four conventional efficacy items along with four questions about the importance of voting and found that the three items which Converse retained defined one of the two factors that emerged. These three variables also tended to have similar correlations with several indicators of political participation that were somewhat stronger than the ones with "voting only political influence," but all four had approximately the same relationships to the political trust and cynicism questions.

[7] Welch and Clark; Asher, "Reliability of the Political Efficacy Items."

[8] Balch, "Multiple Indicators."

[9] Welch and Clark.

[10] Converse, "Change in the American Electorate."

Techniques for handling this problem by path analysis have been developed in the sociological literature within the past five years, but with one exception these methods have not been used in political science.[11] And, the application of these techniques to substantive problems has been rare. Generally, the technique allows the separation of construct stability and indicator reliability in over-time analysis; specification of nonrandom error is also possible.

First, we shall outline two forms of the causal approach to separating stability and reliability. The earlier form of the approach can be performed by hand for simple models. The more sophisticated covariance structure approach gives efficient estimates and allows one to work with a large number of items, but requires a computer. Second, we will show how this approach can give answers to important questions concerning the measurement of political efficacy. Our analysis will have both substantive and methodological interest; many variables used in political science have indicators similar to those sketched above, so that path analysis may be useful in other substantive problems.

The Causal Approach to Measurement Error

In simple path analysis, regression coefficients are used to describe the relationships among variables in a causal model. The basic theorem assumes that the correlation between any two variables is equal to the product of the paths linking the variables. This fact allows the zero order correlation between two variables to be decomposed into direct and indirect effects. Costner shows that path analysis can be used with unmeasured variables and their indicators to estimate item reliability.[12]

The approach can be explained most easily by reference to Figure 1. Let us assume that X_1 is the "true" efficacy score at time one, X_2 the "true" efficacy score at time two.[13] The actual indicators of efficacy at time one are denoted as x_1 to x_4, and at time two are labeled x_5 to x_8. Paths a through d estimate the relationship of the true variable efficacy to each indicator at time one, and paths f through i are similar estimators for time two. Obviously, if each indicator was a perfect reflection of the "true" efficacy score, paths a through d and f through i (the epistemic coefficients) would be equal to 1. Unreliability caused by measurement error would produce diminished path coefficients a through d and f through i. Thus the observed score is a combination of the true value and the error in measurement, which is assumed to be random.[14] The assumption is made in the model of Figure 1 that error terms are randomly distributed, and uncorrelated with each other and the unmeasured variables either at one point in time or over time.

Path e reflects the assumption of the model that "true" efficacy at time two is caused by "true" efficacy at time one. If efficacy were perfectly stable from time one to time two, path e would be unity. To the extent that path e is less than unity, efficacy at time two is being caused by factors other than efficacy at time one.

The rules of path analysis[15] allow estimates of all paths in the model of Figure 1, provided

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[11] See H. M. Blalock, Jr., "A Causal Approach to Nonrandom Measurement Error," *American Political Science Review*, 64 (December, 1970), 1099–1111. The major discussions of the use of causal modeling for this purpose include Herbert L. Costner, "Theory, Deduction, and the Rules of Correspondence," *American Journal of Sociology*, 75 (September, 1969), 245–263; David R. Heise, "Separating Reliability and Stability in Test-Retest Correlations," *American Sociological Review*, 34 (February, 1969), 93–101; David E. Wiley and James A. Wiley, "The Estimation of Measurement Error in Panel Data," *American Sociological Review*, 35 (February, 1970), 112–117; and H. M. Blalock, Jr., "Multiple Indicators and the Causal Approach to Measurement Error," *American Journal of Sociology*, 75 (September, 1969), 264–272.

[12] Costner. Here we will use standardized regression coefficients (beta weights); unstandardized coefficients can also be used. At this point in the paper we make no distinction between sources of error variance in the items due to specification error and sources in error variance due to measurement error.

[13] "True" in this sense meaning an efficacy score that could be perfectly measured. The following discussion is drawn largely from Heise and from Costner.

[14] The assumption that the error terms are uncorrelated is equivalent to the assumption that no major omitted variables cause common variation in the items except through the efficacy construct, or other explicit unmeasured variables. This assertion is the *ceteris paribus* assumption for causal models. It should be emphasized that this method does not deal with the semantic aspects of indicator validity. "True" efficacy may or may not be what most observers mean by efficacy. High reliabilities of the indicators of efficacy with the efficacy construct mean only that the indicators are validated for that particular model; different models might yield different indicator reliabilities.

[15] Sewell Wright, "The Method of Path Coefficients," *Annals of Mathematical Statistics*, 5 (September, 1934), 161–215; O. D. Duncan, "Path Analysis: Sociological Examples," *American Journal of Sociology*, 72 (July, 1966), 1–16.

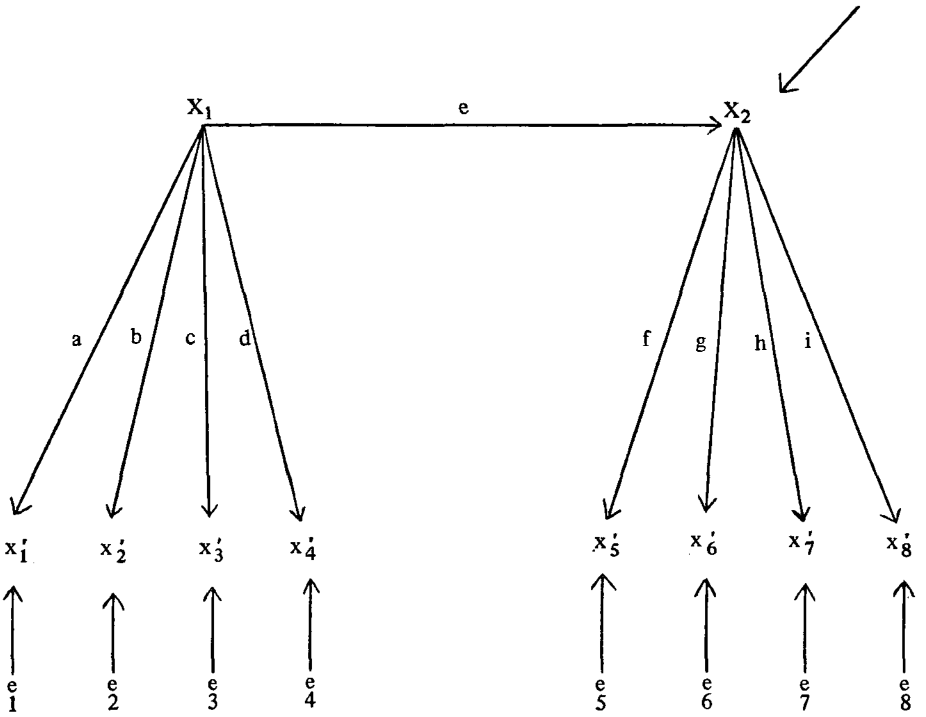

Figure 1. Causal Model for a Four-Item Scale at Two Points in Time

that the number of known correlations exceeds the number of paths to be estimated. In this case we have 28 known correlations ((8 x 8) – 8)/2) from which path estimates can be made. Since we have only 9 unknowns, the model is overidentified,[16] and several estimates can be made for each path. The "Costner" procedure consists of taking the mean of these estimates for the summary estimate. For example, seven equations involving path *a* can be derived:

$r_{x_1'x_2'} = ab$

$r_{x_1'x_3'} = ac$

$r_{x_1'x_4'} = ad$

$r_{x_1'x_5'} = aef$

$r_{x_1'x_6'} = aeg$

$r_{x_1'x_7'} = aeh$

$r_{x_1'x_8'} = aei$

Solving these equations for the paths in terms of the correlations produces many distinct estimates for each path. Two of these derivations are illustrated below:

ESTIMATE 1:

$$b = \frac{r_{x_1'x_2'}}{a}, \quad b = \frac{r_{x_2'x_3'}}{c} \text{ (since } r_{x_2'x_3'} = bc\text{),}$$

$$c = \frac{r_{x_1'x_3'}}{a}, \text{ thus: } b = \frac{r_{x_1'x_2'}}{a} \text{ and } b = \frac{r_{x_2'x_3'}}{r_{x_1'x_3'}/a} =$$

$$\frac{(r_{x_1'x_3'})(a)}{r_{x_1'x_3'}} \quad \frac{r_{x_1'x_2'}}{a} = \frac{(r_{x_2'x_3'})(a)}{r_{x_1'x_3'}}$$

$$a^2 = \frac{(r_{x_1'x_2'})(r_{x_1'x_3'})}{r_{x_2'x_3'}} \text{ and } a = \sqrt{\frac{(r_{x_1'x_2'})(r_{x_1'x_3'})}{r_{x_2'x_3'}}}$$

and ESTIMATE 2:

$$b = r_{x_1'x_2'}/a; \; b = \frac{x_2'x_4'}{d} \text{ (since } r_{x_2'x_4'} = bd\text{);}$$

$$d = \frac{x_1'x_4'}{a}; \text{ thus } b = r_{x_1'x_2'}/a \text{ and}$$

$$b = r_{x_2'x_4'}/r_{x_1'x_4'}/a \quad r_{x_1x_2}/a =$$

$$r_{x_2'x_4'}/r_{x_1'x_4'}/a = \frac{(a)(r_{x_2'x_4'})}{r_{x_1'x_4'}}$$

$$a^2 = \frac{(r_{x_1'x_2'})(r_{x_1'x_4'})}{r_{x_2'x_4'}} \quad a = \sqrt{\frac{(r_{x_1'x_2'})(r_{x_1'x_4'})}{r_{x_2'x_4'}}}$$

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[16] Overidentification in general denotes an excess of known quantities in relation to the number of unknowns to be estimated. See Duncan.

For these examples it is obvious that if the quotient $\frac{\sqrt{x_1'x_4'}}{\sqrt{x_2'x_4'}}$ is unequal to the quotient $\frac{r_{x_1'x_3'}}{r_{x_2'x_3'}}$, the two estimates will be unequal.

Given the abundance of estimates that can be made, three problems arise. First, there is no obvious way to choose among the various estimates computed for each path. Each estimate of a particular path is as "good" as any other. When these estimates disagree, as they almost invariably will, it is very difficult to interpret the results. Thus, it is desirable to have a summary measure for each path, instead of a large number of possibly conflicting estimates. Second, the procedure offers no way to test the model as a whole, since the path coefficients are computed individually; no overall goodness of fit measure is produced. On a more mechanical level, while the calculations involved in this procedure are individually simple, the sheer number of them is overwhelming if the model contains many variables. Furthermore, if the researcher finds that one item appears to be very unreliable, a whole new set of calculations is necessary for a slightly modified model. While this would not be too much trouble for a two- or three-indicator model, recalculating estimates based on larger models would be a major project.

A solution to these problems has been found by Jöreskog and his associates.[17] This solution may be interpreted as a weighted average of the conflicting estimates in the Costner solution. The estimates produced by the Jöreskog procedure give more weight to those Costner estimates with the smallest sampling variability (and covariability).[18] Thus, the Jöreskog procedure averages the Costner estimates in such a way as to produce the most efficient summary estimate. Alternatively, the Jöreskog procedure may be thought of as a confirmatory factor analysis model, in which certain factor loadings are known in advance to equal zero.[19] In essence, the approach selects estimates for each path which most accurately reproduce the correlations among the observed variables.[20] The Jöreskog procedure not only gives unique, efficient estimates of the parameters, but it provides a chi-square statistic which allows hypothesis tests to be made.[21] Finally, the Joreskog procedure has been formalized in a computer program which overcomes the difficulty of calculation mentioned previously.[22] In the next section we will use the causal approach to test the three major hypotheses concerning the efficacy measures.

Hypotheses. A first hypothesis is that the four-item efficacy scale is a highly stable and reliable measure of efficacy. Since much of previous research has shown that the scale is not stable and reliable, this hypothesis can serve as a starting point for the analysis. Positing this assumption, this allows us to examine the whole scale before testing specific hypotheses about the nature of the four items, the complete wording of which may be found in Table 1.

The second hypothesis about the four-item scale is that three of the four items form a relatively cohesive scale, while the fourth item, "voting is the only way people like me can have

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[17] K. G. Jöreskog, "A General Method for the Analysis of Covariance Structures," *Biometrika* 57, #2, (1970), 239–251. The computer program designed to analyze data according to this covariance structure procedure is documented in K. G. Jöreskog, Gunnar T. Gruvaeus and Marielle Van Thillo, *ACOVS: A General Computer Program for the Analysis of Covariance Structures* (Princeton: Educational Testing Service, 1970). A technical appendix outlining the application of the Jöreskog procedure to the four-item two-wave model considered here may be obtained from the authors by request.

[18] See R. M. Hauser and A. S. Goldberger, "The Treatment of Unobservable Variables in Path Analysis," in *Sociological Methodology*, ed. H. L. Costner (San Francisco: Jossey-Bass, 1971), pp. 81–118; for a more technical discussion, see A. S. Goldberger, "Efficient Estimation in Overidentified Models: An Interpretive Analysis" in *Structural Equation Models in the Social Sciences*, ed. A. S. Goldberger and O. D. Duncan (New York: Seminar Press, 1973), pp. 131–152.

[19] Note that this *a priori* knowledge (for instance that certain factor loadings are zero) is what distinguishes confirmatory factor analysis from the more generally known exploratory factor analysis. As its name suggests, confirmatory factor analysis is designed to test hypotheses rather than suggest them.

[20] More precisely, the method minimizes a scalar function of the differences between the variance-covariance matrix of the sample and that matrix produced by the causal structure of the model and the estimates of the parameters. For a more technical discussion, see Jöreskog, "A General Method for the Analysis of Covariance Structures; for extensions of the technique, see K. G. Joreskog, "A General Model for Estimating a Linear Structural Equation System" in Goldberger and Duncan, *Structural Equation Models in the Social Sciences*, pp. 85–107.

[21] The assumptions are essentially those of correlation analysis, including the assumption of multivariate normality. See references cited in the previous footnote.

[22] See Jöreskog et al., *ACOVS*.

Table 1. Correlation Matrix for Eight Items[a]

	1	2	3	4	5	6	7	8
1. No say – 1956[b]	1.0							
2. Voting – 1956	.24	1.0						
3. Complex – 1956	.31	.15	1.0					
4. No care – 1956	.41	.14	.29	1.0				
5. No say – 1960	.32	.15	.16	.23	1.0			
6. Voting – 1960	.20	.34	.19	.18	.23	1.0		
7. Complex – 1960	.18	.08	.44	.21	.26	.14	1.0	
8. No care – 1960	.24	.07	.21	.32	.39	.12	.29	1.0

[a] All correlations are Pearson's r. $N = 978$.

[b] Complete wording of each item is as follows:

1. Sometimes politics and government seem so complicated that a person like me can't really understand what's going on (Complex).
2. Voting is the only way that people like me can have a say about how the government runs things (Voting).
3. I don't think public officials care much what people like me think (No Care).
4. People like me don't have any say about what the government does (No Say).

a say about who runs things," (VOTING) is logically and empirically distinct. In a factor analytic study, Welch and Clark found support for this hypothesis in the fact that only the VOTING item did not load strongly on one dimension with the other items.[23] Independently, House and Mason found that the VOTING item correlated with demographic variables differently than the other three items did.[24] And, Converse has shown that the trend in aggregate responses to the VOTING item has moved in a different direction from the other three items in recent years.[25]

A third hypothesis is that the four-item efficacy scale is comprised of two distinctive subscales. Converse first argued that the COMPLEX item ("Sometimes politics and government seem so complicated that a person like me can't understand what's going on.") represented a subjective competence dimension of efficacy, while beliefs that "I don't think public officials care much about what people like me think" (NO CARE) and "people like me don't have any say about what government does" (NO SAY) are indicative of a dimension of belief in governmental responsiveness. The VOTING item he felt was difficult to place empirically in only one dimension.[26] Balch later provided "external validation" for a two dimensional division by showing that the governmental responsiveness items on one hand and the VOTING and COMPLEX items on the other manifested consistently different relationships with other theoretically relevant variables.[27] The relationship between the two dimensions was unspecified, but one can infer that the dimensions could be correlated.

We used the SRC panel study of 1956–1958–1960 as the data base for our analysis. The efficacy questions were asked in 1956 and 1960, providing a four-year period for possible attitudinal change. Only those respondents who completed both the 1956 and 1960 interviews were included in the analysis. The Costner method was used to estimate the stability and reliability of the basic four item scale; then we moved to the covariance procedure to test other hypotheses about the scale. A matrix of the correlations used for the following computations is presented in Table 1.

Findings

Estimated mean path coefficients calculated from the Costner method are presented in Figure 2a. The reliabilities of the items range from .32 for the VOTING item in 1956, to .64 for the NO SAY item in the same year; half of the items had mean estimates of reliability less than .50. The estimated stability coefficient

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[23] Welch and Clark, "Political Efficacy as an Underlying Political Orientation."

[24] James S. House and William Mason, "Trends in Some Survey Measures of Political Alienation in America," paper presented at the meeting of the American Sociological Association, New York, August, 1973.

[25] Converse, "Change in the American Electorate," p. 328.

[26] *Ibid.*, pp. 337–338.

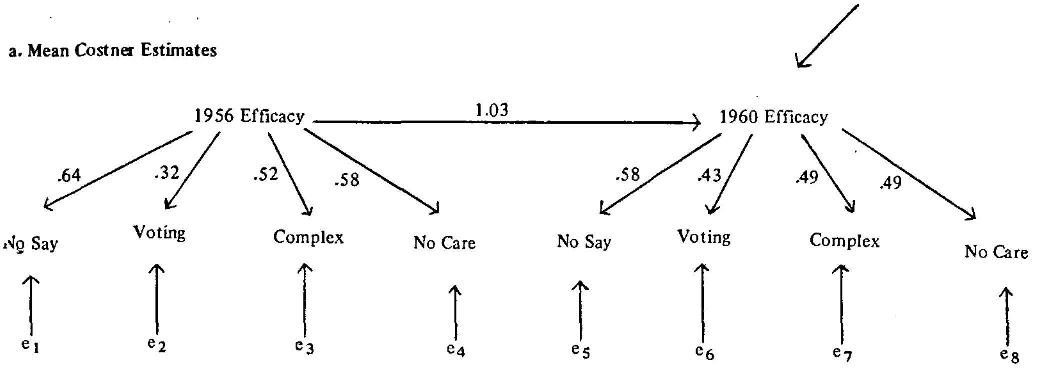
[27] Balch, "Multiple Indications in Survey Research."

Table 2. Summary of Estimates Made Using Costner Method[a]

Path	Maximum Estimate	Minimum Estimate	Mean Estimate	S.D.
a (No Say 1956)	.90	.35	.64	.14
b (Voting 1956)	.64	.16	.32	.13
c (Complex 1956)	.91	.29	.52	.18
d (No Care 1956)	.80	.27	.58	.14
e	2.66	.19	1.03	.64
f (No Say 1960)	.91	.31	.58	.15
g (Voting 1960)	.77	.25	.43	.16
h (Complex 1960)	.85	.18	.49	.17
i (No Care 1960)	.74	.16	.49	.15

[a] Fourteen estimates were made for Paths a, b, c, d, f, g, h, i and seventy-two estimates for e.

a. Mean Costner Estimates


1956 Efficacy
1.03
1960 Efficacy
.64
.32
.52
.58
.58
.43
.49
.49
No Say
Voting
Complex
No Care
No Say
Voting
Complex
No Care
e_1
e_2
e_3
e_4
e_5
e_6
e_7
e_8

b. Jöreskog Estimates

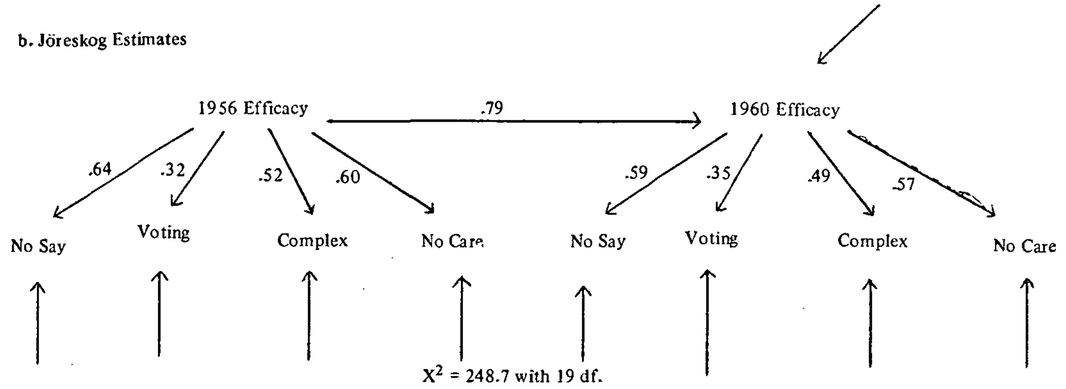

1956 Efficacy
.79
1960 Efficacy
.64
.32
.52
.60
.59
.35
.49
.57
No Say
Voting
Complex
No Care
No Say
Voting
Complex
No Care
X^2 = 248.7 with 19 df.

Figure 2. The Four-Item Efficacy Scale with Uncorrelated Error Terms

was over 1.0, an unrealistic estimate. Table 2 provides more information about the estimates for each path; the reliability estimates are fairly stable, with a low standard deviation, while the standard deviation of the estimates of the stability coefficient is larger.

At this point the difficulties with the Costner method become more apparent. The model of Figure 2a is obviously defective in some way, but it is difficult to pinpoint the problem. The solutions as presented require an unlikely conclusion – that the measures are more than perfectly stable.[28] In terms of item reliability, the VOTING item is weakest, but in 1960, at least, it was not substantially weaker than two of the other items.

Estimating the same model with the Jöreskog method provides a slightly different picture. A comparison of the Costner estimates in Figure 2a with the Joreskog estimates in 2b reveals that the mean reliability coefficients using the Costner method are very similar to those computed using the covariance structure approach. The stability coefficients, however, are quite different. The Jöreskog method calculations show a stability coefficient of .79 between efficacy at time one and time two, compared with that of 1.03 computed by the Costner method. This stability coefficient is still much higher than the Pearson's correlation of .48 found between the four-item scale at the two points in time; this difference represents a gain in estimated variance explained in efficacy in 1960 by the 1956 score from less than 25 per cent to more than 60 per cent. Thus, the conclusion is that the four-item scale is more stable than previously thought. Yet a closer examination of Figure 2b reveals some problems with the simple four-item model. The VOTING item is less reliable than even in the Costner estimates, and the COMPLEX item is not particularly strong. Overall, the fit of the model is resoundingly poor, with a χ^2 of over 248 with 19 degrees of freedom.[29] A correlation matrix reconstructed from the computed path coefficients reveals a particularly large difference between the actual and reconstructed over-time correlations of VOTING and COMPLEX. This suggests that the causal structure of the model in Figure 2 does not accurately describe the relationships among the four items in general; VOTING fits particularly poorly.

Since the least reliable indicator according to the estimates of Figure 2 is the VOTING item, and since hypothesis two suggests that this item is logically distinct, a conceivable alternative to the four-item model is a three-item model which excludes the VOTING item. Such a model is presented in Figure 3a. The estimates of Figure 3a suggest that such a three-item model is plausible. The reduction in χ^2 is quite pronounced,[30] ($\chi^2 = 107.3$) while the reliabilities of the items are almost identical; the estimated stability of the construct differs by only .04 units. This fact, and the observation that the VOTING item is consistently the least reliable indicator, suggests that the three-item hypothesis is more viable than the hypothesis that all four items best represent the construct. Notice however, that even the "reduced" model does not fit the data very well. With a chi-square of 141 and 8 degrees of freedom, it is clear that the three-item model does not provide a perfect representation of the causal structure in the data.

Turning to hypothesis three, we find that the notion of two correlated dimensions is totally unrealistic for these data. The intercorrelations among the constructs of "subjective competence" and "responsiveness" are nonsense. The third hypothesis produces unreasonable estimates for the relationships among the constructs. For instance, the "responsiveness" construct is almost perfectly correlated with the "subjective competence" construct, while the former is less than half as stable as the latter. Practically everything about Figure 3b suggests that the notion of two correlated dimensions is not supported by the data; the

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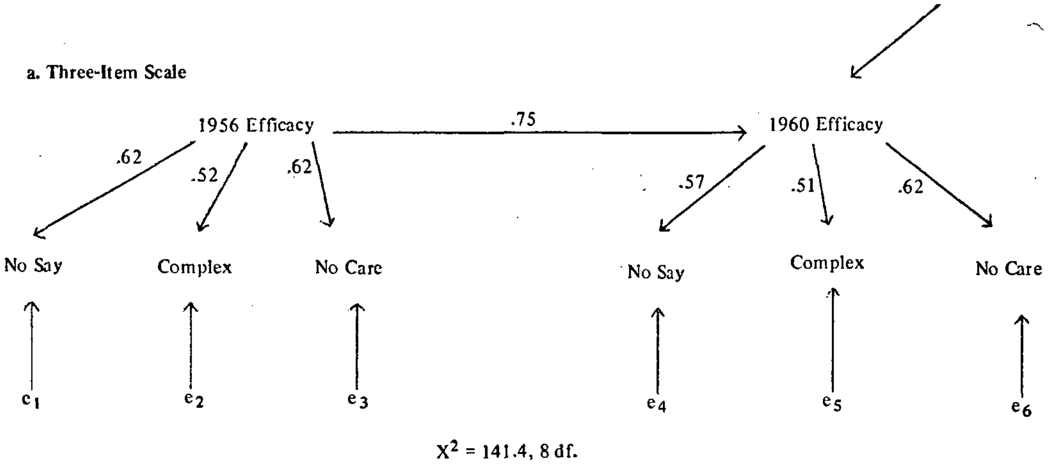
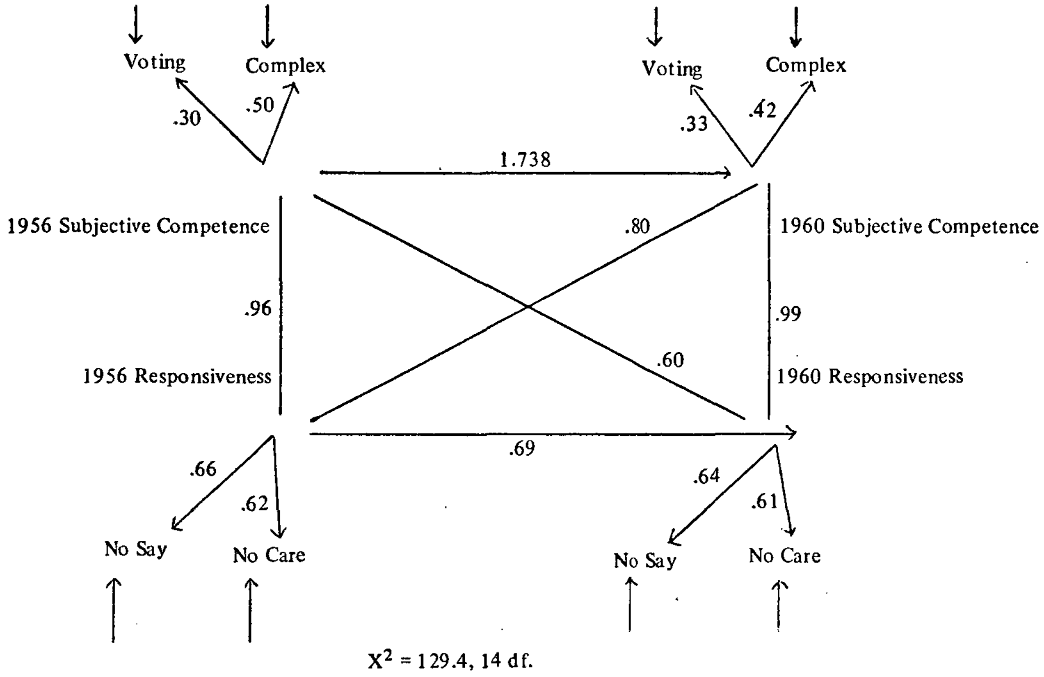
[28] In fact, if the analogy of the interpretation of the estimate *e* as a path coefficient can be maintained, the fact that *e* is 1.03 would simply mean that a score in 1956 which is one standard deviation above the mean becomes a score in 1960 which is 1.03 standard deviations above the mean. This finding is still rather unlikely. We note also that one could formulate the problem in unstandardized terms (i.e., use covariances instead of correlations, and examine unstandardized coefficients instead of path coefficients) however, a parallel unstandardized analysis using Jöreskog's procedure revealed no substantively different conclusions from the ones presented here. We present the standardized results for the purposes of clarity and simplicity.

[29] In general, we expect to find relatively large χ^2 values with large samples such as the present one. The χ^2 statistic will be most useful in such cases to compare different models, rather than to evaluate the absolute fit of any single model. We will use the χ^2 values produced by the different models generated in this section for this comparative purpose.

[30] The use of chi square in this case is heuristic, since the model of Figure 3 is not an exact statistical alternative to the model of Figure 2.


a. Three-Item Scale

1956 Efficacy
.75
1960 Efficacy
.62
.52
.62
.57
.51
.62
No Say
Complex
No Care
No Say
Complex
No Care
e1
e2
e3
e4
e5
e6
X² = 141.4, 8 df.


b. A Four-Item Model with Two Correlated Dimensions

Voting
Complex
.30
.50
Voting
Complex
.33
.42
1.738
1956 Subjective Competence
.80
1960 Subjective Competence
.96
.99
1956 Responsiveness
.60
1960 Responsiveness
.69
.66
.62
.64
.61
No Say
No Care
No Say
No Care
X² = 129.4, 14 df.

Figure 3. Two Alternative Models

hypothesis of two correlated dimensions must be rejected.

The most troublesome aspect of Figure 3b is the extreme variation in the estimated stabilities of the two constructs. This variability suggests that there may be sources of stability in the four items over time that are independent of the efficacy construct. That is, the "unique" components of the individual items seem to produce different amounts of stability in each item. This fact can be seen more clearly in Figure 4a, which allows each item to be correlated with itself over time independently of the efficacy construct. We will call this type of correlation "autocorrelation." In support of the impressionistic evidence offered in Figure 3b, this model strongly suggests that the COMPLEX and VOTING items have sources of autocorrelation unrelated to the political efficacy construct.[31] These two items are consistently the least reliable of the four and demonstrate substantially different stabilities in Figure 3. The obvious conclusion is that there are strong influences on each of these items which are unrelated to efficacy and to the other two items. The COMPLEX item shows the highest level of autocorrelation at .30; approximately nine per cent of the variation in COMPLEX in 1960 is attributable to COMPLEX in 1956 independently of the efficacy construct and the other items. While small in absolute terms, this amount is nearly half of the total common variance in COMPLEX 1956–1960. This finding constitutes very strong evidence that the COMPLEX item does not measure the same thing as the other items, and should not be combined with them. The situation is even clearer with the VOTING item; even though the estimated autocorrelation is smaller, it is a greater proportion of the total common variance. While the common variance is less than 12 per cent, the common variance due to sources other than efficacy is well over 55 per cent of that amount.

The data indicate that the VOTING and COMPLEX items do not have the same sources of stability as the other two items. A formal test of the hypothesis that VOTING and COMPLEX themselves have the same sources of autocorrelation is provided by Figure 4b. This figure shows that VOTING and COMPLEX do not have identical sources of autocorrelation. In fact, the hypothesized single source is actually inversely (if at all) correlated with the VOTING item.[32] The implausibility of this model is further amplified by the χ^2 fit of over 130. When this model is compared with that of Figure 4c which actually uses fewer degrees of freedom, it is easily seen that VOTING and COMPLEX have different sources of stability. This means that it is very unlikely that VOTING and COMPLEX are indicators of the same dimension.

Figure 4c itself presents what seems to be the most plausible model. The model allows independent sources of autocorrelation in VOTING and COMPLEX (S_1 and S_2 respectively) and assumes no autocorrelation in the other items. This assumption of no autocorrelation in NO SAY and NO CARE is the only substantive difference between the models if Figures 4a and 4c. A comparison of χ^2 values of the two models shows that this constraint increases the χ^2 fit only 14.6 units, which is quite a small difference for a sample of nearly 1000. Thus, the autocorrelation in NO SAY and NO CARE is negligible, while that in VOTING and COMPLEX is substantial, and comes from different sources. VOTING and COMPLEX do not belong with the other items of the scale, and they show no evidence of fitting with each other.

Throughout the analysis, the two hypothesized governmental responsiveness items have been high in reliability, and their uncorrelated error terms suggest that this stability results from their relationship to the political efficacy construct. Figure 5 illustrates a model incorporating only these two items. The recomputed correlation matrix is very similar to the actual one, and the chi square value is 18 with 1 degree of freedom, which is substantially less than the initial model's chi square of 248. This suggests a reasonably good fit of the two-item model with the data, given the extremely large number of cases in the analysis.[33] Thus, the two-item scale appears to maximize reliability and stability, while minimizing the contamination of the scale by extraneous sources of autocorrelation.

Discussion

What does the use of path analytic techniques to examine reliability and stability in efficacy tell us about the construct that we did not know before? Before answering this question, we again emphasize that we have not dealt with the semantics of indicator validity. We have measured the reliability and validity of four items that have been widely used to measure efficacy. While it is impossible to say which, if any, measures "real" efficacy, we can say that two of the items seem to us to

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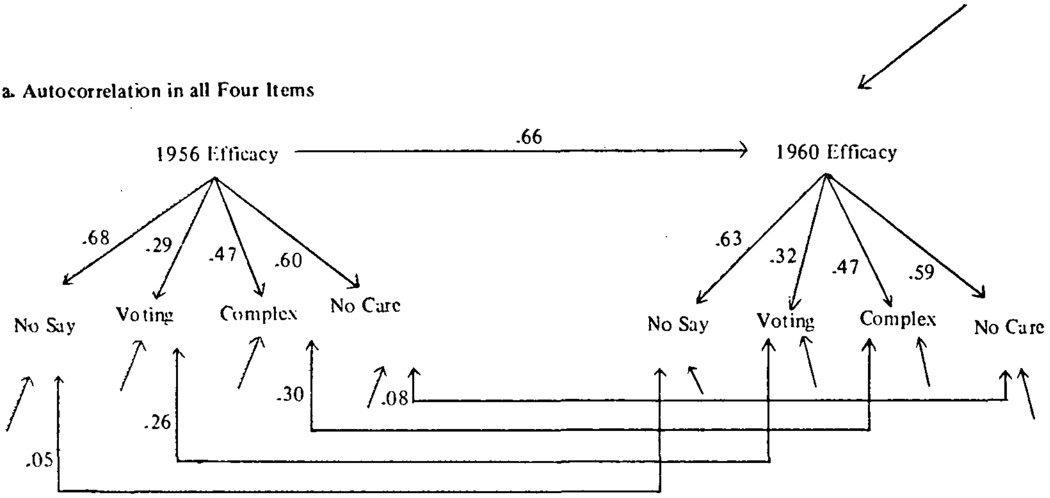
[31] Others might prefer to call the COMPLEX or VOTING items the "real" efficacy while giving a different name to NO CARE and NO SAY. Since all four items were initially designed to measure efficacy and since we have discovered that only two of them measure substantially the same thing, we think it is most reasonable to call what the two items measure in common "efficacy."

[32] The hypothesized single source of autocorrelation for VOTING and COMPLEX is simply a third unmeasured variable which is constrained to have a zero correlation with the two efficacy constructs. The paths linking this construct with the measured items are estimated in the same manner as the paths linking efficacy to the items.

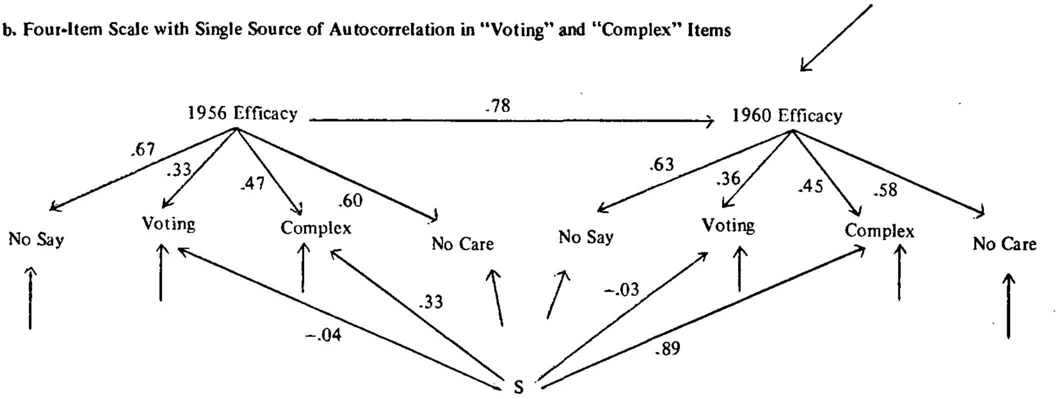
[33] The chi-square statistic is, of course, directly proportional to the sample size. Note that the two values have different degrees of freedom.

a. Autocorrelation in all Four Items


1956 Efficacy
.66
1960 Efficacy
.68
.29
.47
.60
No Say
Voting
Complex
No Care
.63
.32
.47
.59
No Say
Voting
Complex
No Care
.30
.08
.26
.05

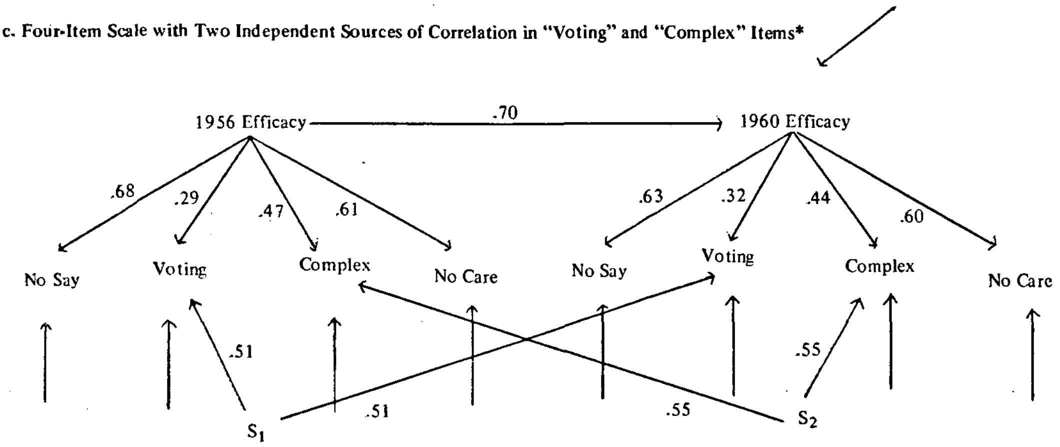
X^2 = 40.7, 15 df.

b. Four-Item Scale with Single Source of Autocorrelation in "Voting" and "Complex" Items


1956 Efficacy
.78
1960 Efficacy
.67
.33
.47
.60
No Say
Voting
Complex
No Care
.63
.36
.45
.58
No Say
Voting
Complex
No Care
.33
−.04
−.03
.89
S

X^2 = 130.9, 15 df.

c. Four-Item Scale with Two Independent Sources of Correlation in "Voting" and "Complex" Items*


1956 Efficacy
.70
1960 Efficacy
.68
.29
.47
.61
No Say
Voting
Complex
No Care
.63
.32
.44
.60
No Say
Voting
Complex
No Care
.51
.51
.55
.55
S1
S2

X^2 = 55.3, df = 17.

*The estimates for S_1 and S_2 are constrained to be equal to allow estimation of the model.

Figure 4. Three Alternative Four-Item Models

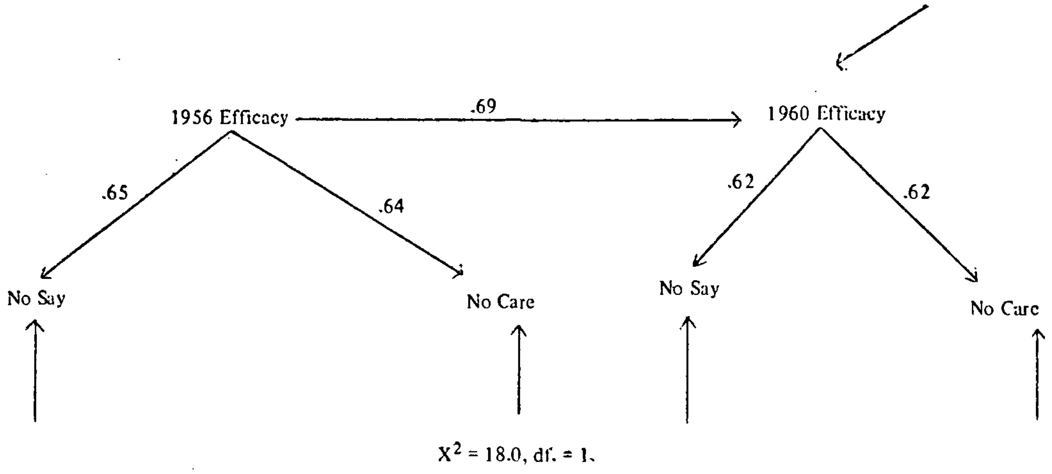


Figure 5. The Two-Item Scale

represent best the efficacy construct. With this caveat, we can make the following conclusions. First, the use of the technique allows us to discard as implausible the models suggested by all three of our original hypotheses. Neither the four-item, the three-item nor the two-dimensional model is strongly supported by the data.

Second, the procedure allowed us to pinpoint two of the four items (NO SAY and NO CARE) which did have high reliabilities. These findings led us to the conclusion that the two "governmental responsiveness" items are highly reliable indicators of the efficacy construct, which is more stable than previously thought. Efficacy in 1956 explains barely less than 50 per cent of the variance in efficacy in 1960 (models 4c and 5); over a four-year period this represents substantial stability.

The use of this measurement model also showed that the other two items (VOTING and COMPLEX) were relatively unreliable and had substantial sources of stability unrelated to the efficacy construct. In other words, some type of systematic error in both 1956 and 1960 inflated the over-time stability of these items independently of their relationship to efficacy. Further, the fit of model 4c to the data suggests that VOTING and COMPLEX are themselves indicators of two different constructs since they have different sources of autocorrelation. Thus, an alternative hypothesis concerning the composition of the efficacy index can be shown to be supported by the data: that efficacy is most reliably measured by two items, and the other two items each reflect a separate construct. Converse's tentative speculation is supported. These findings also suggest that external validation does not substitute for tests of internal validity. The subjective competence items appeared homogeneous in the former tests but proved to be quite distinct here.

There are concrete applications of these results. Those working with efficacy indices in survey research would probably be wise to consider using NO CARE and NO SAY items without the VOTING and COMPLEX items. The former two items are reliable, reasonably stable, and almost uncontaminated by systematic measurement error, while the latter items are relatively unreliable, unstable, and display systematic differences from each other and the NO SAY and NO CARE items.

The next step may be to examine further the different error sources of the COMPLEX and VOTING questions. The case of VOTING is perhaps the more clear-cut. The very wording of the item suggests ambiguity: "Voting is the *only way* people like me can have a say . . ." (emphasis ours). One assumes that an individual who felt highly efficacious would respond "no," believing he or she had a variety of other effective ways to influence government. But an individual quite low in efficacy could respond negatively as well, believing that he or she had no means at all to influence government. At best, such a person would be confused by the question. This indicates a possible curvilinear relationship between efficacy and responses to the VOTING item, with only those who are rather moderately efficacious responding yes. Likewise, the VOTING item may also display a curvilinear relationship to participation. Those who participate in no activities and those who participate in many would likely respond "no" while voting specialists[34] may be most likely to respond affirmatively.

The wording of the COMPLEX item is less ambiguous. Yet, since a yes response admits ignorance, the question wording suggests the possibility of contamination by a social desirability response set, which may in turn be linked to social class. The testing of these hypotheses with an extension of the measurement model presented here will be the next step in the examination of the efficacy construct.

For the moment, however, the use of this measurement model has allowed us to advance knowledge about the operationalization of efficacy. Its stability has been more precisely measured, and its potential indicators narrowed to those which show the strongest relationship to the efficacy construct. Finally, the techniques used here have broader applications than just to political efficacy, since questions of indicator reliability, stability, and dimensionality arise for many political constructs.[35]

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[34] See Sidney Verba and Norman Nie, *Participation in America* (New York: Harper and Row, 1972). The evidence on the curvilinearity based on a simple analysis of variance is mixed. The eta^2 between the voting item and the responsiveness dimension is 6 per cent in 1956 and 4 per cent in 1960, surely a small relationship. Of that, the linear relationship accounts for an R^2 of 5 per cent and 3 per cent respectively. The simple relationship between voting and degrees of political participation is practically nil, although those who participate in a wide variety of political acts are slightly more likely to respond "no" to the item than either voting specialists or nonparticipants.

[35] In fact, the full generality of the Jöreskog procedures has not even been hinted at in this paper. For instance, in the context of a more elaborate analysis, additional variables representing outside influences upon efficacy or individual items could be introduced. The technique can also be used to address more sophisticated measurement issues such as whether tests are parallel, congeneric, or tau-equivalent (K. G. Jöreskog, "Statistical Analysis of Sets of Congeneric Tests," *Psychometrika,* 36 (June 1971), 109–133. For a fuller discussion of the flexibility of the technique, see especially Jöreskog, "A General Method," and Jöreskog, "A General Model."

The Philippines before Martial Law: A Study in Politics and Administration*

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The frequently discussed tension between political participation and order[1] was especially apparent in Third-World competitive electoral systems such as that which existed in the Philippines before martial law was declared in September, 1972. Scarce resources, intensive intra-elite electoral competition, and a growing peasant and urban lower-class electorate increased pressures for costly social services. A high birth rate, outmigration from stagnant rural areas, and industrial growth through a process of import substitution that relied too heavily on capital-intensive technology all expanded the pool of surplus labor, intensifying pressures on politicians to employ clients in the bureaucracy. Rising election costs and a large bureaucracy fueled a cyclical and chronic inflation, which led, when economic growth slowed, to conflict among social groups. As pressures for social services increased and the threat of mobilization by counter-elites grew, urban businessmen, high-level administrators, and technocrats found further justification for limiting political interference in administration.

To help maintain dominance by a small conservative elite over major institutions and protect foreign investors, Philippine regimes sustained barriers against new interests that threatened the status quo. Such barriers helped channel the fruits of growth to a few entrenched interests and perpetuated a circle of economic and social dualism keeping the poorest groups poor.[2] Efforts by the executive to centralize and "rationalize" administration sought to: (1) contain the dislocating effects of economic change through repression and the more efficient provision of limited social services; and (2) rationalize markets for foreign investors, creditors, and that portion of the indigenous elite with national economic interests by attempting to curb the destabilizing effects of political competition. In the Philippines during the 1960s, for example, deficit spending during elections stimulated inflation and helped lead to decontrols, tight money, and a slowing of economic growth in manufacturing.

As long as the Philippine electorate remained small and voters deferred to established notables who were recognized as natural leaders, Philippine elites were able to regulate participation without excessive expenditures on patronage. Traditional habits of deference gradually eroded, however, and vertical ties could only be maintained through greater reciprocity between patron and client. Accordingly, the demand grew for material inducements as politicians became pressed to provide schools, public works, cash payments, jobs, and other forms of patronage in exchange for votes.[3]

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[1] See Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968).

[2] See the argument by D. Weintraub, "Development and Modernization in the Philippines: The Problem of Change in the Context of Political Stability and Social Continuity," *Sage Research Papers in the Social Sciences, Studies in Comparative Modernization Series,* Series No. 90-001, Vol. 1 (Beverly Hills: Sage, 1973), pp. 19–20.

[3] The impact of changes in clientelist relationships upon parties and politicians is discussed in James C. Scott, "Corruption, Machine Politics, and Political Change," *American Political Science Review,* 63 (December, 1969), 1142–1158. See also, by the same author, "Patron-Client Politics and Political Change in Southeast Asia," *American Political Science Review,* 66 (March, 1972), 91–113, and Carl H. Landé, "Networks and Groups in Southeast Asia: Some Observations on the Group Theory of Politics," *American Political Science Review,* 67 (March, 1973), 103–127.

In order to maximize electoral support, Philippine politicians spread scarce patronage as thinly as possible, caring little about when and if a project could be completed. The particularistic nature of many demands increased political influence at the enforcement stage of policy.[4] Consequently, policy enforcement often did not conform to general rules.

Business interests were subject to the demands of influential politicians, which required making payoffs to secure needed capital, legislation, licenses, and other rights. Changes in regimes required corporations to make further accommodations with new incumbents. While large corporations often could pass on political costs to the consumer (as could, to a lesser degree, small corporations with less control over politicians and markets), many corporations could have lowered their costs and planned more accurately had the bureaucracy been insulated from excessive politics and had turnover among national politicians been lower. Business leaders, wanting closer coordination between government and business planning, complained that politics, which "dissipates resources on projects and activities with high political impact but low economic returns," was largely responsible for government failure to plan in ways that would maximize business growth.[5] American business, preferring to work through government rather than through the less predictable legislators,[6] has benefited from the suppression of politics under martial law.[7]

The inflationary push of elections led to two devaluations between 1960 and 1971 and frustrated plans to diversify the economy. By raising the price of capital imports, devaluation slowed manufacturing growth and further reinforced dependence on primary exports. Exporters, whose interests were overrepresented in Congress, thus appeared to gain at the expense of manufacturers producing for the internal market.

It is unlikely that all agencies and levels of administration were equally subject to political pressures. The long-standing efforts of various agencies to rationalize local administration and limit the discretionary power of mayors (e.g., the College of Public Administration and the Local Government Center at the University of the Philippines, the Philippines Department of Finance) suggest that local politics is a useful setting to examine more carefully how Philippine politics led, in the period before martial law, to overstaffed administration and created strong pressures for social services. After demonstrating that certain elite groups in the Philippines favored administrative reform at the national and local level, I shall examine the nature and form of political pressures in the allocation of local public resources. More specifically, I shall assess whether class structure, urbanization, political mobilization, and the strength of political machines affected the distribution of budgetary resources and the absorption of surplus labor by the bureaucracy. In the Philippines, I shall argue, competitive elections and scarce resources encouraged political elites to employ protégés in the bureaucracy, and at the same time increased pressures for social services from the government.

National Reform

The growth of national corporate capital and the more complex integration of the Philippine economy with those of the United States and Japan, generated pressures for stronger presidential leadership, planning, and administrative reform. Administrative reform was urged by foreign banks, aid donors, and international banks such as the Asian Development Bank. Under technical advice from management consulting firms in the United States, the Philippine Congress passed a revised budget act in 1954 authorizing performance budgeting in twelve national agencies.[8] American foundations helped expand the Asian Institute of Management, as well as the College of Business Administration and the College of Public Administration at the University of the Philippines. Technocrats and businessmen were trained through these institutions, as well as through study abroad.[9]

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[4] Scott, "Corruption, Machine Politics, and Political Change," pp. 1142–1146.

[5] Sixto K. Roxas, "Policies for the Private Sector," *Philippine Economic Journal,* 8 (1st Sem., 1969), 26, cited in Romeo B. Ocampo, "Technocrats and Planning: Sketch and Exploration," *Philippine Journal of Public Administration,* 15 (January, 1971), 35–36.

[6] Richard A. Styskal, *Strategies of Influence Among Members of Three Voluntary Associations in the Philippines* (Ph.D. dissertation, University of Oregon, 1967), cited in Robert B. Stauffer, "The Philippine Congress: Causes of Structural Change," *Sage Research Papers in the Social Sciences, Comparative Legislative Study Services,* Series No: 90-024, Vol. 3 (Beverly Hills: Sage, 1973), p. 12.

[7] Martial law, implemented in 1972, rationalized the political environment for business by centralizing administration. Entrepreneurs now have to bribe fewer government officials. For a description of the salutary effect of martial law on foreign business in the Philippines, see the Corporate Information Center of the National Council of Churches of Christ in the U.S.A., "The Philippines: American Corporations, Martial Law, and Underdevelopment," *IDOC-International/North American Edition,* 57 (November, 1973), pp. 27–35.

[8] Angel Q. Yoingco, Antonio O. Casem, Jr., and Amancia G. Laureta, "A Review of the Philippine Experience in Performance Budgeting," *Philippine Journal of Public Administration,* 13 (July, 1969), 263.

The growing influence of American-trained technocrats and businessmen in the Philippines during the late 1960s signalled the rise of an antipolitical, corporatist ideology. This ideology was opposed both to "old-style" politics and to the growing nationalism of senators such as Benigno Aquino and Jose Diokno (and other members of the intelligentsia) – a nationalism which threatened the continued access by foreign corporations to Philippine raw materials, cheap labor, and markets.[10] Technocrats like Sixtos Roxas consistently urged closer coordination between government and private enterprise in planning, and disdained the strong political pressures for social services. They felt many such services were wasteful and unproductive.[11]

The displeasure of technocrats often focused on the Philippine Congress, which provided an institutional power base for segments of the rural landed elite attempting to slow the efforts of the executive to centralize power.[12] Unlike the process in many other ex-colonies, Filipinization of the bureaucracy followed Filipinization of the Congress. The bureaucracy (except at the highest levels) became an avenue for middle-class mobility and was highly vulnerable to political intrusion by powerful legislators. As Grossholtz writes, "Congress is far superior to the bureaucracy and is not above making that clear."[13]

A growing electorate and rising election expenses increasingly preoccupied legislators concerned with individual survival. Obliged to perform personalistic services for local clients, legislators (especially in the House) became overburdened with particularistic legislation, to the exclusion of more general and long-term policy.[14] Additionally, the Philippine Congress appropriated more money than the nation had at its disposal. While excess appropriations increased the discretionary power of the president in choosing which funds to release, it meant that both the president and administrators were besieged by politicians seeking the release of funds for particularistic projects. Congressional control over budgets, enhanced by line-item budgets, directly drew administrators into extensive logrolling.

While projects occasionally produced both economic success and high political payoff, more often they did not. By emphasizing short-term gains at the expense of long-run transformations,[15] patronage politics undercut the attempts of several Philippine administrations to meet planned goals. Businessmen and planners, promoting long-term projects, competed with politicians over budgetary priorities. Predictably, small public works projects easily spread throughout the country, were quickly approved and more readily financed than large projects.[16] The amount appropriated for small public works projects in chartered cities between 1966 and 1969 correlates .54 with the amount actually allocated. The correlation is .34 for large public works projects financed over the same period.[17] Since appropriations for both types of public works projects over the years have been larger than available funds, the government accumulated promises it could not redeem As of June 30, 1970, only 17.8 per cent of the appropriations authorized since the first public works act in 1954, had actually been released.[18]

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[9] See Robert B. Stauffer, "Philippine Martial Law: The Political Economy of Refeudalization" (unpublished paper presented at the annual meeting of the Association for Asian Studies, Boston, Massachusetts, April 1–3, 1974), p. 15.

[10] Stauffer, "Philippine Martial Law," p. 14. See also Ocampo, pp. 53–64.

[11] Ocampo, p. 44.

[12] See Robert Stauffer, "Congress in the Philippine Political System," in *Legislatures in Developmental Perspective*, ed. Allen Kornberg and Lloyd D. Musolf (Durham: Duke University Press, 1970), pp. 334–365.

[13] Jean Grossholtz, "Integrative Factors in the Malaysian and Philippine Legislatures," *Comparative Politics*, 3 (October, 1970), 93–113, quote p. 109.

[14] Stauffer, "The Philippine Congress," pp. 14–16.

[15] See the discussion by Scott, "Corruption, Machine Politics, and Political Change," p. 1148.

[16] For small public works projects, the District or City Engineer is allowed to approve the program of work without obtaining approval by the Division Engineer or the Bureau Head. See Ledivinia Vidallon-Carino, *The Politics and Administration of the Pork Barrel* (Manila: Local Government Center, University of the Philippines, 1966), pp. 10 ff.

[17] Large public works projects are those costing more than P25,000. Public works appropriations are compiled from three public works acts – Republic Acts 4854 (1966–1967), 5187 (1967–1968), and 5979 (1969–1970). Actual expenditures for the 1966–1970 period are from data compiled by the Infrastructure Operations Center of the Presidential Economic Staff in an unpublished report to the president. For funded projects, there may be a one- to five-year lag between the appropriations and the release of funds.

Few legislators were interested in the maintenance of public works projects. As one high Philippine official complained, "Once a road, a school building, a hospital are completed, there are no funds for upkeep or maintenance. All the politician wants is to show people in his district that he can deliver: he is not concerned with viable continuing projects. The investment itself is all that counts."[19]

Even though individual legislators occasionally joined with administrators in pressing for reform, Congress as a whole was quite resistant to changes that would lessen its power. In the early 1950s, the Philippine Budget Commission (with considerable American advice) pushed vigorously for performance budgeting to increase administrative discretion over the management of projects. Although Congress passed a Revised Budget Act (R.A. 922) which shifted emphasis to performance budgeting, it undercut the very basis for performance budgeting through its "demand for a budget in a line-item format as the basis for legislative consideration and (authorization) . . . the majority of members of Congress, in the exercise of their legislative powers, would not surrender to agency administrators the right to determine the type and composition of agency organization and supporting facilities that should be set up to carry out their responsibilities under the law."[20] By 1967 Congress had authorized the appropriations of only 42 agencies on a performance basis; 22 of these agencies were colleges.[21]

Congressional sensitivity to local interests often set Congress at odds with the President over legislation designed to give more power to local units. For example, President Marcos vetoed in 1967 a decentralization bill. Between 1946 and 1970, the percentage of largely particularistic bills (i.e., bills dealing with provincial and municipal government) passed by Congress increased from 19.6 to 64.5 percent.[22] A number of these bills transformed municipalities into chartered cities,[23] thereby increasing the local government's taxing capacity and the percentage of national revenues remitted to them.

Class Structure and Local Reform

In their efforts to unseat machine politicians, urban reformers did not need to agitate for at-large elections, except in Manila where representation in the city council was by district. At-large elections in the United States favored well-known men from the business community and hindered minority or ethnic representatives concentrated in wards.[24] Nevertheless, certain conditions in the Philippines made it difficult for urban reformers to increase "efficiency" by narrowing political access for the lower class.

Changes in city government structure in the Philippines required national legislation by Congressmen reluctant to oppose allied machine politicians in their districts. The large size of the lower, economically insecure classes in most chartered cities favored politicians who cultivated relationships with local barrio and ward leaders. The machine, more than the reform politicians, could bind clientele to it by providing legal relief, occasional employment, and other particularistic services.

With their frequent disdain for machine politics, reform candidates in the Philippines have not met with much long-term success.[25] While urban politicians could not insulate themselves from lower-class demands and expect to remain in office, those with a fairly substantial business and middle-class constituency (who generally want feasibility planning, efficiency, and community-wide, "indivisible" benefits) had to balance middle- and lower-class demands in order to be re-elected. The high turnover among local officials in larger, more developed cities[26] indicates that it has been difficult, with the shortage of local resources and high costs of improving the urban environment, for politicians to maintain their support.

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[18] Department of Public Works and Communications, *Annual Report for Fiscal Year 1969–1970* (Manila: Bureau of Printing, 1970), p. 1.

[19] An anonymous official quoted in Naomi Caiden and Aaron Wildavsky, *Planning and Budgeting in Poor Countries* (New York: John Wiley, 1974), p. 118.

[20] Honesto Mendoza, "Deficiencies in Our Government Budgeting and Accounting Systems," in *Perspectives in Government Reorganization,* ed. Jose Veloso Abueva (Manila: College of Public Administration, University of the Philippines, 1969), p. 229.

[21] Yoingco, Casem, and Laureta, pp. 263 and 274.

[22] Stauffer, "The Philippine Congress," p. 14.

[23] Between 1960 and 1970, some 28 municipalities became chartered cities. See Proserpina D. Tapales and Eleanor P. Maling, "Proposed Criteria for Philippine Cities: A Plea for Congressional Rationality," *Philippine Journal of Public Administration,* 14 (July, 1970), 311–319.

[24] James Weinstein, *The Corporate Ideal in the Liberal State: 1900–1918* (Boston: Beacon Press, 1968), pp. 110–116.

[25] In 1959 six of the eight candidates elected to the city council in Quezon City represented the Citizens League of Quezon City; in 1963 only two reform candidates were reelected. In Pasay City the League for Good Government did not win any elective posts in 1963, and in Makati only one reform candidate was reelected. The elitist image of the reformers and lack of communication between the citizens' leagues and the lower classes appears to have hurt the reformers. See Aprodicio A. Laquian, *The City in Nation Building* (Manila: School of Public Administration, University of the Philippines, 1966), pp. 187–192.

The election of urban reformers and patricians became more difficult in the Philippines as literacy increased and the percentage of the electorate that was lower class grew. Cities in which low levels of literacy limited the size of the electorate were often governed by patricians – a pattern common earlier in many Philippine cities. When local officials were appointed or elected in a limited franchise, mayors (and to a lesser extent councilors) hailed largely from well-known families with extensive business interests in both rural and urban areas. As political office in all cities became elective in 1959, and lower-class voters increasingly registered to vote, more political professionals were elected at the local level.[27]

Pressures for Social Services

With the exception of the Hukbalahap movement in the immediate post–World War II period, lower-class groups in the Philippines were rarely organized or ideologically predisposed to press for a fundamental redistribution of resources. Unlike what happened in Latin American countries such as Chile before the military coup against Allende, in the Philippines the lower classes did not undergo sustained political mobilization "by leaders of political groups external to the low-income communities themselves . . . in the context of a national political environment which is supportive of aggressive demand making by low-income groups in a wide range of issue areas."[28] Nevertheless, a growing lower-class electorate within a clientelist structure of political mobilization raised pressures for both neighborhood-wide and individual inducements (e.g., schools and jobs). The local or individualistic character of patronage, in "which policy is not really policy but highly individualized decisions that only by accumulation can be called a policy,"[29] should not obscure the fact that patronage often served business and middle-class interests.

The Philippine elite and upper-middle class tend to educate their children in private elementary and secondary schools and to use private health services. As a result, local expenditures for social improvements and services (primarily expenditures for intermediate and secondary education) disproportionately served the lower and lower-middle classes.[30] On the other hand, pressures for economic improvements (primarily for construction and maintenance of roads) often originated with businessmen and landowners eager to expand markets.[31]

Competition among Philippine local elites was exacerbated as the elites became increasingly differentiated and as the electorate grew in the 1950s and 1960s. In some Philippine cities, fragmented political power, shifting coalitions, and scarcity increased the bargaining power of barrio or ward leaders and their clients. Urban politicians could no longer, as in earlier days, ignore the requests of community leaders for social services. As Huntington argues in writing of Third-World nations more generally, "without an overriding cleavage to bifurcate the political arena, each faction tries to overcome its opponents of today by alliances with its opponents of yesterday."[32] Similarly, students of American state politics often argue (if not always demonstrate) that in noncompetitive situations, the majority party does not need to concern itself as much as in competitive situations with the interests of lower socioeconomic groups.[33] While persistent ideological cleavages rarely occurred among either the local or national Philippine elite, intense rivalries pushed politicians uncertain of victory to expand their clientele.

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[26] See Thomas C. Nowak and Kay A. Snyder, "Clientelist Politics in the Philippines: Integration or Instability?" *American Political Science Review*, 68 (September, 1974), 1147–1170, at p. 1164.

[27] Laquian documents the transition in Manila "from prominent family to big businessmen, civic leaders, professional politician and 'new-breed' organization man" Laquian, pp. 82–87, quote p. 83. In his study of municipal officials in Capiz and Batangas, Machado also notes the growing recruitment of political professionals into politics. Kit G. Machado, "Changing Patterns of Leadership Recruitment and the Emergence of the Professional Politician in Philippine Local Politics," *Philippine Journal of Public Administration*, 16 (April, 1972), 147–169. See also, by the same author, "From Traditional Faction to Machine: Changing Patterns of Political Leadership and Organization in the Rural Philippines," *Journal of Asian Studies*, 33 (August, 1974), 523–546.

[28] See the discussion by Wayne A. Cornelius, "Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities," *American Political Science Review*, 68 (September, 1974), 1125–1146, quote p. 1146.

[29] Theodore J. Lowi, "American Business, Public Policy, Case Studies and Political Theory," *World Politics*, 16 (July, 1964), reprinted in *The Power Elite in America*, ed. Norman L. Crockett (Lexington: D.C. Heath, 1970), p. 84.

[30] The simple correlation between the percentage of population (in chartered cities) which is college-educated and the percentage of school-aged children enrolled in private elementary and intermediate schools is .80. In 1964, the private secondary school system enrolled from 35 to 100 per cent of all secondary students, depending on the chartered city.

[31] See Arturo G. Pacho, *The Administration of Public Highways Program in Davao* (Manila: Local Government Center, College of Public Administration, University of the Philippines, 1969), appendix A, p. 12. Since the Philippines has a surplus of literate, semi-skilled labor, business and landowning interests are not concerned to expand public primary and intermediate education, particularly nonvocational education, nor politicize public health programs. Lacking land, capital, or marketable surpluses, the poor, we feel, see greater *relative* benefits from cheap education and health services than from roads.

Larger, more economically diverse cities had more political factionalism (r=.35) and weaker political machines (r=.34) than did other cities.[34] Consequently, pressures to meet the social needs of the lower classes might have been greatest in the larger, more economically diverse cities, although scarcities might prevent such needs from being adequately met. These cities, however, also had a larger middle class and a more diverse industrial structure; they often had more vocal business interests which, in seeking to rationalize politics and improve efficiency, also attempted to limit demands from the unorganized or less wealthy sectors of the community. Where both business and lower-class demands were muted, local incumbents would be fairly free to enhance their power in the city administration (i.e., the mayor's office) by employing clients.

Measures and Data

The size of the lower class in Philippine chartered cities is measured by a factor score indexing the percentage of city residents in 1960 with no education, the percentage of the labor force in farming, the percentage of households with no radio, and the percentage with no toilet.[35] The size of the lower class strongly correlates negatively (r=–.70) with the percentage of city residents with some college and (r=–.64) with the percentage of the labor force in professional and administrative positions; therefore, to avoid multicolinearity only the more comprehensive measure of class – the factor score – will be used in the regression equations. City size and diversity is also measured by a factor score. Assuming that business pressures on government may be greater in more industrialized communities, I have added a measure of industrialization which indexes variables such as the percentage of the population in manufacturing and per capita fixed assets in manufacturing.[36]

Party affiliation is not included in the measure of machine strength because party switching was very common in the Philippines prior to martial law and because grass-roots party organizations with autonomy from local and regional elites had not developed in the post–World War II period. The strength of political machines is the mean plurality received by city mayors and by the strongest presidential and congressional candidates in a given city in elections between 1957 and 1969.[37]

Political mobilization is measured by the mean percentage of the population voting in both local and national elections between 1949 and 1969. Change in political mobilization is measured by residuals – deviation of the mean for political mobilization between 1961 and 1969 from the regression line computed by the regression of the mean of political mobilization between 1961 and 1969 on political mobilization between 1949 and 1957. There are two reasons for using electoral data which span the years 1949 through 1969 together with policy data which span the years 1961 through 1969:

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[32] Huntington, p. 415.

[33] For a summary of this literature, see Edward G. Carmines, "The Mediating Influence of State Legislatures on the Linkage Between Interparty Competition and Welfare Policies," *American Political Science Review*, 68 (September, 1974), 1118–1124.

[34] My measure of political machines will be discussed subsequently. Political factionalism is described in Nowak and Snyder, pp. 1158 and 1159.

[35] While a number of chartered cities have rural peripheries, all of the variables loading highly on this factor except "percentage of population in farming" measure poverty, not "ruralness." *Antipolo* or open-pit toilets (neither of which require a central sewer system) and cheap, battery-powered portable radios are widely used in rural areas. Thus the absence of toilets and radios in such areas indicates poverty, not a lack of urban facilities such as electric power and sewers. The author will, upon request, send factor matrices for any or all of the factor scores used in this article.

[36] See Nowak and Snyder, Tables 2 and 4, pp. 1156 and 1158 for the relevant factor matrices.

[37] The logic and validity of using pluralities as measures of machine strength in the Philippines is examined in Nowak and Snyder, pp. 1157–1158. The current measure of machine strength is more comprehensive than that discussed in the previous article because it combines pluralities for both local and national elections.

(1) to increase variance in the measure of change in political mobilization, and (2) to lag changes in expenditures with changes in political mobilization, since rising demands rarely cause immediate policy changes.

Because the use of per capita expenditures as a measure of local government effort penalizes poor chartered cities which had a weaker tax base and could not spend as much as wealthy cities,[38] the analysis relies more heavily on a second measure of government effort: the percentage of the city budget spent on one of three major functional categories – social improvements, economic improvements, and administration. While local governments had sufficient discretion over funds to respond to political pressures by shifting funds from one category to another, there were constraints on the internal reallocation of funds.[39] Therefore, as another measure of government effort, I include changes in the percentage of expenditures allocated to social improvements, economic improvements, and administration over a nine-year period. Using a measure of change over time increases the probability of finding that political forces will influence expenditures.[40]

Seventy-two per cent of total city expenditures go for social improvements, economic improvements, and administration.[41] Social improvements include maintaining and improving intermediate and secondary education; economic improvements primarily consist of constructing and maintaining city roads and bridges; and administrative expenditures are largely allocations for the city mayor's office.

Changes in percentage and per capita expenditures in the different budget categories are measured by the deviation of the percentage of the total budget spent on a given category in 1969 from the regression line computed by the regression of allocations in 1969 on allocations in 1961. This procedure controls for any variation among initial levels of expenditures for particular categories which might affect future expenditure levels.[42] Negative residuals suggest that allocations to a particular category have decreased, while positive residuals indicate that such allocations have increased.

The amount of labor absorbed by the bureaucracy is measured by the percentage of the local budget spent on salaries and wages. Cities with more revenues were required by law (Republic Act 4477) to spend less of their General Fund on salaries and wages.[43] However inappropriate such a law might have been in a labor surplus society such as the Philippines, it was intended to guard against local governments hiring excess labor when such labor did not serve as a substitute for capital. By controlling for the revenue class of cities, I can examine what other variables affect the percentage of the budget spent on salaries and wages. The base I have used for computing the percentage of the budget spent on salaries and wages (i.e., total expenditures) differs slightly from the base used by the Philippines Department of Finance (i.e., the General Fund), in order to provide a more comprehensive analysis of expenditures. The General Fund includes between 70 and 80 per cent of total city expenditures.

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[38] Summarizing American policy literature, Hayes writes: "The extant American literature is fairly conclusive in demonstrating that levels of wealth correlate strongly with amounts expended in policy areas." In her analysis of policy outputs in Brazilian States, Hayes uses per cent expenditures rather than per capita expenditures in different policy areas. Margaret Daly Hayes, "Policy Outputs in the Brazilian States, 1940–1960: Political and Economic Correlates," *Sage Professional Papers, Comparative Politics Series,* Series No. 01–030, Vol. 3 (Beverly Hills: Sage Publications, 1972), p. 17.

[39] Appropriations for the operation and maintenance of schools, for health and sanitation, and for maintenance, repair, and construction of roads could not be less than the amounts received respectively from tuition fees, municipal aids for health and sanitation purposes, and the manufactured oil allotment and motor vehicles allotment. Republic Act No. 5447 created a special education fund based on proceeds from an additional property tax and a certain percentage of the taxes on Virginia cigarettes. This fund was earmarked for uses such as the organization and operation of extension classes, elementary school buildings, repairs and construction, and teachers' salaries. See Pagtakhan-Sumilong and Federico B. Silao, eds., *A Summary of Laws and Regulations Affecting Philippine National-Local Government Relations* (Manila: Local Government Center, College of Public Administration, University of the Philippines, 1970), pp. 7 and 50–51. Tied revenues in Davao City, for example, constituted on the average (1961–1969) 31 per cent of the school fund. The remaining school revenues were transferred from the General Fund. Raul P. de Guzman, et al., *Report on the Davao City Government* (Manila: Local Government Center, College of Public Administration, University of the Philippines, 1970), p. 126. Since secondary education was less of a "public right" in the Philippines than intermediate education (grades 5–6), the transfer of expenditures for intermediate education to the national government in 1964 increased the elasticity of local expenditures on education, and social improvements in general.

[40] Sharkansky argues that as "the span between present and past expenditures increases . . . there is increasing opportunity for factors to enter the budgeting process that are remote from the context that surrounded the first budgeting period." Ira Sharkansky, *The Politics of Taxing and Spending* (New York: Bobbs-Merrill, 1969), p. 114.

[41] Other categories of expenditures include protective services, adjudication expenses, and costs of revenue collection. These categories are not used in the analysis because they are not as easily tied to particular class or political interests.

[42] See George W. Bohrnstedt, "Observations on the Measurement of Change," in *Sociological Methodology, 1969,* ed. Edgar F. Borgatta and George W. Bohrnstedt (San Francisco: Jossey-Bass, 1969), pp. 113–133.

[43] The maximum percentage which can be spent on salaries and wages for city employees under Republic

Compliance with Republic Act 4477 indicates some autonomy from the incessant political demands on administration to absorb protégés. Administrative noncompliance is measured by the percentage of salaries and wages that exceed legal guidelines in fiscal years 1968 and 1969. Penalties for exceeding these guidelines are weak. Routinely, the Philippines Department of Finance allowed overspending on salaries and wages of up to 20 per cent more than the amount allowed by law.

Local Autonomy

The unitary structure of the Philippine political system and the growing power of the central government[44] means that I must determine whether chartered cities were sufficiently autonomous in policy making to be considered meaningful units of analysis. The charters of Philippine cities and subsequent national legislation and regulations gave cities greater autonomy than either municipalities or provinces. I lack the data for units below the chartered city level (e.g., for barrios or individual people) that would be needed in order to determine how grouping data into such cities affects variance in the independent and dependent variables; nevertheless, the impact of national policy on local policy over time can still be assessed. For local policy to be fairly autonomous of national policy, the variance among cities from the national mean for city expenditures in any particular year should be relatively great. If national policy largely determined changes in expenditures, the deviation of yearly national means from the all-year mean should explain most of the variance. A dummy variable regression procedure equivalent to a one-way analysis of variance[45] yields the finding that national-level forces are relatively weak for changes in per capita and percentage expenditures in each category. National level forces explain 3 per cent of the variance for changes in per capita expenditures on economic improvements, 1 per cent for social improvements, 8 per cent for administration, and 3 per cent for changes in per capita total expenditures. The comparable figures for percentage rather than per capita expenditures for the first three categories are 4 per cent, 22 per cent, and 6 per cent, respectively. The relatively strong impact of national policy on changes in *percentage* expenditures on social improvements reflects the transfer of intermediate education from the local to the national government in 1964. Between 1964 and 1965, consequently, the mean percentage of total city expenditures spent on social improvements fell from 28.1 per cent to 19.0 per cent.

The Electoral and Budgetary Cycle

Fluctuations in expenditures were closely tied to the electoral cycle in Philippine chartered cities. Table 1 shows that expenditures tend to increase more rapidly before local or national elections than in off years. Expenditures on salaries and wages were even more sensitive to elections (see Table 2). Casual wage labor on public works projects initiated before elections was a common form of patronage. Since capital could not vote, incumbents tried to employ as many wage laborers as possible.

The electoral cycle had a similar impact on the national fiscal cycle. Data collected by Averch, Koehler, and Denton show unusually large deficits in net government operating receipts in the months preceding elections. Be-

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Act 4477 is as follows:

Revenue Class of City (Most to Least Revenue)	Percentage of General Fund
1	55
2	60
3-4	65
5	70
6	75
7	85

Source: Pagtakhan-Sumilong and Silao, eds., Table 2, p. 24.

[44] Between 1955 and 1966 the percentage of local revenues received from the national government rose from 31.4 and 43.3 per cent, according to the Joint Legislative-Executive Tax Commission, *Ninth Annual Report,* Table 2, p. 33. City expenditures constitute a mean of 25 per cent of total public expenditures in Philippine chartered cities (The Joint Legislative-Executive Tax Commission, *Tenth Annual Report,* Table 1, p. 41).

[45] A more complex version of the dummy-variable procedure for time-series data (two-way rather than one-way analysis of variance) is described in some detail by Robert W. Jackman, "Political Parties, Voting, and National Integration: The Canadian Case," *Comparative Politics,* 4 (July, 1972), 511–536.

Table 1. Change in Real Per Capita Expenditures of Philippine Chartered Cities

Fiscal Year	Interyear Change (Percentage)	Election	Real Per Capita Expenditures (Pesos)
1961			1613
1962	−3.7	National	1554
1963	1.0		1539
1964	9.9	Local	1708
1965	1.1		1727
1966	8.9	National	1880
1967	4.9		1972
1968	6.1	Local	2099
1969	−4.1		2013

Source: Chartered city budgets, Philippine General Accounting Office. The unweighted mean for expenditures is computed with an *N* of 32.

tween 1957 and 1968, net government receipts invariably showed deficits in election years and surpluses in nonelection years.[46]

At the national level, growing deficits during Philippine election years stimulated inflation and increased dependence on foreign creditors. Fiscal retrenchment in nonelection years starved or delayed projects initiated earlier. Both inflation and retrenchment encouraged defections from the ruling political coalition which ultimately defeated incumbents.[47] Since the structural obstacles to achieving solvency and self-sustaining economic growth were so great, regimes could not plan the necessary long-term austerities without losing their support.

As the expenses of Philippine elections grew throughout the 1950s and 1960s, and campaigns became more capital intensive (e.g., greater use of helicopters and trucks), the strain on the balance of payments increased. During the 1960s net foreign exchange reserves declined, and in September of 1969 net government receipts showed an unprecedented deficit.[48] Recurring foreign exchange crises, retrenchment and a fluctuating annual rate of growth frustrated planners and retarded growth in manufacturing. As Golay argues, "Such a 'stop-go' cycle in foreign exchange policy can only serve to perpetuate the structure of discretionary developmental protection which was used to initiate Philippine manufacturing development and thereby preclude, for the

Table 2. Change in Real Per Capita Expenditures on Salaries and Wages of Philippine Chartered Cities

Fiscal Year	Interyear Change (Percentage)	Election	Real Per Capita Expenditures on Salaries and Wages (Pesos)
1961			876
1962	.6	National	881
1963	.1		882
1964	12.4	Local	991
1965	−8.8		904
1966	11.5	National	1008
1967	2.9		1037
1968	4.7	Local	1086
1969	−2.4		1060

Source: See Table 1.

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[46] Harvey A. Averch, Frank H. Denton, and John E. Koehler, *The Matrix of Policy in the Philippines* (Princeton: Princeton University Press, 1971), pp. 99–101.

[47] Marcos is the only Filipino president to have been reelected. The poor showing of Marcos-backed senatorial candidates in the 1971 off-year elections indicated that, with high inflation and rising unrest, defections from the ruling coalition were once again occurring.

[48] Averch, Denton, and Koehler, pp. 97, 99, and 107.

foreseeable future, the competitive maturation of Filipino manufacturing entrepreneurs and their enterprises."[49]

Neither the Philippine Budget Commission nor departmental secretaries could block the use of special funds for political purposes. In order to maintain control over funds, legislators earmarked allocations for special purposes so that administrators could not channel money to what they considered priority projects. Between 1955 and 1967 special funds grew faster than the regular budget,[50] further assuring the inflationary impact of elections, and institutionalizing what technocrats often saw as wasteful expenditures (e.g., for mass primary education). Local budgeting, I shall argue, was similarly constrained.

Conditions other than the electoral cycle affected local budgeting, but in less predictable ways. For example, the national government did not always remit nor remit on time, the entirety of a city's internal revenue allotment. This failure, as well as fluctuations in locally collected income and corporate taxes, made the internal revenue allotment more unstable over time than other sources of revenue. Fluctuations in the percentage of excess individual and corporate income taxes collected by a city also undermined predictable budgeting. Republic Acts 781, 2343, and 6110 remitted to cities a percentage of national income taxes collected within city boundaries if income tax revenues exceeded the mean amount of income tax collected during the four preceding years. Unless local politicians could offer corporations and wealthy individuals with multiple branches or residences sufficient inducements (e.g., licenses, land rights, favorable zoning laws) to pay taxes in their city, convenience and counter pressures by Greater Manila Area politicians encouraged payment in the Greater Manila Area, where virtually all large corporations and members of the elite have offices or residences.

To the dismay of businessmen and planners, budgetary expenditures on economic improvements were more unstable over time than other budget categories (see Table 3). What technocrats often viewed as "wasteful" expenditures (social improvements, administration) were also the most institutionalized and difficult to cut. Both the Philippine constitution and the politics of pork barrel committed politicians to education. As mentioned previously, education had certain revenues tied to it. Furthermore, a high proportion of education expenditures were for salaries and wages, which were relatively inelastic. Administrative expenditures were quite stable also, reflecting, no doubt, the persistent effort by mayors to centralize power by placing key personnel in their office.

Table 3. Stability in Expenditures of Philippine Chartered Cities Over Time

Mean Correlations Between Adjacent Years, 1961–1969	
Percentage of Expenditures	
Economic improvements	.608
Social improvements	.868
Administration	.864
Per Capita Expenditures	
Economic improvements	.780
Social improvements	.979
Administration	.933
Total	.938
Correlations of 1961 Expenditures with 1969 Expenditures	
Percentage of Expenditures	
Economic improvements	.462
Social improvements	.753
Administration	.701
Per Capita Expenditures	
Economic improvements	.752
Social improvements	.874
Administration	.810
Total	.796

Source: See Table 1.

Community Structure, Politics and Expenditures

Large, economically diverse Philippine cities with strong tax bases[51] spent more on total mean per capita expenditures than did small cities (Table 4). While the strength of political machines had virtually no influence on total per capita expenditures, political mobilization did. A politically mobilized population increased pressures for more expenditures.

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[49] Frank H. Golay, "Forward," in *The Protection and Development of Philippine Manufacturing,* by Vicente B. Valdepeñas, Jr. (Manila: Ateneo University Press, 1970), p. xvi.

[50] Caiden and Wildavsky, *Planning and Budgeting,* p. 83.

[51] The simple correlation between city size and diversity and mean revenues per capita in Philippine cities between 1961 and 1969 is .64. Larger, more economically diverse cities are also less dependent on the national government for funds than are smaller cities. For example, city size and diversity correlates –.52 with the percentage of total revenues which are from the internal revenue allotment and –.33 with the percentage of revenues from national government aids.

Table 4. Regression of Sociodemographic and Political Data against Mean Percentage and Mean Per Capita Expenditures, Philippine Chartered Cities, 1961–1969

Dependent Variable	Independent Variable	Simple R	Beta	R^2
Mean Per Cent Expenditures				
Economic improvements	City size and diversity	–.390	–.251	.28**
	Political mobilization	.286	.123	
	Machine strength	.232	.137	
	Industrialization	–.453	–.281*	
	Size of lower class	.041	–.186	
Social improvements	City size and diversity	.719	.417**	.62**
	Political mobilization	–.158	–.071	
	Machine strength	–.438	–.170*	
	Industrialization	.655	.305**	
	Size of lower class	–.437	–.079	
Administration	City size and diversity	–.450	–.245	.34**
	Political mobilization	–.220	–.200	
	Machine strength	.334	.132	
	Industrialization	–.333	–.118	
	Size of lower class	.506	.205	
Mean Per Capita Expenditures				
Economic improvements	City size and diversity	.072	.069	.25**
	Political mobilization	.417	.274	
	Machine strength	.000	.102	
	Industrialization	–.107	–.154	
	Size of lower class	–.370	–.312*	
Social improvements	City size and diversity	.797	.745**	.66**
	Political mobilization	.031	.136	
	Machine strength	–.327	–.048	
	Industrialization	.530	.064	
	Size of lower class	–.534	–.023	
Administration	City size and diversity	.136	–.015	.11
	Political mobilization	.081	–.092	
	Machine strength	.039	.203	
	Industrialization	.029	–.031	
	Size of lower class	–.274	–.413*	
Total	City size and diversity	.523	.483**	.42**
	Political mobilization	.283	.253*	
	Machine strength	–.169	.070	
	Industrialization	.258	–.037	
	Size of lower class	–.546	–.223	

$^{**}p \leqslant .05 \qquad ^{*}p \leqslant .10$

Sources: Sociodemographic data – Bureau of Census and Statistics, *Census of the Philippines, Population and Housing*, 1960 and 1970; electoral data – Philippine Commission on Elections; economic data – data collected by the Philippine Bureau of Census and Statistics for the *Economic Census of the Philippines: 1967.*

The size of the lower class has no independent influence on the mean percentage of expenditures for social improvements. Lower-class groups must have power and organization to press demands. Generally, the larger the proportion of the population that is lower class, the more easily the population is controlled by patrons and the smaller the percentage of the population registered to vote.[52] Nor were relatively unmobilized, lower-class individuals as likely to desire intermediate and secondary education as were lower-middle class individuals.

If I assume that educational aspirations of the illiterate or semiliterate portions of the lower class focused on primary education, which was provided by the national rather than by local government, I must examine national expenditures on education to clarify the relationship between lower-class wants and public services. Table 5 shows a positive correlation and beta between the size of the lower class and

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[52] The percentage of the population registered to vote correlates –.35 with the size of the lower class in chartered cities.

Table 5. Regression of Sociodemographic and Political Data against Mean Per Capita Expenditures on Primary Education, Philippine Chartered Cities, 1961–1968

Dependent Variable	Independent Variable	Simple R	Beta	R^2
Mean Per Capita Expenditures on Primary Education	City size and diversity	–.574	–.174	.54**
	Political mobilization	.375	.466**	
	Machine strength	.374	.158	
	Industrialization	–.429	–.151	
	Size of lower class	.391	.365*	

$^{**}p \leqslant .05$ $^{*}p \leqslant .10$

Sources: Expenditures on education – Philippine Department of Education, Bureau of Public Schools, *Annual Budgets;* other variables – see Table 4.

mean per capita expenditures for primary education made by the national government between 1961 and 1968. This finding suggests that the lower class was sufficiently mobilized to press for certain types of demands and/or that elite paternalism and the ideological commitment of politicians to mass primary education made expenditures on education in less developed cities a good political, if not economic, investment.[53] In less developed cities, local political patrons retained more traditional power over voters. Consequently, the payoff per peso expenditure on education might have been higher in such cities since education was scarcer and votes were more easily delivered to national politicians.[54] A second important influence on expenditures for primary education shown in Table 5 is political mobilization.[55] Political mobilization created pressures for public services on both local and national politicians.

Both large, economically diverse cities and industrialized cities spent a larger mean percentage of their budgets on social improvements than did smaller, less industrialized cities (see Table 4). In the larger, diverse cities, apparently, lower and lower-middle class demands for free intermediate and secondary education took precedence over business demands for roads and bridges. Exposure of the lower class and the lower-middle class to the life styles of more affluent classes was most intensive in the large cities. Widespread literacy in such settings intensified the exposure of these classes to the wide variety of consumer and service goods advertised by large corporations and consumed disproportionately by the middle and upper classes. The deep sensitivity of Filipinos to cultural and economic status differences produced an insatiable hunger for education and consumer goods.

Cities with high levels of industrialization, furthermore, needed a skilled labor force that was literate and disciplined. Since "the Philippine economy has a unique demand for secondary graduates not matched in other countries,"[56] there might have been some convergence in the demands for free secondary education by the lower class, the lower-middle class, and business interests.

The other, more minor components of per capita expenditures on social improvements – garbage collection, parks and plazas, and local health services – filled needs most vigorously articulated by groups concentrated in large cities. While plazas are common in all Philippine cities, creating parks is a less pressing concern in rural cities which have relatively low population densities. Additionally, refuse in rural communities such as paper and cans is scarce, valuable, and likely to be quickly recycled by the consumers themselves.[57]

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[53] Expenditures on mass primary education in the Philippines may be an irrational use of resources, since the rates of return to Philippine educational capital generally are not impressive. The only exception to this generalization appears at the high school level where the rate of return is impressive. Jeffrey G. Williamson and Don J. De Voretz, "Education as an Asset in the Philippine Economy," in *Philippine Population in the Seventies,* ed., Mercedes B. Concepcion (Manila: Community Publishers, 1969), pp. 133–168.

[54] See Nowak and Snyder, Tables 12 and 13, pp. 1164–1165.

[55] Because there were literacy requirements for voting (albeit very lax), the relationship between educational expenditures and political mobilization or voting turnout is reciprocal. Rising expenditures on education raise literacy, which increases the percentage of the population eligible to vote. In turn, the existence of more voters increases pressures for social services such as education.

[56] Williamson and De Voretz, p. 165.

[57] In the most urbanized cities, a division of labor occurs between affluent consumers who create garbage, scavengers who help recycle it, and garbage

As shown in Table 4, political mobilization is not significantly related to the mean percentage of the budget spent for any of the major fiscal categories. While demands for public services were undoubtedly higher when a greater percentage of the population actually voted, I do not know the class composition of the voting population in Philippine chartered cities. Undoubtedly, politicians faced by diverse voters must provide a variety of public services. Cities with higher levels of political mobilization had more complex class structures than other cities do.[58]

Where intra-elite competition was not intense and political machines were strong, a smaller mean percentage of the cities' budgets was spent on social improvements and a larger mean percentage on economic improvements and on administration (see Table 4). As suggested previously, cohesive local elites and strong political machines could eschew alliances with lower-class groups and thus could better insulate themselves from demands for particular types of public services. Such elites at the same time could be more responsive to business interests and spend relatively more in centralizing the local administration.[59] Additionally, incumbents in cities with budgets containing a small percentage of school expenses (since these are relatively inelastic) had a greater margin to manipulate other portions of the budget to build patronage and maximize electoral support than did incumbents faced with large educational expenses. Since, as noted earlier, strong machines occurred more frequently in small, less diverse cities, expenditures on economic improvements might also reflect efforts by local elites to attract indigenous and foreign business. Economic growth in areas where the traditional basis of local elite power was relatively intact would, in the short run, disproportionately benefit local landowners, skilled workers, and capital.

Sociodemographic and political variables explain relatively little variance in changes in percentage of expenditures for any of the major fiscal categories (Table 6). As mentioned previously, the percentage of expenditures for various budget categories is more sensitive to national policy changes than are the variables comprising "expenditures per capita." Growth in political mobilization, however, increased the percentage of the budget spent for social improvements and decreased the percentage spent for economic improvements. A growing electorate shifted local expenditures from elite to mass services.[60]

An important question to consider is why change in political mobilization correlates positively with change in the percentage of the budget spent on social improvements, whereas mean political mobilization does not correlate positively with mean expenditures on social improvements. Growth in political mobilization is more strongly related to the size of the lower class than is mean political mobilization, and thus better measures literate lower-class voters' demands for social services. As in England, the middle classes in the Philippines were enfranchised before peasants and urban workers. The Philippine franchise grew slowly from the

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collectors who help dispose of it. Since garbage expands with affluence, cities with larger middle and upper classes have more garbage than other cities.

[58] The simple correlation between mean level of political mobilization between 1949 and 1969 and class diversity is .50. Class diversity is computed as follows: class diversity = 1 − [percentage of working population estimated as being: $(\text{professional})^2$ + $(\text{administrative})^2$ + $(\text{in sales})^2$ + $(\text{in farming})^2$ + $(\text{in mining})^2$ + $(\text{in transportation})^2$ + $(\text{in crafts and factory operatives})^2$ + $(\text{in service})^2$ + $(\text{in stevedoring})^2$] The estimates are from occupational statistics for urban populations in the provinces within which the chartered cities are located. See advanced reports for the Bureau of Census and Statistics, *Census of the Philippines, 1970, Population and Housing.* The formula for class diversity is adapted from Stanley Lieberson, "Measuring Population Diversity," *American Sociological Review,* 34 (December, 1969), 850–862.

[59] In his comparative study of two Philippine cities, Leichter found Bacolod City's mayoral incumbents (members of a cohesive socioeconomic, sugar-based oligarchy) particularly responsive to millers' and planters' needs for roads. Iloilo City, governed since 1955 by a political professional lacking land and business enterprises, did not show comparable sensitivity to demands for roads. The need to build up a political base through expansion of the city administration took precedence. Howard M. Leichter, *Political Regime and Public Policy: A Study of Two Philippine Cities* (Ph.D. dissertation, University of Wisconsin, 1973), pp. 214–220.

[60] This shift need not imply a redistribution of resources. Expanding public services may simply mean offering functional replacements for services formerly provided through traditional exchange with kin and patrons. Using cross-national data and three different measures of equality, Jackman finds no support for Lenski's argument that the extension of the suffrage in itself leads to greater equality. Robert W. Jackman, "Political Democracy and Social Equality: A Comparative Analysis," *American Sociological Review,* 39 (February, 1974), 29–45. In spite of growth in the percentage of Filipinos voting between 1956 and 1971, the proportion of income received by the bottom 49 per cent of the population has decreased. See Bureau of Census and Statistics, *Survey of Households, Family Income and Expenditures,* 1957 and 1971.

Table 6. Regression of Sociodemographic and Political Data against Changes in Percentage and Per Capita Expenditures, Philippine Chartered Cities, 1961–1969

Dependent Variable	Independent Variable	Simple R	Beta	R^2
Change in Per Cent Expenditures				
Economic improvements	City size and diversity	–.279	–.303	.25
	Changes in political mobilization	–.321	–.461*	
	Machine strength	.031	.012	
	Industrialization	–.115	.121	
	Size of lower class	.155	.222	
Social improvements	City size and diversity	.349	.328	.21
	Changes in political mobilization	.223	.308*	
	Machine strength	–.129	–.094	
	Industrialization	.270	.046	
	Size of lower class	–.106	–.015	
Administration	City size and diversity	–.249	–.210	.18
	Changes in political mobilization	.120	.196	
	Machine strength	.147	.086	
	Industrialization	–.320	–.249	
	Size of lower class	–.032	–.331	
Change in Per Capita Expenditures				
Economic improvements	City size and diversity	–.084	–.058	.13
	Changes in political mobilization	–.044	–.211	
	Machine strength	.235	.249	
	Industrialization	.065	.227	
	Size of lower class	.204	.231	
Social improvements	City size and diversity	.344	.479**	.44**
	Changes in political mobilization	.174	–.008	
	Machine strength	.072	.086	
	Industrialization	.419	.302*	
	Size of lower class	.260	.568**	
Administration	City size and diversity	–.022	.106	.22
	Changes in political mobilization	.292	.190	
	Machine strength	.405	.391*	
	Industrialization	–.002	.056	
	Size of lower class	.186	.024	
Total	City size and diversity	.274	.356*	.31*
	Changes in political mobilization	.083	–.079	
	Machine strength	.245	.334*	
	Industrialization	.324	.279	
	Size of lower class	.143	.314	

**$p \leqslant .05$ *$p \leqslant .10$

Sources: Socioeconomic and political data – see Table 4; budgetary data – see Table 1.

first election in 1907 through 1937, and during that period included less than 10 per cent of the population. The percentage of the population voting grew rapidly after women were enfranchised in 1938. By the 1950s the only major restrictions on voting were age and a rather lax literacy requirement.[61] Since literacy in recent years has grown most rapidly among the lower classes, these classes had the most rapid increase in political mobilization. The simple correlation between the growth in literacy in cities and growth in the percentage of the population registered to vote is .51. The size of the lower class correlates positively ($r=.33$) with growth in voting registration. Because the mean size of the voting population in cities more heavily "weights" the long-literate middle classes, it is not surprising that it correlates more strongly with mean percentage and per capita expenditures on social improvements.

The positive betas between change in per capita expenditures on social improvements and both (a) industrialization and (b) city size and diversity confirm the results using mean expenditures on social improvements as the dependent variable. But the strong positive relationships between the size of the lower class and change in per capita expenditures on social improvements does not confirm these results. Extensive national government expenditures on public primary education in less developed cities may have altered the demands of lower-class groups in such cities. As primary education became available to all, demands for secondary education rose. The more rapid growth in literacy between 1960 and 1970 among less developed cities (25.4 per cent) as compared with more developed cities (6.2 per cent) suggests that the national government has narrowed the literacy gap between these two types of cities. Nine-year means for per capita expenditures on social improvements are less likely to reflect this change in preferences than are measures of change.

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[61] Carl H. Landé, *Leaders, Factions, and Parties: The Structure of Philippine Politics*, Yale University Southeast Asia Studies Monograph Series No. 6 (New Haven: Yale University, Southeast Asia Studies, 1965), pp. 28–30.

A second possible contributor to the growth in per capita expenditures in cities with a large lower class is investment by indigenous and foreign elites in revenue-generating extractive industries and agribusiness. Agribusiness[62] correlates positively (r=.337) with changes in per capita expenditures on economic improvements and (r=.460) on administration, but less strongly (r=.282) with changes in per capita expenditures on social improvements. Political machines having close ties to owners and managers of giant agribusiness complexes that dominate the local economy were particularly receptive to business needs for public works such as roads. In such areas, the monopoly of corporations and landowners over employment and credit[63] kept laborers dependent and allowed political machines to build up the local political apparatus.

For changes in per capita expenditures, both agribusiness (r=.435) and machine strength (r=.245) measure forms of elite dominance over voters which supported the more rapid growth in expenditures on economic improvements and administration than on social improvements. For changes in the percentage of expenditures devoted to economic improvements, agribusiness has the strongest independent effect.[64] Growing total expenditures, made possible by rising revenues, in turn may strengthen political machines.

Employment in the Bureaucracy

As anticipated, the correlation between political mobilization and percentage of budget spent on salaries and wages is positive. Political mobilization, however, has no independent effect on the percentage of the budget spent on salaries and wages (Table 7), although it does have an independent and positive effect on the percentage of personnel expenditures spent on wages alone.[65] Politicians faced with a growing electorate need liquid resources to meet short-run demands for jobs. Rotating temporary wage labor on public works projects allowed politicians to spread jobs and expanded the percentage of personnel expenditures spent on wages alone.

The negative correlation between machine strength and percentage of budget spent on salaries and wages suggests that highly competitive elections made local politicians particularly eager to put their followers on the government payroll. As the size of the economically insecure electorate increased, the local boss was less able to keep rewards flowing at an acceptable rate. Local power centers proliferated and elections became more competitive, leading insecure politicians to employ even more allies and clients in the bureaucracy.

In other nations – i.e., those in which the socioeconomic elite is an administrative rather than a political elite, or those characterized by ideological mass parties – agencies faced by

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[62] "Agribusiness" represents a factor score which indexes such measures as mean employment in mining and manufacturing, mean fixed assets in mining and manufacturing, and per capita assets in mining and manufacturing. The factor matrix and rationale for using these measures are discussed in Nowak and Snyder, Table 1, pp. 1155–1156.

[63] See, for example, Nanette Garcia-Dungo, *A Southern Industrial Complex* (Quezon City: Community Development Research Council, University of the Philippines, 1969), and Frank Lynch, *A Bittersweet Taste of Sugar* (Quezon City: Ateneo de Manila University Press, 1970). Unlike the situation in colonial Java where companies owned or leased acreage on which to raise sugar, Philippine sugar centrals contract for sugar produced by local landowners. Areas bordering on sugar centrals thus contain the local landowning elite, tenants, and migrant contract labor used by planters during harvest.

[64] Adding agribusiness to the regression equation summarized in the top part of Table 6 yields betas of .286, –.145, and –.113 between agribusiness and change in percentage expenditures on economic improvements, social improvements, and administration, respectively.

[65] Substituting "percentage of salaries and wages spent on wages alone" for the "percentage of budget spent on salaries and wages" as the independent variable in the stepwise regression equation summarized in Table 7 changes the beta between political mobilization and the independent variable from .053 to .304.

Table 7. Regression of Sociodemographic, Political, and Administrative Data Against Percentage of Budget Spent on Salaries and Wages, Philippine Chartered Cities, 1961–1969

Dependent Variable	Independent Variable	Simple R	Beta	Partial Correlations for Variables with F<1.5 not in the Equation[a]	R^2
Per cent budget spent on salaries and wages	Per cent budget spent on social improvements	.247	.506**		.62**
	City revenue class	–.011	.394*		
	Administrative noncompliance	.204	.356**		
	Per cent civil service employees permanent	.261	.232		
	Machine strength	–.406	–.336**		
	Industrialization	.236		.177	
	City size and diversity	.237		.072	
	Size of lower class	–.228		.042	
	Political mobilization	.187		.053	

[a] Variables insufficiently strong to enter the stepwise regression equation.

$^{**}p \leqslant .05$ $^{*}p \leqslant .10$

Sources: Sociodemographic and political data – see Table 4; budgetary data – see Table 1; civil service data– Civil Service Commission.

strong local politicians with a cohesive mass organizational base may be more responsive to pressures for public services and jobs. Communist Italian communities, when not under heavy pressure to win middle-class votes through policies of fiscal moderation, have higher per capita operating expenditures than do non-Communist communities because of the party's strongly interventionist ideology.[66] As mentioned previously, elite participation in Philippine electoral politics (unlike in many other ex-colonies) began before Filipinization of the bureaucracy, producing a very weak bureaucracy. Consequently, administrators could only resist political interference in the appointment of public employees by taking severe risks, such as the possibility of losing appropriations.

Predictably, local governments which were administratively noncompliant – i.e., governments that did not comply with R.A. 4477 limiting personnel expenditures in the General Fund – spent a greater percentage of their budget on salaries and wages (Table 7). Large, more diverse cities were less likely to be administratively noncompliant (r=.33) and had more permanent civil service personnel (r=.40) than did smaller, less diverse cities. Possibly, small, less diverse cities with poorly trained budget personnel that were not part of the permanent civil service were particularly susceptible to pressures expanding personnel expenditures; or, local politicians in such cities had sufficient power to defy the law.

The percentage of the budget spent on social improvements, the city revenue class, and the percentage of civil service employees who are permanent all affect personnel expenditures in the anticipated manner (Table 7). The bulk of expenditures for social improvements was for education, and largely for teachers' salaries. Cities with a high percentage of permanent civil service employees had a high percentage of their budget committed to salaries and wages.

The aforementioned results help explain why larger and more industrialized Philippine cities with larger revenues actually spent a greater percentage of their budgets on salaries and wages than did other cities, in spite of Department of Finance guidelines to the contrary. The percentage of the budget spent on salaries and wages correlates weakly but positively with both industrialization and city size and diversity (Table 7). Large, economically diverse cities had weaker machines (r=–.34), spent more on social improvements (r=.71), and employed a higher percentage of civil service employees who were permanent (r=.40) than did other cities. Additionally, large cities channeled more of their expenditures through special funds which were exempt from ceilings on salaries and wages. Efforts to keep down personnel expenditures in large, diverse cities were undercut by special funds, high expenditures on education, weak machines, and rather lax enforcement of the regulation setting ceilings on salaries and wages.

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[66] Pietro Giarda, "Un'analisi statistica sui determinanti delle spese degli enti locali," in *Studi sulla finanza locale*, ed. Cesare Cosciani (Milan: Giuffrè, 1967), cited in Robert C. Fried, "Communism, Urban Budgets and the Two Italies: A Case Study in Comparative Urban Government," *Journal of Politics*, 33 (November, 1971), 1047.

Conclusion

As part of a patronage system with competitive elections, the Philippine local administrative and budgetary process prior to martial law was vulnerable to political exigencies. My data indicates that the Philippine political system was at least minimally responsive to certain needs of the masses – as indicated by the stability of expenditures for social improvements over time, the positive correlations between growth in expenditures for social improvements and political mobilization, and the negative correlations between expenditures for social improvements and machine strength.

The conflict between political access and order was apparent in the Philippines long before martial law. Tensions between technocrats and politicians, and differing governing styles among local politicians, reflected this conflict. Mayors who were educated in the United States or were heavily exposed to American administrative ideology (many of whom came from regional or national dynasties) frequently approached government with a zeal for efficiency and decried excessive political pressures.[67] Such mayors could often afford to push for greater planning since they tended to be stronger politically than *nouveaux riches* politicians with a weaker economic power base.[68] Often mildly reformist in their approach to local governance, local politicians from family dynasties emphasized efficiency in administration – which often served primarily middle- and upper-class interests. Insulating the administration was part of a governing style designed to deflate and limit lower-class demands.

At the local level, the exigencies of politics in more factionalized cities often hampered meaningful program planning. Still, local spoils did help lubricate a patronage system somewhat sensitive to local lower-class needs, particularly for voters who supported winning political candidates. Frequent turnover among local politicians and the fluidity of factions assured sporadic distribution of patronage to clients linked to different factions.

The growing influence of technocrats in the Marcos administration undoubtedly helped establish an antipolitical climate. The Marcos administration, using a plebiscite to legitimize the regime, officially closed Congress four months after the declaration of martial law. One of the regime's major rationalizations for continuing martial law presently is the excess of politics under the entrenched oligarchy in Congress, since the nationalist movement has largely been suppressed.[69]

Even prior to martial law, Marcos had made vigorous efforts to centralize and consolidate power. Averch, Koehler and Denton indicate that the Central Bank's cyclical pattern of expanding credit before elections and contracting credit after elections was modified in the late 1960s as the Marcos administration continued to increase expenditures rapidly in off-years.[70] Sustained deficit spending and a "judicious use of direct payoffs to barrio councils instead of relying upon the more traditional *lider* network"[71] assured Marcos's re-election in 1969 but also had its costs: devaluation in 1970, mounting inflation, rising urban and rural unrest, and a growing external debt. An atmosphere conducive to authoritarian rule was further encouraged by massive flooding in July, 1972, declining agricultural output, a growing trade deficit, rising unemployment, a fall in real wages,[72] and growing threats of American disinvestment in the face of greater nationalism. Increasingly, these problems were attributed to the excesses of politics. As Stauffer argues: "Any reading of the Philippine media and of the pronouncements of key Filipino administrators during this period attests to the reliability of this shift, as do statements by President Marcos himself. They had come to view the political process as inhibiting development: the solution increasingly proffered was a turn to authoritarianism and technocratic rule.[73]

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[67] In my open-ended interviews with 22 city mayors between 1969 and 1971, mayors from powerful families with extensive agribusiness and corporate interests seemed more development- and efficiency-oriented than did the upwardly mobile, political professionals. Many "patricians" were widely read, and were more willing to make use of technocrats as consultants for reform in city government or for planning integrated infrastructure projects.

[68] In his study of public policy in Bacolod and Iloilo cities, Leichter concludes that the "oligarchs of Bacolod, whose political support is based upon their economic dominance, have not found it necessary to politicize the bureaucracy in order to maintain their position and power." The middle-class politicians of Iloilo City, often mistrustful of the socioeconomic elite which dominated local politics before 1955, managed budgets in a far less programmatic manner, politicized their police department to a greater extent, and spent a smaller portion of their budget on infrastructure than their Bacolod counterparts. Leichter, Tables 5–1 and 6–4, pp. 158, 209, 217, quotation at p. 224.

[69] Sidney H. Schanberg, "Marcos Says He Must Keep Martial Law," *New York Times*, June 17, 1974, p. 7.

[70] Averch, Denton and Koehler, pp. 102–109.

[71] Jean Grossholtz, "Philippines 1973: Whither Marcos?" *Asian Survey*, 14 (January, 1974), 111.

[72] The Hongkong and Shanghai Banking Corpora-

At the local level, martial law curtailed the power of local officials. They lost some control over their police force to the local Philippine Constabulary detachment. Furthermore, in 1974 the central government took over certain revenues and capital equipment (e.g., for building roads) from local governments[74] and, with cause, could remove local officials. Through controlled referenda beginning in 1973, the regime repeatedly asked voters whether they would like to have local officials elected. A heavy "no" vote helped the regime discredit the legitimacy of local officials elected prior to martial law. Additionally, the regime established a Department of Local Government and Community Development "to police the local ranks and purge undesirables and 'backsliders', to use a New Society Term."[75]

Insulated from electoral and political pressures, the local and national bureaucracy will now undoubtedly serve as a more efficient arm of the executive. Who will benefit from this efficiency is less clear. The heavy emphasis of the new regime on foreign investment and labor discipline, and its reluctance to certify middle-class landholdings and estate agriculture for land reform[76] suggests a posture particularly sensitive to the middle and upper classes.

Furthermore, there is considerable question whether the high-growth high-investment strategy of the new regime will in fact rectify a pattern of economic and social dualism set down during the colonial era. In the absence of economic dynamism outside the "modern" sector, it is unlikely that the "trickle-down" effects of aggregate growth will offset growing maldistribution, particularly without conditions which press or predispose the regime – either through elections or through an "equity emphasizing Third World socialism"[77] – to favor distribution over growth.

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tion, "The Chairman's International Survey for the Year 1972," *Far Eastern Economic Review,* April 30, 1973, pp. 37–38.

[73] Stauffer, "Philippine Martial Law," p. 8, footnote omitted.

[74] "Martial Law Situation in the Provinces," *Pahayag,* September, 1974, p. 13.

[75] M. C. Castro, "All Local Officials Resign," *Pahayag* March, 1975, p. 1.

[76] See Benedict J. Kerkvliet, "Land Reform in the Philippines since the Marcos Coup" (paper presented at the annual meeting of the Association for Asian Studies, Boston, Massachusetts, April 1–3, 1974).

[77] The quotation is from Robert L. Ayres, "Development Policy and the Possibility of a 'Livable' Future for Latin America," *American Political Science Review,* 69 (June, 1975), 507–525, at p. 524. In criticizing assumptions underlying "conventional" development policy applied to Latin America, Ayres questions whether such policy is anything more than a blueprint for unbalanced, nonintegrated growth. His analysis, we feel, is equally applicable to the Philippines. Ayres does not prescribe "an equity-emphasizing Third World socialism," but suggests that it may be compatible with policy emphasizing distribution over growth.

Divisive Primaries Do Hurt: U.S. Senate Races, 1956—1972

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There are a number of "common sense" reasons for believing that a divisive primary will hurt a candidate's chances in the general election. Most often mentioned is the possibility that supporters of the losing candidate in the divisive primary will stay home or vote against the party during the general election. But the influence of divisive primaries may go beyond general party supporters and decrease the willingness of party workers to help campaign in the general election. Having given of their time in the primary campaign, party workers, especially those who supported the loser, may not be willing to volunteer to work again in the general election campaign. The impact of divisive primaries may extend to nonsupporters of the party. Independents and potential converts from the opposition party may be repelled by nasty charges made during the primary campaign.[1]

Despite these reasons, in the only published empirical analysis of the effect of divisive primaries on a candidate's chances of winning a Senate seat, Andrew Hacker concluded that

> a divisive primary, in and of itself, bears little relation to a candidate's prospects at the general election. The sorts of candidates who win and lose are much the same in contests where one or neither or both of the contenders have first undergone a divisive primary.[2]

Somewhat sadly,[3] this analysis of senatorial races from 1956–1972 (including the years covered by Hacker) clearly vindicates common sense: divisive primaries do hurt. As will be shown later, a logical flaw in the use of control groups in the original analysis misled Hacker; reanalysis of his own data also supports the conclusion that divisive primaries lessen a candidate's chances in the general election.

Divisive Primaries

This study employs a somewhat more restrictive operational definition of "divisive primary" than did the Hacker study. Here, any primary in which the victor has a margin over the next candidate of less than 20 per cent of all votes cast is considered a divisive primary. Hacker considered divisive all elections in which the winner received less than 65 per cent of the vote. Thus, for example, the 1972 Democratic primary in Oklahoma is not defined here as divisive even though Edmondson received less than 60 per cent of the vote, for he received more than twice the vote of any of his ten competitors.[4]

This study is based on 293 elections for seats in the United States Senate.[5] Of the 586 major party candidates, 105 or roughly 18 per cent, faced divisive primaries before the general election.[6] There were eight races where both candidates had tough primary battles; in 89 races there was a divisive primary in only one party. Candidates who survived divisive primaries were victorious in 23 of the 89 contests in which they were pitted against candidates who did not have a divisive primary, a success rate of only 26 per cent. This is roughly equal to the 27 per cent reported by Hacker. However, Hacker does not dispute the fact that candidates who face divisive primaries generally lose; what he contends is that they are candidates who would probably have lost anyway, based on the "traditional" factors of incumbency and party orientation.[7]

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[1] Andrew Hacker suggests the "common sense" reasons in his article, "Does a 'Divisive' Primary Harm a Candidate's Election Chances?" *American Political Science Review*, 59, 1 (March, 1965), 105–110. The hypothesis that a divisive primary is likely to cause a loss of party workers in the Fall has been substantiated at the congressional level in Iowa by Donald Johnson and James Gibson, "The Divisive Primary Revisited: Party Activists in Iowa," *American Political Science Review*, 68, 1 (March, 1974), 67–77.

[2] Hacker, p. 110.

[3] Somewhat sadly, I say, because I am indebted to Professor Hacker for those hours in the classroom some years ago when he tried to instill in me the right mind-set to question his work. I half suspect he designed his article as a challenge and has waited for the past nine years for someone to take him up on it.

[4] It seems clear that "divisive" (Hacker's quotation marks) is an unclear, emotion-laden term. Perhaps "close" would be better – certainly it would more clearly imply the operational definition used here. However, "divisive" is the prevalent term in the literature, and to tie in with that literature, "divisive" is used here as well.

[5] I have not included in this analysis a few races in which a single candidate was unopposed; in which a third candidate received more than 20 per cent of the vote; or in which a candidate died after the primary, but before the general election. Also excluded are nonpartisan special elections.

[6] This compares with the 35 per cent that Hacker found using the broader definition.

Incumbency

An incumbent can most easily be defined as one who holds the office that is being sought – regardless of how long he has held that office. Clearly incumbent senators were more successful in general elections than other candidates, winning reelection in 192 of 222 attempts (86 per cent). Incumbents rarely had to face divisive primaries before the general election; in only 12 races had they overcome stiff primary opposition (5 per cent). Challengers, on the other hand, had to survive divisive primaries in 22 per cent of their races. Thus, the incumbency factor does give some support for the position that many of those who went through divisive primaries were likely losers, because they were challenging incumbents.

However, that view does not preclude the possibility that divisive primaries made the incumbents' task harder and the challengers' task virtually impossible.

As Table 1 shows, both incumbents and challengers did worse when they had to face a divisive primary and their opponent did not. Looked at from the challengers' point of view, the differences are especially dramatic: challengers upset incumbents only 6 per cent of the time when they had stiff primary opposition and the incumbent did not; yet they pulled upsets more than 5 times as frequently when it was the incumbent who had the primary opposition, while they breezed through.[8]

Divisive primaries were also a factor in the races which pitted nonincumbents against each other. There were 29 races in which one nonincumbent had to overcome a divisive primary, while the other did not have to; those who had gone through divisive primaries won just 12 races.[9]

These differences show that divisive primaries hurt incumbents, nonincumbents, and challengers to some degree. However, when we further control for the party orientation of the state, the consistently negative influence of divisive primaries will become clearer, and we can note those conditions under which divisive primaries have an especially strong impact.

Party Orientation of the State

Depending upon the state, a candidate may receive a marked advantage in the general election just by winning the nomination of a specific party. In a few states during this time period, receiving the Democratic nomination was tantamount to being elected. Obviously, a divisive primary in either party in such a state is unlikely to have much influence on the outcome of the general election. Even in states which are not quite so one-party dominant, the Republican or Democratic nomination may convey a sufficient edge so that the candidate of the weaker party would only expect to win if there was a sharp division within the stronger party. In this case, divisions within the weaker party are likely to merely shift its chances of winning from small to insignificant. In states in which the two parties are about equally competitive, the influence of divisive primaries is likely to be the greatest, for the divisions in the primary may be just enough to tip the balance to the opposition.

Hence, for our purposes it is sufficient to look at three categories of states: (a) one-party dominant, (b) two-party competitive, and (c) all others (those in which one party is appreciably stronger than the other, but not so

Table 1. Divisive Primaries and the Fate of Incumbents

	Only Incumbent Had Divisive Primary	Neither or Both Had Divisive Primary	Only Challenger Had Divisive Primary
Won	8	139	45
Lost	4	23	3
Percentage of races won by incumbent	67%	86%	94%

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[7] Hacker, p. 110.

[8] The difference in winning percentage between incumbents who overcame divisive primaries and incumbents who faced challengers who won divisive primaries is statistically significant at the .05 level.

[9] This difference is not statistically significant at the .05 level.

Table 2. States Classified by Party Orientation

One-Party Dominant	Two-Party Competitive	Democrats Stronger	Republicans Stronger
Alabama	California	Alaska	Kansas
Arkansas	Colorado	Arizona	Nebraska
Georgia	Delaware	Connecticut	New Hampshire
Louisiana	Idaho	Florida	North Dakota
Mississippi	Illinois	Hawaii	Vermont
Missouri	Indiana	Kentucky	Wyoming
Nevada	Iowa	Maine	
North Carolina	Michigan	Maryland	
Rhode Island	New Jersey	Massachusetts	
Texas	New York	Minnesota	
	Ohio	Montana	
	Oregon	New Mexico	
	Pennsylvania	Oklahoma	
	Utah	South Carolina	
		South Dakota	
		Tennessee	
		Virginia	
		Washington	
		West Virginia	
		Wisconsin	

strong as to make party nomination tantamount to election).

These categories are operationally defined by using the median of the biennial averages of the percentage of the party vote in the combined gubernatorial, senatorial, and congressional races in each state going to each party.[10] One-party dominant states are defined as those in which a single party had a median vote of more than 55 per cent and never won less than 52.5 per cent of the composite two party vote in any year (1956–1972). Two-party competitive states are defined as those where the median vote for neither party exceeded 52.5 per cent and the vote for neither party exceeded 60 per cent in any one year. In the other states, the party with the higher median vote was deemed the stronger party.[11]

When we just looked at incumbency as a control variable, the influence of divisive primaries was somewhat muted, because incumbency depresses primary challenges. However, if we further control for state party orientation, we can see that – for incumbents, nonincumbents, and challengers alike – increasing the chances of victory in the general election encourages primary competition.[12]

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[10] With some minor adjustments this is Paul David's "composite B" in his *Party Strength in the United States, 1872–1970* (Charlottesville: University Press of Virginia, 1972), Part II. Biennial data for 1970 was calculated where missing, and estimated for 1972, where it would influence classification. David interpolated where necessary to obtain a biennial dataseries for each type of race, then he simply averaged the three percentages.

[11] This classification differs somewhat from the Hacker classification, but the results of the later analysis are essentially the same using either classification scheme.

[12] This is essentially the position of V. O. Key, Jr. in his *Politics, Parties and Pressure Groups*, 5th ed.

Table 3. Percentage of Candidates in Divisive Primaries by Party Orientation and Incumbency[a]

	Party Orientation		
Type of Candidate	Candidates of Dominant or Stronger Party	Candidates of Equally Competitive Party	Candidates of Minor or Weaker Party
Incumbents	8% (120)	6% (65)	3% (37)
Nonincumbents	38% (52)	32% (38)	17% (52)
Challengers	32% (37)	26% (65)	18% (120)

[a] Figures in parentheses are *N*s.

The data in Table 3 directly contradict the Hacker contention that those who engaged in divisive primaries were those who were likely to lose in any case. Primary battles were almost twice as likely in the stronger party as in the weaker party.

Since the smell of an easy victory seems to draw candidates, it is especially significant that the eventual nominee is later defeated with such regularity. Now we turn to a more detailed analysis of the effects of divisive primaries under differing conditions of party orientation.

One-Party States

In the one-party states both divisive primaries and upsets are such rare events that the data can do no more than suggest the harmful effects of divisive primaries on election chances. In 48 races the minor party won just three times. Two of these followed a divisive primary within the dominant party: Helms over Galifianakis in North Carolina, and Tower over Sanders in Texas, both in 1972.

Unbalanced State Competition

Table 4 indicates that where state competition is unbalanced, divisive primaries within the stronger party significantly handicapped incumbents, nonincumbents, and challengers. Divisive primaries only within the weaker party significantly hurt the prospects of nonincumbents, but did not significantly affect the chances of incumbents or challengers.[13]

Stronger-party incumbents won 95 per cent of the races in which they did not have to overcome the handicap of divisive primaries, but only 60 per cent of the races in which they did. Challengers from the stronger party were unsuccessful in ten tries after overcoming stiff intraparty competition; they won 30 per cent of the time when they had only limited opposition from within their own party. Where two nonincumbents were competing, the stronger party won all six races where only the weaker party had a divisive primary, 73 per cent of the races where neither party had a divisive primary, and just 53 per cent of the races where the stronger party had a divisive primary. Thus, the winning percentage for candidates who faced divisive primaries within the stronger party was 26 to 35 points lower than the winning percentage of similar candidates who did not face such a primary.

Two-Party States

The races in states where the two parties were almost equally balanced generally involved incumbents trying for reelection. There were only 19 races in which neither candidate was an incumbent, and just six of these pitted a candidate who had gone through a divisive primary against one who had not. Despite their small number, these races afford some support for the thesis that divisive primaries hurt, for the candidates who had the easier time of it in the primary won four of the six races.[14]

The 65 races which involved incumbents offer much more dramatic support for the thesis. As Table 5 shows, where there was no divisive primary in either party, incumbents were generally reelected; where incumbents had faced divisive primaries, they generally lost; and where incumbents were lucky enough to face challengers who were burdened with divisive primaries, the incumbents almost always won.[15]

Table 4. Percentage of Races Won by Candidates of the Stronger Party: Unbalanced Competitive States[a]

	Type of Primary		
Type of Candidate	Divisive within Stronger Party	Neither Divisive	Divisive Only within Weaker Party
Incumbents	60% (5)	95% (62)	94% (17)
Nonincumbents	53% (15)	73% (22)	100% (6)
Challengers	0% (10)	30% (23)	0% (1)

[a] Numbers in parentheses are *N*s.

(New York: Thomas Y. Crowell, 1964), pp. 437–439. Hacker also reports an overall 2:1 ratio of primaries in "friendly" states to primaries in "hostile" states.

[13] "Significant" here refers to statistically significant differences in winning percentage (at the .05 level) between those who had divisive primaries and those, of the same type, who did not.

[14] This difference is not statistically significant.

[15] The differences among the winning percentages in Table 5 are significant at the .05 level.

Table 5. The Fate of Incumbents in Two-Party Competitive States

	Incumbent Had Divisive Primary	Neither Had Divisive Primary	Only Challenger Had Divisive Primary
Won	1	33	15
Lost	3	12	1
Percentage of races won by incumbent	25%	73%	94%

Why the Two Studies Differ

Despite slight differences in definitions and years covered, the Hacker data actually strongly support the proposition that divisive primaries hurt. In the conclusion of the original analysis, Hacker chose to treat divisive primaries as though they were attributes of the *elections,* rather than of the *candidates.*[16] Thus, he compared, for example, the percentage of stronger-party candidates winning when *either one of the candidates had had a divisive primary* with the percentage of stronger-party candidates winning when *neither or both had had a divisive primary* But he did not compare the percentage of stronger-party candidates winning after *the nominee of the stronger party had had a divisive primary* with the percentage of stronger-party candidates winning when *the nominee of the stronger party had not had a primary battle.* Making the more appropriate comparison, we find significant support within the Hacker data for the position that divisive primaries are a handicap.

Thus, the data from the two studies substantially agree in indicating that divisive primaries in either the stronger or weaker party significantly reduce its chances of winning.

Conclusions – And an Implication for Representation

The data consistently support the thesis that divisive primaries reduce a candidate's prospects for winning the general election. Hacker contended that the apparent relationship between divisiveness and electoral success was spurious, that the real causes of success were incumbency and state party orientation. This analysis has shown that contention to be untrue. Even controlling for those variables, divisive primaries do hurt.

This relationship has an important implication regarding representation. The fact that divisive primaries do hurt strengthens the representation of weak and minor state parties in the U.S. Senate. Where there is a difference in strength between state parties, divisive primaries are more frequent in the stronger than in the weaker party. Further, a divisive primary in the stronger party significantly hurts its candidate's prospects of winning, while a divisive primary in the weaker party rarely affects its candidate's chances of winning. Therefore, the effect of divisive primaries is generally to increase the number of senators representing

Table 6. Comparing the Two Studies: Winning Percentage of Stronger-Party Candidates (Unbalanced Competitive States)[17]

Percentage of Races Won by Stronger-Party Candidates	Type of Primary: Divisive Only within Stronger-Party	Type of Primary: Neither or Both Divisive	Type of Primary: Divisive Only within Weaker Party
Hacker study	33%	68%[a]	85%
This study	35%	77%	92%

[a] Estimated from combined gubernatorial-senatorial data in Hacker article.

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[16] I am indebted to James L. Payne for this succinct statement of the flaw in the original analysis.

[17] Hacker data based on Tables I, II, and VII, plus the discussion on p. 107. Because the Hacker study frequently combines gubernatorial and senatorial races, it is impossible to make other comparisons for just the Senate races. It would appear that divisive primaries have also hurt somewhat in gubernatorial races. For example, the combined figures show incumbents burdened with divisive primary winning just 60 per cent of the general elections; incumbents on an "equal footing" regarding primary experiences with their opponents winning 72 per cent of the time; and incumbents helped along by the challenger having a primary fight winning 82 per cent of the races.

the weaker party in their state. Once elected, those senators are hard to dislodge (in the 1956–1972 period, minor and weaker party incumbents won 78 per cent of their reelection races).

As of 1974, 22 senators represented the weaker party in their respective states. Twelve of these (55 per cent) were first elected when the stronger party had a divisive primary, and two others won when a dissident member of the stronger party ran as an independent.[18] In contrast, of the 49 senators who represented the stronger party in their respective states, only 5 (10 per cent) were first elected when the weaker party had a divisive primary.[19]

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[18] The twelve are: Stevens, Fannin, Gurney, Cook, Beall, Jr., Brooke, McIntyre, Domenici, Helms, Thurmond (in his first race as a Republican), Baker, and Brock. Mathias and Weicker received help from Mahoney and Dodd, respectively, one-time Democrats running as independents.

[19] Gravel, Dole, Metcalf, Hruska, and Abourezk. The difference in percentages between the weaker and stronger party representatives is statistically significant at the .01 level.

Authority and Community: The Contributions of Carl Friedrich, Yves R. Simon, and Michael Polanyi*

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To pieces is our world now going,
What's fitting loses all its might;
How ever shall that sense be growing
Which, only, leads us to the Right?

–Goethe, *Faust: Part II*, Act 1

The problem of authority in the modern world has both a theoretical and a practical side. The theoretical problem is that authority as a concept and as a phenomenon is often confused with power, force, or coercion – power "plus" perhaps consent or legitimacy. Thus it is viewed with suspicion. I intend to demonstrate, however, that authority does not add something to power; rather, it transforms power. When authority is understood to depend upon community and right, in the manner I shall suggest in the body of this essay, then its radical differences from power should be clear. Like power, force, and coercion, authority is directive, but it is so in a way utterly different from them.

The practical problem of authority is more striking than the theoretical. Men can live comfortably with considerable looseness in their conceptual language, but the existential consequences of the problem of authority present serious dangers to modern public life. Weak commitment to authority produces three related consequences. First, the decay of authority means the decay of political legitimacy. If its citizens perceive a state as legitimate, then, *ceteris paribus*, they will feel themselves bound by its laws, regulations, and rules. In the modern world, as John H. Schaar has written, ". . . we are nearing the end of an era. . . ," because ". . . the philosophical and experiential foundations of legitimacy in the modern states are gravely weakened, leaving obedience a matter of lingering habit, or expediency, or necessity, but not a matter of reason and principle, and of deepest sentiment and conviction."[1] Authority depends upon standards of right and on cultural values in order to imbue power with morality. To the extent that community has given way to a society of separate individuals and groups, authority loses its ground and turns into compromises of power. Common commitment to values and norms is replaced by processes of interest-group accommodation.[2] The legitimacy of policy thus formulated is called into question.

Second, the practice of morality is made difficult by the crisis of authority. Moral principles may be discovered by individuals in solitude. This is a lesson we learn from Socrates and Christ. The practice of such principles and the firm grasp of them, however, require social support. This we learn from the outline of the just *polis* in the *Republic* and from the early Christian community in the Acts of the Apostles. The community and its authoritative institutions (including, but not limited to, government) provide a supportive context in which the practice of virtue by individual persons is reinforced and made a fixed disposition:

> Unless the principles of morality are embodied in the collective life of the group – unless individual conscience is constantly comforted, stimulated, controlled, by the reactions of the group, protected by the power of collective conscience against its own fallacies, against its own weakness, against its own inclinations toward a comfortable blindness, the worst possibility [that ethical truth will disappear] will materialize often.[3]

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[1] "Legitimacy in the Modern State," in *Power and Community*, ed. Philip Green and Sanford Levinson (New York: Vintage Books, 1970), p. 279.

[2] "Legitimacy in the Modern State," esp. pp. 285–288, 297–301, 311–315. See also Theodore Lowi, *The End of Liberalism* (New York: Norton, 1969), *passim*, esp. pp. 93–97, 287–314, for a discussion and examples of the crisis of public authority.

[3] Yves R. Simon, "Beyond the Crisis of Liberalism," in *Essays in Thomism*, ed. Robert E. Brennan, O. P. (New York: Sheed and Ward, 1942), p. 273. For

As the modern world succumbs to the temptations of moral individualism, moral integrity becomes more and more difficult.[4]

Finally, suspicion of authority ends in the decay of visions of the good society. Emphasis is placed on the value of freedom and on the use of freedom to satisfy private wants, thereby glossing over the question of the best way of life. Yet obligation and authority are difficult to reconcile with such a view. Obligation becomes simply one more strategy for interest maximization.[5] Any conception of a common way of life as a candidate for the best way of life is excluded. If authority is to have any meaning, it must be grounded in a public conception of the best life and of the good society, which may serve as valid reasons for obedience to laws designed to pursue them. The relationship between authority and community suggested in this essay is designed to meet such criticisms as that of Stephen Salkever that neither liberalism nor contemporary community theory addresses the fundamental question of the desirability of certain ways of life.[6] A properly developed theory of authority and community does depend on a definite view of individual and social life.

The Concept of Community

I have suggested above an intimate connection between authority and community. Indeed, the thesis of this essay is that authority depends upon community. Authority can exist only within the context of a community; authority can exist only where there is a community to anchor and buttress it. Power is present in any human group; yet if authority is absent, no morality informs political life and its directives. Hence, there is no moral obligation to obey the directives of such a group. But there can be no authority without community in the group. A word should be said about community before proceeding to the examination of this thesis.

It will not do either to romanticize community or to scorn it. It is a grave mistake to conceive of "the good old days" as times of strong community life and thus to contrast them with the present. Clearly there is today a crisis in and a decay of community. Yet full community has at all times been a goal, not an achievement. We forget the troubles of the past and our own strengths if we place community in a nonrecoverable "Golden Age." On the other hand, it will not do to see community as a condition of utter conformity and stifled individuality. This is the temptation of community (just as anarchy is the temptation of liberalism). But community need not fall to the temptation. A proper understanding of authority and community helps us avoid both temptations. Each genuine community is a "community of the free"; each community mixes individual choice and communal rules, organic evolution and purposive striving, law and love.[7] The identifiability of a group of persons as a distinct group does not make it a community; nor does contiguity of residence, though often the term is used loosely for any residential unit – neighborhood, town, or city. Similarly, the simple fact of sharing a common occupation, ethnic heritage, or belief does not form a community. For example, the collectivity of those who accept the efficacy of modern medical procedures does not constitute a community.

Rather, a community is a group of persons who share a basic human value and who, at least to some extent, are aware that they share it. This sharing and awareness create an internal bond among them, making them fellows in the same community, setting them apart from others, and conferring an individual and collective identity which is special and unique. Values, traditions, and events are experienced as affecting not only their individual lives, but also their common life. The members of a community sense that they share the same

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a discussion of this point as it appears in Hegel, see Judith N. Shklar, "Hegel's *Phenomenology:* The Moral Failures of Asocial Man," *Political Theory,* 1 (August, 1973), 259–286.

[4] Tocqueville meant something like this when he discussed the individualism spawned by democracy. "Individualism is a mature and calm feeling, which disposes each member of the community to sever himself from the mass of his fellow-creatures, and to draw apart with his family and his friends; so that, after he has thus formed a little circle of his own, he willingly leaves society at large to itself." *Democracy in America,* trans. Henry Reeve (New Rochelle, N.Y.: Arlington House, n.d.), part II, book 2, chap. 2, p. 104.

[5] Stephen G. Salkever, "Virtue, Obligation, and Politics," *American Political Science Review,* 68 (March, 1974), 78–92. See also Brian Barry, "Liberalism and Want-Satisfaction: A Critique of John Rawls," *Political Theory,* 1 (May, 1973), 152–153.

[6] Salkever, p. 90.

[7] See Yves R. Simon, *The Tradition of Natural Law,* ed. Vukan Kuic (New York: Fordham University Press, 1965), p. 96; and Carl J. Friedrich, *Man and His Government* (New York: McGraw-Hill, 1963), pp. 138–144, and "The Concept of Community in the History of Political and Legal Philosophy," in *Nomos II: Community,* ed. Friedrich (New York: Liberal Arts Press, 1959), pp. 3–24.

commitments and traditions and, to a large extent, the same experiences. They may, as a community of doctors, for example, share a commitment to the use of the believed-efficacious techniques of modern medicine. They may, as a community of scientists, share a commitment to the truth-revealing power of scientific theory and method. They may, as a family, share a common life of joys and sorrows. They may, as a nation, share a common way of life, rooted in heritage and creed. They may, as a church, share a faith and a hope, and a life of action in faith and hope. In all of these cases, each member of the community possesses a belief or a way of life which proceeds from the core of his humanity, and each is aware that he shares this belief or life with others (though he need not know each of them) who sustain and encourage him in significant ways. Robert A. Nisbet speaks of community in a similar way:

> The word . . . encompasses all forms of relationship which are characterized by a high degree of personal intimacy, emotional depth, moral commitment, social cohesion, and continuity in time. Community is founded on man conceived in his wholeness rather than in one or another of the roles, taken separately, that he may hold in a social order. It draws its psychological strength from levels of motivation deeper than those of mere volition or interest, and it achieves its fulfillment in a submergence of individual will that is not possible in unions of mere convenience or rational assent. Community is a fusion of feeling and thought, of tradition and commitment, of membership and volition. It may be found in, or be given symbolic expression by, locality, religion, nation, race, occupation, or crusade.[8]

As Yves R. Simon puts it, ". . . the most important part of community life takes place in the heart of man."[9] Because it goes to the heart, then, community is an integral part of social life. To be without a community is truly to be alienated from the deepest roots of human existence.

It is community in this sense which is the ground of authority. In the sections which follow, I attempt to show how this understanding informs the work of Carl J. Friedrich, Yves R. Simon, and Michael Polanyi, and how their discussions of authority significantly bolster the thesis that authority is possible only within a community. I have selected these authors not only for the cogency of their arguments, but also for the diversity of their approaches. They come at the concept of authority from three quite different sets of philosophical and metaphysical principles. Such diversity of starting points should add weight to the soundness of whatever common conclusions about authority and community they reach.

Carl Friedrich: Authority and Community

I begin with Friedrich because of the three theorists, he has given the best-known defense of the importance of the concept of authority and of its dependence upon community. His discussion of authority, however, is also the least theoretically sound of the three authors I shall consider. The deficiencies of his treatment of authority, therefore, may anticipate the strengths of Simon and Polanyi.

Friedrich's development of the concept of authority ties it closely to tradition, legitimacy, and community. Because of these ties, authority is to be distinguished sharply from power. "Authority is not a kind of power, but something that accompanies power. It is a quality in men and things which enhances their power, something which creates power but is not itself power."[10] Command is not the whole of power; men follow from fear, but also from consent. The transmutation of coercion into consent is part of the skillful use of power. These two kinds of power distinguish the leader from the ruler. The latter depends primarily upon coercion; the former upon consent.

Authority, then, is a source of power, primarily of consensual power. Authority may be defined as the capacity to issue communications permitting reasoned elaboration in terms of the values, beliefs, and interests of the political community to which they apply.[11]

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[8] *The Sociological Tradition* (New York: Basic Books, 1966), pp. 47–48.

[9] *Tradition*, p. 96.

[10] Carl J. Friedrich, *An Introduction to Political Theory* (New York: Harper & Row, 1967), p. 126. For elaboration of the distinction see *Man and His Government*, chap. 9; *Tradition and Authority* (New York: Praeger, 1972), chap. 9, and *Introduction to Political Theory*, pp. 122–124. Similar criticisms of the idea of authority as a particular form of power may be found in Sebastian de Grazia, "What Authority is *Not*," *American Political Science Review*, 53 (June, 1959), 321–331; Robert A. Nisbet, "The Twilight of Authority," *The Public Interest*, No. 15 (Spring, 1969), pp. 3–9 and "The Nemesis of Authority," *The Intercollegiate Review*, 8 (Winter–Spring, 1972), 3–13, and Hannah Arendt, "What is Authority?" in *Between Past and Future* (Cleveland: Meridian Books, 1963), pp. 97–106.

[11] Cf. Friedrich, *Man and His Government*, pp. 224–226; *Tradition and Authority*, pp. 46–48; "Au-

Friedrich traces the word "authority" back to the Roman *auctoritas.* The Roman Senate added *auctoritas* to popular laws by "augmenting" them with the truths of the past or of religion. The *auctoritas* was advice which could not properly be disregarded. "It was a matter of adding wisdom to will, reason to preference, a knowledge of values shared and traditions hallowed, to whatever the people wanted to do."[12] Authority, then, for Friedrich, is connected with reason; its first property is the potential for reasoned elaboration of the communication, law, rule, or decision issued. The reasoned elaboration need not be carried out; the potentiality for it, and the recognition of this potentiality, are all that need be present. The elaboration is "reasoned" because it contains adequate reasons for the recipient. It refers to a careful consideration of alternative courses of action. They need not be conclusive reasons. "But whether they are set forth or not, these reasons exist, and they are what gives the law authority."[13]

This capability of reasoned elaboration is a capability in terms of a set of values which the person possessing authority shares with his followers. Here is the link for Friedrich between authority and community. "What matters is that this capacity to issue communications which may be elaborated by reasoning is there, that it exists, and that as a consequence a community of opinions, values, and beliefs, as well as of interests and needs, is manifested in such communications."[14] Community in the sense specified earlier in this essay serves as the foundation of authority. As Friedrich himself points out, "the stress our analysis thus far laid upon values as the progenitors of authority . . . expresses a political philosophy that emphasizes political co-operation and community rather than control and power."[15]

Authority, then, exists to relate particular judgments to the basic values of a community, "to broader verities, to values and beliefs that transcend the particular judgment involved in a concrete situation."[16] Authority, and the power based upon it, has a fluid quality, because it is based upon values, beliefs, and interests which constantly change in response to changes in the environment or to creative innovation.

The genesis of authority in the shared values of a community suggests authority's connection with tradition, since tradition is an important carrier of value. Thus, reasoning from tradition is a key method of authority. Tradition transmits a heritage of values, beliefs, and information from one generation to another. Yet because the tradition must be interpreted, it is capable of change, though tradition may hinder change on occasion.[17] It follows from this, according to Friedrich, that tradition is related to the very basis of politics. While brief periods of revolutionary fervor may do without tradition, political activity depends upon tradition; revolutions soon develop their own traditions. Friedrich is careful to distinguish tradition from traditionalism, an ideology absolutizing tradition.[18]

While the points above could stand further elaboration, it can be concluded at this juncture that Friedrich has made a significant contribution to the understanding of authority by showing it to adhere in communications capable of reasoned elaboration and by demonstrating how this capability must be rooted in communal values and, in most cases, in traditions embodying these values. Yet there seems to be something missing. Communal values and traditions point beyond themselves; they do not claim to be their own warrant. They are warranted, or claim to be warranted, by their accord with truth and a reality which transcends them and of which they are only immanent manifestations. Even authority itself points beyond tradition and community, as we can see when the people of Capharnaum marvel about Jesus that he "was teaching them as one

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thority, Reason, and Discretion," in *Nomos I: Authority,* ed. Friedrich (Cambridge: Harvard University Press, 1958), p. 34, and *The Philosophy of Law in Historical Perspective,* 2nd ed. (Chicago: University of Chicago Press, 1963), p. 203.

[12] *Man and His Government,* p. 219.

[13] *Man and His Government,* p. 222; see also pp. 220–221, and *Tradition and Authority,* pp. 51–56.

[14] *Man and His Government,* p. 224. Cf. *Tradition and Authority,* chap. 5.

[15] *Tradition and Authority,* p. 62. For Friedrich's concept of community see his "The Concept of Community"; also *Man and His Government,* pp. 38–44, 136–154, and *Introduction to Political Theory,* pp. 90–104.

[16] *Tradition and Authority,* p. 58. For a similar view, Peter Winch, "Authority," in *Political Philosophy,* ed. Anthony Quinton (New York: Oxford University Press, 1967), pp. 97–111.

[17] *Tradition and Authority,* pp. 16–18, 65.

[18] *Ibid.,* chap. 2. An even stronger emphasis on the necessity for tradition grounding authority is made by Arendt, *Between Past and Future,* pp. 91–141. See also Michael Oakeshott's conception of politics as attending to the traditional arrangements of a community, *Rationalism in Politics* (New York: Basic Books, 1962), esp. pp. 111–136. See also Winch, pp. 107–109.

having authority, and not as the Scribes."[19] They recognize that he does not speak only within the rabbinical tradition, but as though he had direct access to the reality to which that tradition pointed. An authority, then, must be a witness – a witness not only to a tradition, but to the truth which validates the tradition. Yet, paradoxically perhaps, as my analysis of Polanyi will show, truth is accessible only within a commitment to authority.

Friedrich himself senses the need to go beyond tradition and communal values in his discussion of authority's relationship to justice. Authority is seen as the link between justice and power. Justice is defined in the political sense as

> . . . the epitome of all that is in conformity with the communal values, opinions and beliefs, and more particularly therefore with what can be authoritatively stated as satisfying the demand for equality.
>
> The bald statement that political authority is just provides a shorthand expression for the fact that authority in so far as it is the capacity of reasoning upon values provides the bridge between power and justice. In that perspective it would be possible to assert the proposition that authority justifies power, provides power with an explicit argument for the relations to values prevalent in a given community.[20]

Friedrich seems to sense that this definition of justice makes authority and justice relative to any system of values. Therefore, he immediately adds that "truth is one of the key values to which authority in many contexts is linked." Yet Friedrich illustrates this relation of authority to truth only with an allusion to scholarship and states that authority is limited in its connection with truth by the limitations of human reason.[21] Friedrich seems to develop two different ideas of justice without explaining adequately their relationship. On the one hand, he relates justice to transcendent values; on the other, he limits it to the value judgments of a particular community. Since authority is tied to justice, the problem affects his analysis of authority.

The crux of the difficulty with Friedrich's definition is that it relates authority solely to the fluid, changing values and interests of the political community.[22] This definition means, as Friedrich admits, that Hitler and Stalin exercised true authority.[23] It does little good to say that they exercised it only in relation to their followers, for this is true of all who exercise authority. It is as true of the Pope as of the U.S. Congress. For a communication – a law for example – to be authoritative, it must be susceptible to reasoned elaboration not only in terms of the (relative) value judgments of the community but also in terms of the truth. This assertion need not (and does not) imply the possession of an "absolute truth" by one man or group of men, but only an openness to reality and truth on the part of the one in authority. By "truth" I mean an insight into or a discovery about reality, which is capable of being understood by some and communicated to others through symbols. The "insight" or "discovery" includes religious, scientific, human, aesthetic, and political insight or discovery. By "absolute truth" I mean a claim about reality which purports to embody it, or some aspect of it, fully and completely in a doctrine, dogma, proposition, or law. It is the ideologist's imitation of the authority's knowledge. While Friedrich seems to recognize a relationship between authority and truth, he still clings to a concept of authority rooted in the relative value judgments of the political community.

Friedrich does not appear to take his own examples of authority to heart. He uses as examples the authority of the Roman Senate, the doctor, and the parent; yet in all of these, and in that of the teacher, we see that authority does not rest wholly upon ties with the value judgments of the populus, the patient, the child, or the student. Indeed, in all these cases it rests primarily, as Friedrich admits about the doctor, on greater knowledge and better reasons.[24] Clearly the authority of the parent is primarily based upon a knowledge of certain truths which the child does not know and which the parent wishes to communicate to the child. The situation is analogous regarding the teacher, the doctor, and the Senate. Political authority does rest partly upon shared values. Such is the thesis of this paper. Nevertheless, this illustration points out the important fact which Friedrich misses; namely, that authority, including political authority, needs to be oriented to truth in order to be genuine.

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[19] Mark 1:22.

[20] *Man and His Government*, pp. 228–229.

[21] *Ibid.*, p. 230; see also pp. 260–261; *Philosophy of Law*, pp. 188–191, and *Transcendent Justice* (Durham, N.C.: Duke University Press, 1964).

[22] For Friedrich's direct discussion of values see *Man and His Government*, pp. 62–69 and *Tradition and Authority*, pp. 62–65.

[23] Cf. *Man and His Government*, p. 227 and *Tradition and Authority*, p. 49. Parallel is Friedrich's argument that ideology may legitimize rule, thus making it possible for Hitler and Stalin (and many other tyrants) to exercise legitimate rule as well as authority. See *Tradition and Authority*, pp. 94–97.

[24] *Introduction to Political Theory*, p. 129. For a more general criticism of Friedrich's theory of politics, see Richard W. Crosby, "Carl Friedrich's Empirical Theory of Politics," *The Political Science Reviewer*, 3 (Fall, 1973), 183–200.

Yves Simon: Authority and the Common Good

Yves Simon's theory of authority achieves this openness to truth in his consideration of the common good. His development of the theory of authority is undoubtedly Simon's most significant contribution to political philosophy.[25] Simon initially considers authority in common sense terms as a rule of conduct. "Authority is an active power, residing in a person and exercised through a command, that is, through a practical judgment to be taken as a rule of conduct by the free will of another person."[26] One person's defining rules of conduct for another is government; the problem of government is therefore the problem of authority.

Simon indicates that authority today has a bad name; it is distrusted and contrasted with truth, justice, and life, but most of all with liberty. It is associated with unequal exchange and with arbitrary establishment of truth. Authority and authoritarianism are seldom distinguished. Liberal and democratic thinkers have tended to conceive of social progress in terms of liberty and liberty in terms of self-determination or self-government; yet authority implies government by external forces. Therefore, the growth of liberty should imply the decay of authority.[27] The second problem grows out of what Simon calls the "deficiency theory of government."[28] An older liberal theory of government, retained to a certain extent by modern theorists, held that only deficiencies in men make government necessary. As education and better institutions remove deficiencies, government will become less necessary. Yet liberals today tend to believe that much government activity is necessary for the preservation and normal growth of all the benefits which society stands for. Progress seems to call for more government supervision, not less, in their maintenance, use, and distribution.

Simon admits that the deficiency theory is plainly applicable to parental authority, which is necessarily substitutional and temporary. Parental authority is exercised only because of some deficiency (of knowledge, virtue, or experience, for example) in the child. It aims at making up this deficiency, and, therefore, at its own disappearance and the creation of autonomy in the child. Simon goes on to assert that this self-liquidating quality of paternal authority (e.g., colonial government) is a principle of justice in all kinds of political systems.[29] Simon next asks whether any case can be discovered in which authority is not substitutional but essential. He postulates that, "if any functions of authority originate in nature and plenitude rather than in deficiency, it can be reasonably conjectured that they are relative to common existence and common action."[30] Authority will be essential if unanimity of opinion about the end of and means to the common good cannot be attained even in a community of intelligent, informed, and virtuous men. Simon's theory of authority is thus founded upon the concepts of community and common good and the obvious necessity of unity of action (though not of opinion) in a multitude of citizens. If Simon can demonstrate that even such a society needs authority to direct it to its ends, then he will have demonstrated an essential function of authority.

Taking first the determination of the means to the common good, Simon observes that if there is only one means, then unanimity on that means will arise naturally in a perfect society; authority will not be necessary. As Simon puts it:

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[25] Simon was concerned with the concept of authority for the last twenty years of his life. See his *Nature and Functions of Authority* (Milwaukee: Marquette University Press, 1940) and *A General Theory of Authority*, with an Introduction by A. Robert Caponigri (Notre Dame: University of Notre Dame Press, 1962). See also *The Philosophy of Democratic Government* (Chicago: University of Chicago Press, 1951, 1961), chap. 1. (All quotations are from the 1961 paperback edition.)

[26] *Nature and Functions of Authority*, p. 7. The "person" may be the whole community acting as a public person or particular individuals in official positions.

[27] *Ibid.*, pp. 5–6; one of the best statements of this position is Robert Paul Wolff, *In Defense of Anarchism* (New York: Harper & Row, 1970).

[28] *Philosophy of Democratic Government*, pp. 4–6 and chap. 1, *passim; General Theory of Authority*, pp. 21–22; *Freedom and Community*, ed. Charles P. O'Donnell (New York: Fordham University Press, 1968), pp. 49, 51.

[29] *Ibid.*, pp. 7–19.

[30] "Common Good and Common Action," *Review of Politics*, 22 (April, 1960), 203.

> When the means to the common good is uniquely determined, affective community supplies an essential foundation for unanimous assent; unanimity is, then, the only normal situation, and, if everything is normal, authority is not needed to bring about unified action. Unity of action requires authority in so far as not everything is normal, in so far as wills are weak or perverse and intellects ignorant or blinded. The function of authority remains substitutional.
>
> But when, on the other hand, there is more than one means to procuring the common good, there is no foundation whatsoever for unanimity.[31]

Unanimity, Simon observes, may be all-important. It is, for example, literally a matter of life and death which side of the street we drive on; the common good requires that all drive on the same side. Yet intelligent, virtuous, and informed men may disagree on which side it should be; there are two possibilities, so the means to the common good is not uniquely determined. Authority is needed to make the determination. As deficiencies, in societies or in individuals, decrease, the number of available choices increases.[32] Simon thus concludes: "The function of authority with which we are concerned, i.e., that of procuring united action when the means to the common good are several, does not disappear but grows, as deficiencies are made up; it originates not in the defects of men and societies but in the nature of society. It is an essential function."[33] The deficiency theory does not hold in this case; authority has an essential role.

The examination of authority does not end with the discovery that it has an essential function in determining the means to the common good, for the question of the determination of the goal of social action remains. The answer at first seems obvious: if society is composed of stupid and vicious people, authority is clearly necessary. But if it is composed exclusively of good and enlightened people, it seems that they will spontaneously intend the common good. Yet the question is profoundly difficult. In his attempt to answer it, Simon uses the Scholastic distinction between matter and form, distinguishing the common good formally considered from the common good materially considered. The question then becomes how the members of society are related to the matter and form of the common good. "So far as community life is concerned, the problem of matter and form within the end can be posed as follows: Is it desirable that the common good be willed and intended, both with regard to matter and with regard to form, by private persons acting in a private capacity?"[34] Simon's answer is not simple, for he is not satisfied with a sharp distinction between private and common goods; the citizen must be devoted to the common good at all times. Yet it also "matters greatly for the common good itself" that particular persons properly defend particular goods. Simon argues that the distinction between form and matter resolves this difficulty. The private person must always will the common good formally understood. That is, he must place the common good and whatever specific policy it might demand before his private advantage. Yet as far as content or matter goes, he is to will and intend private goods. The obligation of the public official is different:

> But the public person is defined by the duty of willing and intending the common good considered both in its form and in its matter. And because the service of the common good normally involves an arrangement of things private, and sometimes requires the sacrifice of private interests, the subject of the public capacity exercises authority over the private person, whose business it is to look after particular matters.[35]

Since the private person is concerned with only one particular aspect of the common good, there must be above him a person or group of persons properly concerned, formally and materially, with the whole of the common good. This concern develops into a direction of society by public reason and will toward the common good. Thus, "the most essential function of authority is the issuance and carrying out of rules expressing the requirements of the common good considered materially."[36] In other words, authority is essential in the determination of social goals and priorities.

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[31] *Philosophy of Democratic Government*, pp. 29–30. This is so because prudential judgments do not enjoy necessary intersubjectivability (see pp. 27–29).

[32] *Ibid.*, pp. 31–33. Simon has considered the question of deficiency, perfection, and choice at greater length in *Freedom and Community*, esp. chaps. 1–2 and in *Freedom of Choice*, ed. Peter Wolff (New York: Fordham University Press, 1969), chap. 3.

[33] *Ibid.*, p. 33; see also "Common Good and Common Action," pp. 210–223 for a slightly different statement of the argument just presented.

[34] "Common Good and Common Action," p. 225; see also *Philosophy of Democratic Government*, p. 39.

[35] "Common Good and Common Action," p. 227; also *Philosophy of Democratic Government*, pp. 41–42.

[36] "Common Good and Common Action," p. 228; also *Philosophy of Democratic Government*, pp.

In summary, then, three functions of authority may be distinguished: the paternal or substitutional, the essential (unifying common action when the means to the common good is not uniquely determined), and the most essential (procuring the intention of the common good, materially considered). The latter two would be necessary even in a "society composed exclusively of clever, virtuous, and fully mature persons." Authority is, for Simon, authority because it is linked to the common good of a community. For Simon the binding power of authority is derived from its moral cogency, its relation to the common good. It elicits a distinctly ethical motive for obedience. For Simon, then, the concepts of community and common good are central to the concept of authority.

Community and common good are intimately related – community being defined as a society relative to a common good.[37] Society does not exist to serve individual needs alone, for the daily life of the community transcends the limitations in duration and diversity of individual plenitude:

> Human communities are the highest attainments of nature, for they are virtually unlimited with regard to diversity of perfections, and virtually immortal. Beyond the satisfaction of individual needs the association of men serves a good unique in plenitude and duration, the common good of the human community.[38]

Man is sometimes a part of a community, sometimes not. When he is, the common good is greater than private good, and the latter may have to be sacrificed, as long as the common good and the private good are of the same order. The common good is not ultimate absolutely speaking, but only in an order which itself is not ultimate. It does not take priority, for example, over the individual's relationship with God.[39]

What is the nature of the common good? Simon answers that there are two kinds of societies – the community and the partnership – and, correspondingly, two kinds of goods – the common good and the sum of particular goods. The common good of a community calls forth "a common life of desire and action." The good of a partnership does not.[40] The good of a community consists in a common life of desire and action, a "unity in knowing and loving or hating," for ". . . the most important part of community life takes place in the heart of man."[41] Common action is central to community and, therefore, authority is essential, since common action cannot be taken for granted, but must be caused. The causation of common action is the function of authority. Authority thus depends upon and creates community and common good. All three are mutually creating and reinforcing.

Simon's theory of authority, then, gives strong support to my thesis that authority is possible only in community. Politics for Simon attempts to secure or to pursue what is beneficial for men in community – the common good. Authority, then, is not an external force commanding an individual against his will. Rather, because it proceeds from communal life, which is a shared internal life, it speaks to the life and commitment to common action which the individual member of the community shares with the other members. It is thus compatible with liberty.[42] Authority also makes possible the continued existence and growth in excellence of the community by forming and perfecting new members and by assisting old members. Or, as Simon says, "here, over and above whatever is done by example, love, and friendship, the communica-

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47–48. For other functions of authority not directly relevant to political society, see *Philosophy of Democratic Government*, pp. 59–62, n. 23 and *General Theory of Authority*, chaps. 3–5.

[37] *Philosophy of Democratic Government*, p. 64; also "Common Good and Common Action," p. 210. Simon systematically considered community and common good in three places: *Philosophy of Democratic Government*, pp. 48–50, 62–66; *Tradition of Natural Law*, pp. 86–109; "Common Good and Common Action," pp. 202–244. See *General Theory of Authority*, chap. 2 and *Freedom and Community*, pp. 103–108, 130–144.

[38] "Common Good and Common Action," p. 207; also *Tradition of Natural Law*, pp. 88–89.

[39] *Tradition of Natural Law*, pp. 91, 105–107. This position is identical to Maritain's. For an elaboration of it, see Jacques Maritain, *The Person and The Common Good*, trans. John J. Fitzgerald (New York: Charles Scribner's Sons, 1947).

[40] *Philosophy of Democratic Government*, p. 49; also "Common Good and Common Action," pp. 208–210 and *Freedom and Community*, pp. 103–109.

[41] *Tradition of Natural Law*, pp. 95–96; also *General Theory of Authority*, pp. 125–126.

[42] Cf. *Freedom and Community*, pp. 103–109, esp. 107–108. The relationship between liberty and authority is beyond the scope of this paper. For a comprehensive treatment, see Vukan Kuic, "The Contribution of Yves R. Simon to Political Science," *The Political Science Reviewer*, 4 (Fall, 1974), 55–104.

tion of excellence follows a way proper to authority, for the greater excellence of the able leader consists in his adequate relation to the common good, and it is precisely this relation which is communicated in the act of taking his orders."[43]

This reference once again reminds us that although Simon, like Friedrich, grounds authority in community, he goes beyond Friedrich to refer it to a common good measured by standards which transcend the immediate values or beliefs of any particular community. In Simon's conception, an authority is a witness to the community for the common good, which is the good it must pursue. The common good will always build on what the community is, what it values, but it will always call the community to become something it has not yet become.

This notion of authority as witness to the truth is especially clear in Simon's discussion of the place of authority in the theoretical judgment; that is, in the realm of theoretical knowledge. "When the issue is one of truth, not of action, the person in authority has the character of a witness."[44] So understood, Simon argues, authority has a large role to play in the cognition of theoretical truth and in the social teaching of the truth. Yet, Simon goes on to say, the function played by authority in the realm of theoretical truth is substitutional, not essential. It "plays a part which under better circumstances would pertain to the object."[45] Simon seems to be saying that when directly confronted with truth, the mind wavers from lack of confidence. Authority supplies that confidence in the absence of an unattainable objectivity. The role of authority in the province of scientific and theoretical knowledge possesses profound difficulty and importance, but is seldom treated. Simon here touches it, but he does not explore it in depth. Does authority have an essential or merely a substitutional role to play in the discovery (as opposed to the communication) of truth? Moreover, in the realm of theoretical knowledge does authority continue to be grounded in community? While Simon addresses such questions, he does not make them the center of his attention.

Michael Polanyi: Authority and Personal Knowledge

Michael Polanyi framed an epistemology and a philosophy of science directed against methodological limitations on science and against the limitations on knowledge imposed by the modern paradigm of Cartesian skepticism.[46] This theory of knowledge can only be summarized briefly and its bearing on authority sketched. According to Polanyi, the old epistemological dichotomies dependent upon the notion of critical doubt – absolute vs. relative, subjective vs. objective – do not characterize knowledge. Moreover, the methods of induction and deduction do not adequately describe the process of scientific discovery. Polanyi replaces these with the new dichotomies of subsidiary vs. focal awareness and of tacit vs. explicit knowledge.[47] He maintains that man does not know by means of induction or deduction but by the integration of his subsidiary awareness of particulars with his focal awareness of the whole through active perception. Man knows by integrating fragments into a comprehensive whole. The significance of the particulars cannot be explicitly known or stated. All knowledge thus has a tacit dimension. The norm of objective knowledge is then an impossible one, for it attempts to eliminate the tacit or unspecifiable part of knowledge. It attempts to make all knowledge explicit and thereby readily communicable to every mind, so long as each of those minds is abstracted from all "subjective" hindrances.[48] It calls for detachment. This for Polanyi is impossible, because the tacit dimension is essential in the discovery, validation, and communication of knowledge. The knower cannot be detached; rather he must, in Polanyi's term, "indwell" the particulars. He takes meaning from the subsidiary terms by entering into them and making them a part of himself. All knowledge then, Polanyi argues, is personal knowledge. It arises from the commitment of the knower. As Polanyi puts it:

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[43] *General Theory of Authority,* p. 145. All of chap. 4 of this work is an explanation of this point.

[44] *General Theory of Authority,* p. 84.

[45] *Ibid.,* p. 92.

[46] The results of this inquiry are expressed primarily in *Personal Knowledge: Towards a Post–Critical Philosophy* (New York: Harper & Row, 1964). Other major statements are: *Science, Faith, and Society* (Chicago: University of Chicago Press, 1964), *The Study of Man* (Chicago: University of Chicago Press, 1963), and *The Tacit Dimension* (Garden City, N.Y.: Doubleday, 1966). My analysis in the following pages owes much to discussions over a number of years with James L. Wiser. See his "Michael Polanyi: Personal Knowledge and the Promise of Autonomy," *Political Theory,* 2 (February, 1974), 77–78 and "Political Theory, Personal Knowledge, and Public Truth," *Journal of Politics,* 36 (August, 1974), 661–674.

[47] This exposition follows *Personal Knowledge,* pp. 55–131, 255–298; *Tacit Dimension,* pp. 3–25, and *Study of Man,* pp. 11–70.

[48] For elaboration, see Wiser, "Political Theory," pp. 668–671, and "Michael Polanyi," pp. 79–80.

> According to the logic of commitment, *truth is something that can be thought of only by believing it.* It is then improper to speak of another person's mental operation as leading to a true proposition in any other sense than that it leads him to something the speaker himself believes to be true.[49]

If this is the case, is Polanyi saying that all knowledge is subjective? Most emphatically not.[50] The knower indwells in order to appreciate reality, not to appreciate a structure created by the self. The ultimate honesty of his personal commitment ties the knower to reality. This honesty is derived from intellectual passions which lead the knower to search for reality and to identify it by its preciousness. The knower knows and communicates his knowledge with universal intent. As James L. Wiser puts it:

> Although emerging from within a personal or existential achievement, truth was to be understood to be discovered by the rational or noetic element in man. As such, it addressed itself to that very faculty which . . . was common to all men. Given this understanding, the achievement of truth was a representative act, and its appearance carried with it the implications of a universal validity.[51]

While written specifically about Plato and Aristotle, these words apply also to Polanyi.

If, then, all knowledge is personal knowledge, what is the place of authority, community, and tradition? Since knowledge as the result of tacit operations passionately supported cannot be formulated in totally explicit definitions, rules, maxims, or procedures; it must be transmitted tacitly, that is, as part of a tradition and a structure of teacher and student. ". . . [T]he transmission of knowledge from one generation to the other must be predominantly tacit."[52] Knowledge is like a skill or an art; it must be passed from master to apprentice. The tradition in which the master and apprentice (or teacher and pupil) operate also serves as a guide to what others have found to be rich in the representation of reality. Tradition is, then, not valued for itself, but because it is transparent, because it allows reality to shine through.[53] But tradition depends upon authority and community (or "convivial order" as Polanyi calls it). ". . . [O]ur adherence to the truth can be seen to imply our adherence to a society which respects the truth, and which we trust to respect it."[54] The learner must "affiliate" himself to a community that cultivates the tradition of knowledge to which he aspires, that appreciates its values and strives to act by its standards.[55] Such a convivial order makes fellowship, communication, and participation in joint activities possible. This, of course, recalls the idea of community in Yves Simon. Even more striking in this respect is Polanyi's adding to the idea of convivial order the idea of authority in the achievement of the common good.[56] In the case of scientific communities, of course, the common good is scientific knowledge. Authority, then, is necessary to keep the community together and to keep it directed toward its end. Authority is necessary to suppress deviation through mutual control, but even more basic is its function in pointing to new possibilities for discovery within the tradition. Authority has a dynamic character. The acceptance of authority is the recognition that what appears meaningless has a meaning which can be discovered by the same kind of indwelling practiced by those who have gone before. It is a joint celebration of the structure of the convivial order.[57] Thus, "the effect of Polanyi's argument on behalf of traditional knowledge is to suggest that *belonging* to a community, *believing* in its traditions, and thereby *understanding* the experience which engendered them are all part of a single process, i.e., the acquisition of knowledge."[58] Thus, unlike Simon, Polanyi sees an essential connection between authority, community, and the acquisition of theoretical knowledge.

Authority for Polanyi has two aspects: the horizontal and the vertical. The former can be illustrated by the following reference to authority in science:

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[49] *Personal Knowledge,* p. 305. Emphasis in original.

[50] Polanyi defends himself against this charge in *Personal Knowledge,* pp. 299–324; see also pp. 104, 132–150.

[51] "Political Theory," pp. 663–664.

[52] Polanyi, *Tacit Dimension,* p. 61.

[53] Polanyi, *Personal Knowledge,* pp. 53–55, 374–380; see also Wiser, "Michael Polanyi," pp. 82–84.

[54] Polanyi, *Personal Knowledge,* p. 203.

[55] *Ibid.,* p. 207.

[56] *Ibid.,* pp. 210–212. Cf. *Tacit Dimension,* pp. 53–92.

[57] Cf. *Tacit Dimension,* pp. 61, 72–74; *Personal Knowledge,* pp. 53, 163–164, 207–209; *Science, Faith, and Society,* chap. 2.

[58] Wiser, "Michael Polanyi," p. 84. Emphasis in original.

> The authority of science resides in scientific opinion. Science exists as a body of wide-ranging authoritative knowledge only so long as the consensus of scientists continues. It lives and grows only so long as this consensus can resolve the perpetual tension between discipline and originality. Every succeeding generation is sovereign in reinterpreting the tradition of science.[59]

The body of recognized scientists establishes the tradition and authority of science by forming a scientific consensus which is a "chain of mutual appreciation."[60] Each scientist is responsible for maintaining watch over the traditions of science in his own area; yet, since areas overlap, a network of authority is formed. In many ways this is what Friedrich says about authority. A scientific communication is authoritative because it is capable of elaboration in terms of the consensual values of the scientific community, which are embodied in a dynamic tradition. Yet there is a vertical dimension also, an orientation toward the truth, which we saw in Simon. A scientific communication is authoritative because it is in principle universal, because it makes contact with reality. It is genuinely knowledge and thus makes a claim on each person, not just those within the tradition. A scientific claim is made with universal intent; yet, since the contact with reality is personal, it is in fact meaningful only to a community of similar persons. Like Socrates' claim to authority in Athens, which is based on his insight into reality but understood only by his followers, any knower's claim to authority has these two inseparable aspects.[61] When we speak of society, the universal (or vertical) aspect of authority must be the common good. To the extent that society is a convivial order (community) committed to seeking the common good, authority is present as the force directing it toward that common good. Thus, Polanyi's epistemology is congruent with Simon's political philosophy, but demonstrates more profoundly the dependence of all authority upon truth, community, and tradition.

Conclusions

Our odyssey through the theories of authority in the thought of Carl Friedrich, Yves Simon, and Michael Polanyi has brought us, I hope, to a hospitable shore. The presentation of these three theories was intended to be cumulative, so that our conclusions about authority may be quickly drawn. First of all, the connection between authority and common good in Simon and between authority and reality in Polanyi helps us to dispel the strangeness which at first glance surrounds the claims to authority by Socrates and Christ.[62] Such a claim is not comprehensible in terms of power or of widespread following, but only in terms of a personal contact with transcendent reality. The "vertical" aspect of authority overwhelms the "horizontal" in these two cases. These extreme cases confirm the insight of Polanyi and Simon, *pace* Friedrich, that all authority is rightful authority, that a higher source than common values must validate any claim to authority.[63] An authority is a witness to the truth.

But Christ's speech and action draw on the traditions of the Law and the Prophets, and Socrates' speech draws on the traditions of Athens and of philosophy. Moreover, both gather a community around them. Thus, their authority is personal and is meaningful because it appeals to a community of seekers of truth and draws on the traditions of such a search. It follows that a "horizontal" aspect is always present. This is the second conclusion to be drawn. Authority is possible only on the basis of community and tradition. Outside such a community, the communications of the one in authority are unintelligible. The personal quality of knowledge and authority and the necessities of communication make essential community as a system of shared values, trusts, and traditions of action and speech. The authority as witness must be part of a community which makes possible his reaching beyond the horizon of the community. In any case of authority, either the "vertical" orientation to truth and reality or the "horizontal" relation to the wisdom of the community may predominate. Perhaps in the majority of cases it will be the latter. Yet both must be present in every authority and in every authoritative pronouncement.

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[59] *Science, Faith, and Society*, p. 16.

[60] *Ibid.*, p. 16.

[61] Socrates' claim is evident in the *Apology*, *Gorgias*, and *Republic;* Cf. Eric Voegelin, *Order and History, Vol. III: Plato and Aristotle* (Baton Rouge: Louisiana State University Press, 1957), esp. pp. 7–10, 36–45, 48–70. See also Wiser, "Political Theory," pp. 664–668, 674 for the two aspects of authority.

[62] For Socrates, see previous note; for Christ, Luke 20:2–8 and esp. Mark 1:21–28.

[63] Cf. Charles W. Hendel, "An Exploration of the Nature of Authority," in Friedrich (ed.), *Nomos I*, pp. 3–27, esp. p. 14.

Third, the creation by Socrates and Christ of communities extending to the present teaches us that the existence of community requires authority. Such is also the teaching of Friedrich, Simon, and Polanyi. My thesis in this paper has been that authority requires community, but this emphasis should not be allowed to obscure that equally community must have authority. Men may experience occasional moments of spontaneous sharing, love, and trust. While such an experience may linger long in the heart, the unity it creates will not last unless it is maintained by some form of authority, personal or institutional.

We may then think of authority as the reflection, within a community of men pursuing a common goal or goals set by their shared tradition, of the tension between the given and the demanded. Authority as insight into the demanded directs the community from where it is (the given) to where it ought to be (the demanded). Its directions are given in the language and symbols of the tradition, while at the same time it subtly reforms that language and those symbols in conformity with the demands of truth. Thus, the theory of community and authority does portray a definite way of life for man, a life in pursuit of truth and common good in association with other such seekers.[64]

I have not spoken only of political authority, though much of the foregoing is drawn from discussions of political authority. This is because I believe that all authority must be grounded in transcendent standards and in community. Yet authority is shaped and manifested in different ways in different contexts. Political authority is related to the common good and to the common political ideas, experiences, and goals of a community, while scientific authority is related to reality and the shared assumptions and methods of the scientific community. Parental authority is oriented to standards of maturity and responsibility and to the beliefs, commitments, and experiences of the family. Authority, in its various contexts, may be exercised by one person, a few persons, many persons, or even all persons in the community. The nature of authority must not be confused with the manner of its exercise.[65] The nature of authority is to direct an activity to its proper end. The right to direct is not necessarily lodged in one, few, or many. The type of community, activity, and end will determine the manner of exercise of authority. A full discussion of political authority would require a fuller consideration of the nature of politics, common good, and the political community.

What can we say, finally, from this perspective about the crisis of authority in the modern world? All three grounds of authority – personal knowledge, community, and tradition – are weakened and under attack in our time. The Cartesian paradigm of systematic doubt predominant in epistemology cuts the link between authority and truth.[66] Its effects can be seen even in Friedrich's theory of authority. The impact of individualism and the ideal of personal autonomy have weakened belief in and ties to community. Communities, of course, exist and will continue to exist. Yet individualism as an ideal and a practice makes difficult the existence of communities based on rational common commitment, trust, memory, faith, collective ideals, and discipline.[67] Finally, tradition and the ceremony, ritual, and emotion connected with it have acquired a bad name in the contemporary world.[68] They are associated (often rightly) with traditionalism, reactionary politics, and obscurantism. The attack on tradition as such, however, is fundamentally an attack on history. It is at bottom an attempt to escape history and the necessarily historical (and therefore limited) existence of man. Tradition is what makes historical existence bearable by giving some meaning and perspective to the distance between the given and the demanded. Political theorists can do little, perhaps, directly to respond to the practical crisis of authority. Yet ideas have consequences, and the practical crisis of authority has roots in the undernourished soil of our theoretical understanding of authority, tradition, knowledge, and community. This means that the task of the political theorist is twofold: to reformulate and improve our understanding of such concepts and to speak to the political dislocations of our time in terms of them.[69] Only then will our writing and teaching be authoritative.

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[64] *Pace* the accusation of Salkever, "Virtue, Obligation, and Politics," p. 90.

[65] On this distinction see especially Simon, *Philosophy of Democratic Government*, pp. 37–38.

[66] Cf. Wiser, "Political Theory" and "Michael Polanyi"; also Schaar, "Legitimacy in the Modern State," pp. 294–295.

[67] This may be the place to point out that the theory of authority I have elucidated is not hostile to freedom. Friedrich, Simon, and Polanyi all emphasize the interdependence of freedom and authority. Since the exploration of this interdependence would require a larger compass, I have chosen not to consider it in this paper.

[68] For connections between ceremony, ritual, tradition, and community see Harvey Cox, *The Feast of Fools* (Cambridge: Harvard University Press, 1969), *passim;* and Maurice R. Stein, *The Eclipse of Community* (New York: Harper & Row, 1964), chap. 10.

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[69] An exciting recent attempt to work with the idea of community is Glenn Tinder, "Transcending Tragedy: The Idea of Civility," *American Political Science Review,* 68 (June, 1974), 545–560; Tinder, "In Defense of Pure Tolerance," *Polity,* 6 (Summer, 1974), 446–468; and Tinder, *Tolerance: Toward a New Civility* (Amherst: University of Massachusetts Press, 1976).

The Relative Importance of Socioeconomic and Political Variables for Public Policy*

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Since the quantitative study of public policy as a dependent variable first began, a dominant issue has been the relative importance of socioeconomic and political variables for determining policy outcomes. Dawson and Robinson, in their pivotal article, concluded that socioeconomic conditions were more important than the political variable of interparty competition in shaping welfare policies.[1] Their findings, along with the earlier theoretical contributions of Key and Lockard,[2] sparked a series of investigations on public policy determinants, each of which attempted to make comparative judgments about the impact of socioeconomic and political variables.[3]

More recent work indicates that the question continues to be salient. Fry and Winters state that a major aim of their study is "to examine the relative importance of political and socioeconomic variables"[4] for explaining the redistributive policies of the American states. They decide that political variables are more important than socioeconomic ones.[5] Booms and Halldorson, revising Fry and Winters, also address the topic, concluding that their reformulation "raised considerably the relative explanatory power of the socioeconomic variables."[6] While doubting that any definitive answer to this question is possible, Uslaner and Weber nevertheless assert, in their investigation of the politics of redistribution, that "there is a great deal to be said"[7] for explanations which emphasize political over socioeconomic factors. Finally, a current piece by Tompkins on state welfare expenditures contends that the central issue in the literature on policy outcomes is still the relative importance of socioeconomic and political variables.[8]

In these various studies, different statistical techniques have been employed to assess the relative importance of the independent variables: simple bivariate correlation (rarely in isolation), partial correlation, and multiple regression. Regardless of the particular analytic technique stressed, the strategy has generally been to compare the magnitudes of the coefficients of the socioeconomic and political variables in their relation with the policy variable, on this basis making a judgment about which are more important.

The aim of this brief paper is to demonstrate that research efforts to date have failed to assess accurately the relative importance of socioeconomic and political variables for public policy, in large part because they have relied on statistical techniques inadequate to the task. As Duncan, and Linn and Werts, have shown, zero-order correlation, partial correlation, and multiple regression would provide unbiased estimates of importance only for very peculiar causal structures.[9] When the researcher's implicit or explicit theory does not correspond to this very restricted set of models, as will generally be the case, then an assessment of relative importance based on parameter estimates in a formally constructed causal model must be undertaken.

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*I would like especially to thank Donald J. McCrone and Lawrence B. Mohr for their contributions to the development of this paper.

[1] Richard E. Dawson and James A. Robinson, "Inter-party Competition, Economic Variables, and Welfare Policies in the American States," *Journal of Politics,* 25 (May, 1963), 265–289.

[2] V. O. Key, Jr., *Southern Politics* (New York: Vintage Books, 1949), p. 307; Duane Lockard, *New England State Politics* (Princeton, New Jersey: Princeton University Press, 1959), pp. 320–340.

[3] Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review,* 63 (September, 1969), 858–866; Thomas R. Dye, *Politics, Economics, and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally, 1966); Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," *American Political Science Review,* 60 (March, 1966), 73–82; Ira Sharkansky and Richard I. Hofferbert, "Dimensions of State Politics, Economics, and Public Policy," *American Political Science Review,* 63 (September, 1969), 867–879.

[4] Brian R. Fry and Richard F. Winters, "The Politics of Redistribution," *American Political Science Review,* 64 (June, 1970), 508–522.

[5] Fry and Winters, p. 521.

[6] Bernard H. Booms and James R. Halldorson, "The Politics of Redistribution: A Reformulation," *American Political Science Review,* 67 (September, 1973), 924–933.

[7] Eric M. Uslaner and Ronald E. Weber, "The 'Politics' of Redistribution: Toward a Model of the Policy-making Process in the American States," *American Politics Quarterly,* 3 (April, 1975), 130–170.

[8] Gary L. Tompkins, "A Causal Model of State Welfare Expenditures," *Journal of Politics,* 37 (August, 1975), 392–416.

It is suggested here that comparison of "effects coefficients," derived from path analysis, is the preferred method of assessing the relative importance of different independent variables for explaining a given dependent variable.[10] To support this contention, the limitations of simple correlation, partial correlation, and multiple regression coefficients are first discussed. Then, the advantages of the effects coefficient are presented. Finally, the effects coefficients for a current model of welfare policy are calculated, and the results compared to estimates from the aforementioned correlation and regression approaches. In evaluating these effects coefficients, which are actually congruent with the policy model posited, socioeconomic variables emerge as clearly more important than political variables, contrary to interpretations based on the more traditional statistical techniques.

Comparing Correlation Coefficients to Evaluate Relative Importance

The shortcomings of comparing simple correlation coefficients in order to assess the relative importance of socioeconomic and political variables for public policy are perhaps obvious. Indeed, when simple correlations are reported in the literature on the subject, they almost never serve as the sole basis of evaluation. Dawson and Robinson first look at bivariate correlations, finding substantial relationships between interparty competition and socioeconomic conditions, between interparty competition and welfare policy, and between socioeconomic conditions and welfare policy.[11] But they then go on to examine these bivariate relationships while controlling for the third variable. (This was done by dividing the control variable into three categories, i.e., upper, middle, lower, and looking at the adjusted correlations within each category.) They conclude that socioeconomic conditions are more important than interparty competition for welfare policy in the American states. The primary analytic technique of Hofferbert, however, is bivariate correlation.[12] He finds that the degree of malapportionment of state legislative districts and the extent of divided control of state governments, respectively, have inconsequential relationships with "welfare orientation," whereas socioeconomic conditions do not (r=.70). Thus, he contends that socioeconomic factors appear to have more impact on state policies than political structure variables.

It is useful to elaborate a specific example in order to make the limitations of correlational comparison explicit. Suppose, like Dawson and Robinson, one is interested in the relative effects of socioeconomic conditions (X_1) and interparty competition (Y_2) on welfare policy (Y_3). Then the zero-order correlations, r_{13} and r_{23}, are compared and, because r_{13} is of greater magnitude, it is decided that socioeconomic conditions are more important for welfare policy than interparty competition. Assuming there are no other problems (e.g., measurement error) this judgment will be accurate only if the actual interrelationship among interparty competition, socioeconomic conditions, and welfare policy corresponds to the causal model diagrammed in Figure 1. That is, interparty competition and socioeconomic conditions must be *completely* independent. This certainly does not seem likely, especially given the substantial correlations Dawson and Robinson report between the two variables.[13] Further, it has been argued repeatedly that socioeconomic factors influence political variables, in which case an evaluation based on a comparison of r_{13} and r_{23} would tend to underrate the total impact of socioeconomic forces on policy.[14] A further contention is that the apparent relationship between interparty competition and welfare outcomes is largely spurious, a product of the effect of socioeconomic variables on both.[15] If this is so, then r_{23} would of course exaggerate the effect of interparty competition.

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[9] Otis D. Duncan, "Partials, Partitions, and Paths," in *Sociological Methodology 1970*, ed. E. Borgatta and G. Bohrnstedt (San Francisco: Jossey-Bass, 1970), pp. 38–47; R. L. Linn and C. E. Wirts, "Assumptions in Making Causal Inferences from Part Correlations, Partial Correlations, and Partial Regression Coefficients," *Psychological Bulletin*, 72 (November, 1969), 307–310.

[10] Michael S. Lewis-Beck and Lawrence B. Mohr, "Evaluating Effects of Independent Variables," *Political Methodology*, 3 (February, 1976), 27–47.

[11] Dawson and Robinson.

[12] Hofferbert.

[13] Dawson and Robinson.

[14] Cnudde and McCrone, p. 860; Dye, *Politics, Economics, and the Public*, p. 285; Lockard, *New England State Politics*, pp. 336–337.

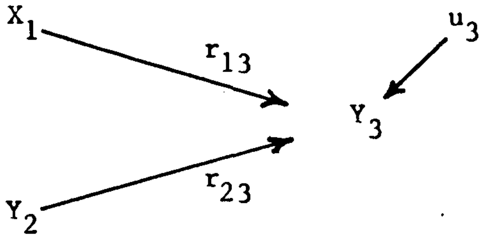

Figure 1. Causal System Referenced by Zero-Order Correlation Coefficients

While the limitations of comparing simple bivariate correlations in order to arrive at the relative importance of variables are clear, it may not be so apparent that the same criticisms apply to the comparisons sometimes made among multiple correlation coefficients. For instance, Fry and Winters in support of their hypothesis that political variables are more important for redistributive policies than socioeconomic variables, report that in separate regression analyses the political variables were found to explain 38 per cent of the variance but that socioeconomic variables explained only 17 per cent.[16] (Recall that percentage of variance explained is simply the square of the multiple correlation coefficient.) Unless the socioeconomic variables are completely unrelated to the political variables in the manner diagrammed in Figure 1, then this comparison of two squared multiple correlations still provides a distorted picture of their relative importance for redistributive policies.

Comparing Partial Correlation Coefficients to Evaluate Relative Importance

Nearly all of the authors whose work is reviewed here heavily emphasize comparison of partial correlation coefficients, with the noteworthy exception of Cnudde and McCrone, who appreciate some of the inadequacies of the technique.[17] As noted above, Dawson and Robinson compute correlations between interparty competition and welfare policy, controlling for socioeconomic conditions, and between socioeconomic conditions and welfare policy, controlling for interparty competition.[18] Looking at the resultant coefficients, they conclude that socioeconomic conditions are more important for welfare policy than interparty competition is. Thomas Dye finds that the partial correlations between political system variables and policy outcomes, controlling for economic development, are consistently much lower than the partial correlations between economic development and policy outcomes controlling for the political system.[19] Therefore, he infers that political system variables are generally considerably less important than economic development variables in shaping policy outcomes. A slightly different conclusion is offered by Sharkansky and Hofferbert, who assert, "the coefficients of partial correlation reveal that this policy factor [Welfare-Education] is associated most clearly with high scores on Competition-Turnout and Affluence, with neither being significantly more important than the other."[20]

The popularity of the partial correlation technique is unfortunate, for it is a highly inappropriate device for determining the relative importance of different independent variables. Partial correlation yields a correlation of residuals. As Linn and Werts, and Duncan, have shown, the causal structure to which this correlation corresponds is that diagrammed in Figure 2.[21] Suppose, as before, that X_1 = socioeconomic conditions, Y_2 = interparty competition, and Y_3 = welfare policy. In evaluating the impact of Y_2 on Y_3, all that the partial correlation $r_{23.1}$ may tell us is whether Y_2 is spuriously related to Y_3 (e.g., $r_{23.1}$ is zero or not statistically significant), and in this sense an unimportant influence on it. Also, if one is interested, $r_{23.1}$ of course provides information about the impact of one error term or residual on another, e.g., u_2 on u_3.

If politics are in fact unimportant for policy as Dye and others have suggested, and only appear to have an effect because socioeconomic conditions are a prior common influence, then

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[15] Dawson and Robinson; Dye, *Politics, Economics, and the Public.*

[16] Fry and Winters, p. 519.

[17] Cnudde and McCrone.

[18] Dawson and Robinson.

[19] Dye, *Politics, Economics, and the Public,* pp. 293–295.

[20] Sharkansky and Hofferbert, p. 877.

[21] Linn and Werts, p. 308; Duncan, 1970, pp. 39–40.

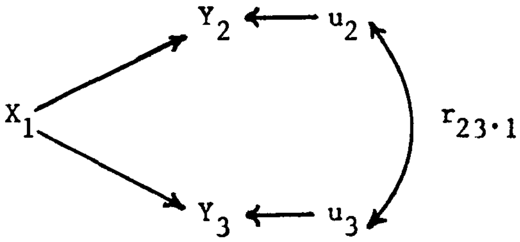

Figure 2. Three-Variable Causal System Referenced by the Partial Correlation Coefficient, $r_{23.1}$

partial correlation will indicate this spuriousness.[22] Assuming that Figure 2 reflects a true state of the world, it makes no sense to compare $r_{23.1}$ to $r_{13.2}$. This is because the causal structure congruent with $r_{13.2}$ (see Figure 3) is *different from, in fact directly contradicts,* the model of reality affirmed in Figure 2. As can be seen by comparing the two figures, it is impossible to maintain that both models are valid at the same time, e.g., Figure 2 for $r_{23.1}$ indicates that X_1 affects Y_2 but Y_2 does not affect X_1, while Figure 3 asserts exactly the opposite. Obviously the model that corresponds with reality is the only valid one. When Figure 2 is declared to be correct, then an examination of $r_{13.2}$, which implies Figure 3, is irrelevant and misleading. If, after due deliberation, it cannot be determined which model is properly specified, or neither appears to mirror the real world accurately, then the partial correlation technique can tell us nothing whatsoever about the relative effect of X_1 and Y_2 on Y_3. In this case, it will be necessary to turn to statistical techniques that accord with the true structure of relations among the variables.

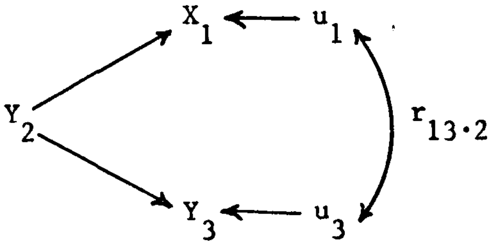

Figure 3. Three-Variable Causal System Referenced by the Partial Correlation Coefficient, $r_{13.2}$

The narrow applicability of the partial correlation approach is in no way avoided if multiple-partial correlation coefficients are compared, as Dye, and Fry and Winters, have done.[23] Fry and Winters provide an especially clear example of the use of this technique for evaluating relative importance:[24]

> The most interesting and significant finding in this study, however, concerns the relative importance of political and socioeconomic variables in determining redistributive fiscal policies in the states. . . . For the 48 states the multiple-partial for political variables controlled for the socioeconomic variables is .46 while the multiple-partial for the socioeconomic variables controlled for the political variables is only .27.

The difficulties with the simple partial correlation approach still hold in the multiple-partial case. That is, when two multiple-partial correlation coefficients are compared, as Fry and Winters do, two different and contradictory causal systems are being referenced simultaneously. However, the situation is, if anything, even more problematic than with Figures 2 and 3, for the greater number of variables involved makes the implied rival models still more complex.

Comparing Multiple Regression Coefficients to Evaluate Relative Importance

An approach to relative importance that is in some ways more satisfactory, but has been less frequently used, is comparison of multiple regression coefficients. Cnudde and McCrone employ regression analysis, although they look only at the unstandardized coefficients.[25] The difficulty with unstandardized coefficients is that, because the independent variables have different measurement scales, making judgments about relative effect is troublesome. Therefore, attention will focus on standardized partial regression coefficients, or beta weights.[26] Booms and Halldorson explicitly argue that beta weights provide a "method for comparing the relative importance of variables in 'explaining' the dependent variable."[27]

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[22] If the partial correlation is calculated and found to be zero or not significant statistically, then the conclusion of spuriousness is legitimate, granting this causal structure. However, if the partial correlation is found to be statistically significant, then it is not necessarily proper to infer, as is frequently done, that the independent variable in question, e.g., political structure, *does* have an impact on policy. This caution is understandable when it is recalled that the partial correlation is merely a correlation among residuals, e.g., $r_{u_2u_3}$. Thus, even if this correlation is significant, it may simply reflect some third variable other than socioeconomic conditions, e.g., geographic region, which is operating to produce spuriousness between political structure and public policy.

[23] Dye, *Politics, Economics, and the Public,* pp. 295–297; Fry and Winters.

[24] Fry and Winters, p. 521.

[25] Cnudde and McCrone.

In following this strategy, the most straightforward method is simply to place the variables of interest in a regression equation, calculate the standardized partial regression coefficients, and compare their magnitudes. Continuing the previous example of evaluating effects of socioeconomic conditions (X_1) and interparty competition (Y_2) on welfare policy (Y_3), the equation would look like this:

$$Y_3 = \beta_{31.2}X_1 + \beta_{32.1}Y_2 + u.$$

To determine the relative importance of X_1 and Y_2 for Y_3, the sizes of $\beta_{31.2}$ and $\beta_{32.1}$ would be compared. This approach is possible if the causal relations underlying the variables correspond to the diagram in Figure 4. In this system, both X_1 and Y_2 have a direct impact on Y_3 and they are related to each other, but the nature of this latter relationship is unspecified.

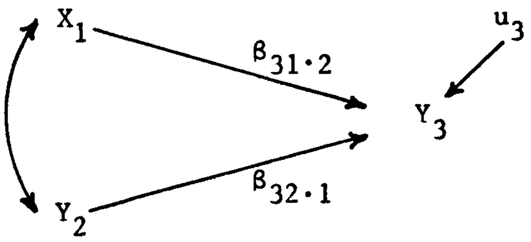

Figure 4. Three-Variable Causal System Referenced by Partial Regression Coefficients

The potential difficulty for interpretation, as Figure 4 makes clear, is that the beta weights only assess *direct* effects of independent variables. Suppose, however, that socioeconomic conditions affect the political variable of interparty competition, as has often been suggested.[28] Then, X_1, in addition to its direct effect on Y_3, has an indirect effect on Y_3 through its impact on Y_2. A comparison of $\beta_{31.2}$ and $\beta_{32.1}$ would necessarily neglect this indirect influence, thereby undervaluing the overall importance of socioeconomic conditions in shaping welfare policy. The omission of indirect effects is especially distorting when multicollinearity is large (as indeed would be expected between socioeconomic conditions and interparty competition), for then much of the influence in the system cannot be uniquely assigned to any one variable.[29]

Comparing Effects Coefficients to Evaluate Relative Importance

In the foregoing, I attempted to demonstrate that zero-order correlation, partial correlation, and multiple regression generally produce misleading judgments about the relative importance of socioeconomic and political variables for public policy. Now, I should like to illustrate how path analytic techniques can generate coefficients which provide an accurate evaluation of the effects of different independent variables. These coefficients, known as "effects coefficients," can be derived for any causal system, including the special ones covered by coefficients from simple correlation, partial correlation, or multiple regression. (Path analysis is not an untried technique in the study of public policy outcomes. Uslaner and Weber, and Tompkins, provide recent instances of its use.[30] However, an effects coefficient, which can be calculated from a path model, has *not* been used in any form to evaluate the relative impact of independent variables. Uslaner and Weber, for example, propose a six-equation recursive model of redistribution policy;[31] but they determine relative influence simply by comparing beta weights in a single equation, which amounts to no more than the multiple regression approach discussed above, with all its attendant shortcomings.)

The utility of effects coefficients is most easily shown by exploring a concrete example. Since it is beyond the scope of this essay to develop a model of public policy outcomes, the following presentation will confine itself to a plausible model already popular in the literature. This model, diagrammed in Figure 5, is most fully and explicitly treated by Cnudde and McCrone.[32] They state that welfare policy (Y_3) is caused directly by socioeconomic conditions (X_1) and interparty competition (Y_2), and that interparty competition (Y_2) is also influenced by socioeconomic conditions (X_1).

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[26] H. M. Blalock, Jr., "Causal Inferences, Closed Populations, and Measures of Association," *American Political Science Review,* 61 (March, 1967), 130–136; J. W. Tukey, "Causation, Regression, and Path Analysis," in *Statistics and Mathematics in Biology,* ed. O. Kempthorne, T. S. Bancroft, J. W. Gowen, and J. L. Lush (Ames, Iowa: Iowa State College Press, 1954), pp. 35–66; M. E. Turner and C. D. Stevens, "The Regression Analysis of Causal Paths," *Biometrics,* 15 (June, 1959), 236–258; Sewell Wright, "Path Coefficients and Path Regression: Alternative or Complementary Concepts?" *Biometrics,* 16 (June, 1960), 189–202.

[27] Booms and Halldorson, p. 932.

[28] Dye, *Politics, Economics, and the Public,* p. 285; Lockard, *New England State Politics,* pp. 336–337; Cnudde and McCrone, p. 860.

[29] Duncan, 1970, p. 40.

[30] Uslaner and Weber; Tompkins.

[31] Uslaner and Weber, pp. 138, 156.

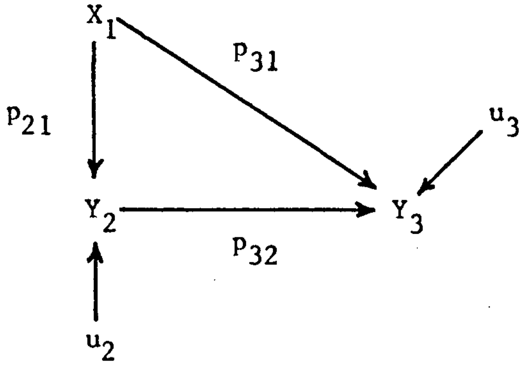

Figure 5. A Three-Variable Recursive System

As I have indicated previously, given this causal structure, the zero-order correlations r_{13} and r_{23} cannot be meaningfully compared because X_1 and Y_2 are not independent and, more specifically, because r_{23} is partly spurious. Further, partial correlations $r_{13.2}$ and $r_{23.1}$ are of no use, for they involve comparison of two different and contradictory causal systems, neither of which corresponds to Figure 5. Finally, an examination of the beta weights, $\beta_{31.2}$ and $\beta_{32.1}$ is inadequate because the indirect effect of X_1 is not taken into account. A straightforward extension of path analytic techniques, however, allows the relative impact of X_1 and Y_2 to be correctly evaluated.

Figure 5 represents a simple three-variable recursive model; for a set of structural equations to be recursive, it must meet two assumptions: (1) uncorrelated error terms; and (2) no causal feedback.[33] The model is expressed in the following system of simultaneous linear equations (the variables are assumed to be in standard form):

$$Y_2 = p_{21}X_1 + u_2$$

$$Y_3 = p_{31}X_1 + p_{32}Y_2 + u_3.$$

Because it is a recursive system, ordinary least squares (ordinary multiple regression) applied to each equation yields the most efficient estimates of the parameters p_{21}, p_{31} and p_{32}.[34] Assuming that the paths in Figure 5 are so estimated, it remains to provide a summary measure of the influence of each of the independent variables, X_1 and Y_2, on Y_3. The effects coefficient is proposed here as such a measure.

The theory and mathematics underlying the effects coefficient are developed at length elsewhere.[35] Therefore, this paper restricts itself to a brief review of its computation and interpretation. In any causal system, all effects of an independent variable are either direct (DE) or indirect (IE). The effects coefficient is simply the sum of the two. It is written E_{ki},

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[32] Cnudde and McCrone.

[33] Ronald J. Wonnacott and Thomas H. Wonnacott, *Econometrics* (New York: Wiley, 1970), pp. 193–195.

[34] Arthur S. Goldberger, "On Boudon's Method of Linear Causal Analysis," *American Sociological Review*, 35 (February, 1970), 97–101; Kenneth C. Land, "Identification, Parameter Estimation, and Hypothesis Testing in Recursive Sociological Models," in *Structural Equation Models in the Social Sciences*, ed. A. S. Goldberger and O. D. Duncan (New York: Seminar Press, 1973), pp. 19–49.

[35] See Lewis-Beck and Mohr, 1976, for a complete explication of the effects coefficient. Basically, the effects coefficient is an extension of earlier attempts to assess "total effects" in a causal system (for earlier treatments, see Duane G. Alwin and Robert M. Hauser, "The Decomposition of Effects in Path Analysis," *American Sociological Review*, 40 (February, 1975), 37–47; Otis D. Duncan, "Path Analysis: Sociological Examples," in *Causal Models in the Social Sciences*, ed. H. M. Blalock (Chicago: Aldine, 1971), pp. 137–138; John M. Finney, "Indirect Effects in Path Analysis," *Sociological Methods and Research*, 2 (November, 1972), 175–186; Kenneth C. Land, "Principles of Path Analysis," in *Sociological Methodology 1969*, ed. E. F. Bongatta (San Francisco: Jossey-Bass, 1969), pp. 16–17; Michael S. Lewis-Beck, "Determining the Importance of an Independent Variable: A Path Analytic Solution," *Social Science Research*, 3 (June, 1974), 95–107). However, the effects coefficient is more satisfactory than prior efforts for a number of reasons. First, it has generalized applicability. That is, it may be used to assess the impact of *any* independent variable in the system, endogenous as well as exogenous (on this distinction, see Wonnacott and Wonnacott, pp. 155–156). And, it is applicable to *any* linear additive causal structure, whether it be recursive or nonrecursive, just-identified or overidentified (on these differences, see Wonnacott and Wonnacott, pp. 193–195, 172–189). (Of course, for nonrecursive systems, estimation techniques such as two-stage least squares must be used, rather than ordinary least squares; see J. Johnston, *Econometric Methods* (New York: McGraw-Hill, 1972), pp. 380–384). Further, the effects coefficient is based on a more precise and comprehensive breakdown of relationships in the causal system, dividing the possible relations between two variables into direct effect (DE), indirect effect (IE), spurious relation (S) and unanalyzed relation (U).

"the effects of variable i on variable k." E_{ki} may be interpreted as referring to the change in k per unit change in i. For the simple system in Figure 5, the effects coefficients are easily found: $E_{31} = p_{31} + p_{32}p_{21}$ and $E_{32} = p_{32}$. A comparison of E_{31} and E_{32} permits a more accurate evaluation of the relative importance of X_1 and Y_2 for Y_3 than does comparison of coefficients from the other statistical techniques considered. The effects coefficients, derived from path analysis, are superior to other coefficients for this purpose because they are free of spuriousness and incorporate indirect effects known to be operating in the system.

A final, practical note on the utility of the effects coefficient is perhaps in order. The die-hard empiricist might argue that computation of effects coefficients is needless effort, for even though it may actually be more congruent with the causal system, the coefficients will not yield an interpretation of relative importance substantively different from one of the other statistical techniques. By actually calculating E_{ki} for Figure 5, however, and by comparing them to other coefficients, one sees how this argument does not hold. Correlations reported by Sharkansky and Hofferbert in their study of public policy in the American states serve as a convenient and relevant data-base for deriving the necessary estimates.[36] In Table 1 are presented their correlations among the socioeconomic variable, Affluence (X_1), the political variable, Competition-Turnout (Y_2), and the policy variable, Welfare-Education (Y_3).

Table 1. Coefficients of Simple Correlation Between Affluence (X_1), Competition-Turnout (Y_2), and Welfare-Education (Y_3)

	X_1	Y_2	Y_3
X_1			
Y_2	.66		
Y_3	.69	.68	

Sharkansky and Hofferbert compare partial correlations, concluding that neither Competition-Turnout nor Affluence is "significantly more important than the other."[37] The partial correlations from the data of Table 1 are, respectively, $r_{31.2} = .44$, and $r_{32.1} = .41$. Likewise, an evaluation of the simple correlations (see Table 1) or the beta weights from multiple regression ($\beta_{31.2} = .43$, and $\beta_{32.1} = .41$) suggests that socioeconomic and political factors are of about equal importance for welfare policy.

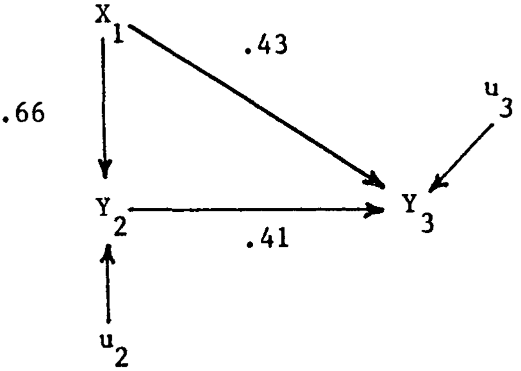

Figure 6. A Recursive Model of Welfare Policy

The recursive model of welfare policy depicted in Figure 5 appears in Figure 6 with its paths estimated from the correlation matrix. Computing the E_{ki} in the manner outlined above, $E_{31} = p_{31} + p_{21}p_{32} = .43 + .27 = .70$, and $E_{32} = p_{32} = .41$. Evaluating the two effects coefficients, one observes that political factors, while influencing welfare policy, do not seem nearly so important as socioeconomic conditions. Thus, if Figure 6 is correctly specified, i.e., depicts the structure of relationships in the real world, then the interpretations based on coefficients from simple correlation, partial correlation, or multiple regression are simply wrong. Because these coefficients, unlike E_{ki}, do not correspond to the model, they lead to erroneous empirical judgments of the relative importance of socioeconomic and political variables for welfare policy.[38]

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[36] Sharkansky and Hofferbert, pp. 876–877.

[37] Sharkansky and Hofferbert, p. 877.

[38] When totally different causal structures underlie the statistics employed for evaluation, comparable results cannot reasonably be expected. In an analysis of two distinct data sets, not only were the distances between the effects of variables altered considerably depending on whether simple correlation, partial correlation, standardized partial regression, or effects coefficients were used, but there was also an occasional discrepancy in sign, and the rank ordering of the variables in terms of their effects was changed in a great many instances (Michael S. Lewis-Beck and Lawrence B. Mohr, "Evaluating Effects of Independent Variables: A Path Analytic Approach," Institute of Public Policy Studies (Ann Arbor: The University of Michigan, Discussion Paper #59).

Conclusion

The competing conclusions on the relative importance of socioeconomic and political variables for public policy that have been appearing since Dawson and Robinson's catalytic article are generally distorted, because of the inadequacy of the zero-order correlation, partial correlation, or multiple regression techniques on which they are based. The coefficients from any of these statistical techniques, respectively, do not provide satisfactory comparisons of effect because they correspond to unrealistic and, in the case of partial correlation coefficients, contradictory models of public policy. To assess accurately the relative importance of these independent variables, it is first necessary to specify correctly the underlying causal structure and estimate its parameters. Then, the path coefficients must be analyzed to determine the effects of the socioeconomic and political variables. An examination of the effects coefficients, derived from this path analysis, is offered as the preferred method for assessing and comparing the effects of these variables. When the effects coefficients for a common model of welfare policy are estimated in a data-based example, socioeconomic variables are found to be considerably more important than political variables. This conclusion differs substantially from interpretations founded on an application of the other statistical techniques reviewed here.

Political Literature and the Response of the Reader: Experimental Studies of Interpretation, Imagery, and Criticism*

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Introduction

The impact of literature on the social process has received only sporadic attention by political and social scientists. The issue engaged Socrates who, although an admirer of Homer, was suspicious of the influence of music and literary works and was firm in his conviction that the only poetry that ought to be allowed into the Republic were those hymns praising the gods and famous men: "For if you go beyond this and allow the honeyed muse to enter. . . , not law and the reason of mankind, which by common consent have ever been deemed best, but pleasure and pain will be the rulers in our State."[1] Socrates merely voiced his suspicions, however, and the mystery remains: literature, music, and verse appear to have *some* influence on the human soul, but no one seems quite certain what this is or how it operates.

The plight of the humanities has been complicated in modern times because of their conflict with science. C. P. Snow[2] has pointed to a lack of understanding between those with scientific training and the rest of the Western elite whose basic education has been in the humanities. Blotner, for example, regards the two approaches as poles apart: "Whereas the scientist is dedicated to objectivity and statistical accuracy, the novelist is often consciously subjective."[3] Leavis has similarly argued that a poem, unlike a meter reading, cannot be pointed to in any scientific sense: "It is 'there' only in the re-creative response of individual minds. . . ."[4]

According to the Commission on the Humanities of the American Council of Learned Societies, the humanities include those studies

> . . . that help man to find a purpose, that endow him with the ability to criticize intelligently and therefore to improve his own society, and that establish for the individual his sense of identity with other men both in his own country and in the world at large.[5]

No one, of course, doubts the importance of the goals of the humanities, but as Maxwell has pointed out, evidence is lacking which shows the connection between the study of the humanities and the goals they espouse[6] Hence, as he goes on to say, research in the humanities frequently lacks direction and purpose, and this has an important methodological consequence – namely, that few criteria exist for judging research methods and results in the humanities:

> To oversimplify, there is no one or no single group of even nominally neutral research techniques by means of which a researcher arrives at an interpretation of "Ode on a Grecian Urn" which is necessarily duplicative by other researchers. Thus there is no defined set of such interpretations, no objective criteria for determining which interpretation or interpretations

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*Revised from a paper originally presented at a meeting of the American Political Science Association, Chicago, 1974. Appreciation is expressed to Rondal G. Downing and William Stephenson, both of the University of Missouri – the former for having shown me the importance of literature to the political process, the latter for having shown me how to begin measuring it. Ellen Siegelman was unusually helpful in clarifying matters of both method and substance.

[1] Plato, *The Republic,* trans. Benjamin Jowett (New York: Heritage Press, 1944), p. 537.

[2] C. P. Snow, *The Two Cultures: And a Second Look* (London: Cambridge University Press, 1963).

[3] Joseph Blotner, *The Political Novel* (Garden City, N.Y.: Doubleday, 1955), p. 5.

[4] F. R. Leavis, *Two Cultures? The Significance of C. P. Snow* (New York: Pantheon, 1963), p. 48.

[5] *Report of the Commission on the Humanities* (New York: American Council of Learned Societies, 1964), p. 19.

[6] W. David Maxwell, "A Methodological Hypothesis for the Plight of the Humanities," *AAUP Bulletin,* 54 (March, 1968), 78–84. As Buckingham earlier despaired, "All of us believe that attitudes are influenced by literature. . . . Yet if we were challenged to defend our belief, we should be hard put to it to make out a convincing case." See Leroy H. Buckingham, "The Development of Social Attitudes Through Literature," *School and Society,* 52 (November 9, 1940), 453.

add most to knowledge or which contribute most to the goals of the humanities.[7]

The division between science and the humanities is often said to revolve around what is objective and what is subjective. Hence, when we talk about the interpretation of a book or poem, appeal is often made to "mental events which occur in the reader."[8] But the dichotomy is perhaps too extreme. Just as subjectivity is implicated in much of science, as Kuhn[9] has argued, so is there an objective component to the humanities, as Stephenson, in response to Maxwell, has demonstrated with respect to interpretations of Keats's *Ode.*[10] To say that a phenomenon is subjective or only a matter of opinion is not to deny its status as an object of scientific inquiry. Intelligent individuals may disagree about the "best" interpretation of a poem or a political novel, and may even disagree about what is to qualify as "literary" in a normative sense, but points of view and interpretations are apt to display similar and dissimilar features which are replicable and capable of systematic comparison.

It is the intent of this paper neither to argue the validity of literary interpretations nor to justify the inclusion or exclusion of writings as works of literature, but rather to outline a methodology for their study. Substantive conclusions will therefore be featured only insofar as they serve to provide illustrations of principles more generally applicable in scientific inquiry. As a preliminary matter, no distinction is made between fiction and nonfiction, prose and verse, or highbrow and lowbrow. All are alike in kind to the extent that they explicitly or implicitly address political themes and engage a reading audience. In this broad sense, the label "political literature" also incorporates political campaign literature, as well as the literature of academic political science.

What is intrinsic to political literature is the response which it stimulates – the "re-creative response" of individual minds to which Leavis has drawn attention – and the most important feature of this response is its subjective nature. Unlike facts, literary interpretations cannot be confirmed or refuted, but only advanced; consequently, they are matters of preference and, as such, are subjective. They are capable of detailed examination nonetheless, and it is to the study of the subjectivity of reader response that the following pages are addressed.

The methodological principles at issue are illustrated first through an analysis of differing interpretations of Golding's *Lord of the Flies.*[11] Consideration will then be given more directly to experimental procedures, focusing on Mazlish's psychohistorical account of Richard Nixon.[12] Principles will then be outlined for the study of literary effect in the single case, using Burdick's political novel, *The Ninth Wave.*[13] Inasmuch as these works have attracted considerable attention and are easily available, familiarity with them will be assumed.

Interpretations of Golding's *Lord of the Flies*

On the face of it, Golding's novel is the story of a group of young boys on an island, their attempt to institute a workable political and social system, and their eventual regression to a state of barbarism as the voice of reason (represented by Ralph, Piggy, and Simon) is eventually overcome by man's baser tendencies (Jack and Roger). According to the author:

> The theme is an attempt to trace the defects of society back to the defects of human nature. The moral is that the shape of a society must depend on the ethical nature of the individual and not on any political system however apparently logical or respectable.[14]

Many viewpoints have been expressed about the meaning of this novel, which is so complex that it invites idiosyncratic interpretation. For purposes of illustrating the methodological points at issue, 45 students in a senior seminar in political theory were instructed to read the novel and to render their own interpretations of it. The more than 200 statements generated by this exercise were typed one to a card, forming what might be termed a population of interpretations, some of which seemed to match Golding's. For example, one student wrote:

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[7] Maxwell, p. 82. Maxwell overstates his case somewhat: Much contemporary historical and interpretative criticism in literature is quite rigorous with the critic arguing directly from textual material available for others to see. Literary interpretation is not unguided impressionism, and, within limits characteristic of all explanations, many careful literary interpretations are replicable.

[8] F. L. Lucas, *Literature and Psychology* (London: Casswell and Co., 1951), p. 204.

[9] Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 2nd ed. (Chicago: University of Chicago Press, 1970).

[10] William Stephenson, "Applications of Communication Theory: II. Interpretations of Keats' 'Ode on a Grecian Urn'," *Psychological Record,* 22 (Spring, 1972), 177–192.

[11] William Golding, *Lord of the Flies* (New York: Capricorn, 1959).

[12] Bruce Mazlish, *In Search of Nixon* (New York: Basic Books, 1972).

[13] Eugene Burdick, *The Ninth Wave* (Boston: Houghton Mifflin, 1956).

[14] Golding, p. 189. Golding's pessimistic view of mankind was somewhat modified in some of his later works.

> The novel shows how a small-scale society can develop and, through the differences and misunderstandings of man, crumble before it really has a chance to work.

Others, on the other hand, seemed to reflect interests that the reader had more obviously projected onto the novel:

> The novel has Marxist overtones, of the connections between economic conditions and social structure.

A sample of 50 of these interpretative elements was drawn, with efforts being made to ensure comprehensiveness, and the item sample was then given to the students, each of whom was instructed to render his interpretation of the novel once again, this time in the form of a *Q*-sort – i.e., by ranking the 50 statements from "most like my interpretation of the novel" (+5) to "most unlike my interpretation" (–5), each *Q*-sort thereby providing operational substance to "the recreative response of individual minds" deemed by Leavis to be of central importance. The *Q*-sorts were then correlated and factor analyzed; a portion of the factor matrix is shown in Table 1. Factor scores were then estimated for the 50 statements in each of the four factors.[15]

As Table 1 indicates, the interpretations rendered by the students tended to fall into four identifiable categories, consequently producing results that might serve to cast doubt on Maxwell's earlier contention that there is no technique by means of which a researcher can arrive at literary interpretations duplicative by others. The first six subjects, for example, reached essentially the same interpretation (factor A); persons 7–11 on the other hand reached a different interpretation (factor B), as did subjects 12–16 (C) and 17–20 (D). The interpretations of subjects 21–24 were mixed, having something in common with more than one of the main interpretations; individuals 25 and 26 advanced unique interpretations unrelated to factors A to D.

Table 1. Four Factors Representing Interpretations of Golding's *Lord of the Flies*

Subjects	Factors A	B	C	D
1	(73)	12	37	02
2	(69)	–08	35	–12
3	(69)	07	25	–04
4	(66)	28	12	09
5	(65)	04	07	–10
6	(65)	30	–07	37
7	04	(51)	32	–09
8	37	(50)	31	31
9	–11	(48)	04	24
10	38	(47)	11	14
11	30	(45)	34	–02
12	16	04	(60)	23
13	34	09	(56)	10
14	06	03	(53)	02
15	–22	31	(44)	04
16	30	31	(43)	15
17	01	19	12	(48)
18	38	24	08	(46)
19	31	08	–01	(43)
20	07	–12	10	(42)
21	(57)	(61)	04	01
22	(52)	(49)	–02	–08
23	(54)	17	(56)	22
24	13	–22	(56)	(41)
25	09	00	17	06
26	10	24	19	23

Factor loadings in parentheses significant ($p<.01$).

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[15] Briefly, each reader provided a formal model of his interpretation of *Lord of the Flies* by *Q*-sorting the 50 statements from +5 (most like my interpretation) to –5 (most unlike) in a forced, quasi-normal distribution. The rank-ordering of the statements by each of the $n = 45$ students was then correlated with the rankings of all other students, eventuating in a 45 x 45 correlation matrix. Factor analysis indicates which students reached similar understandings of the novel, as determined by the magnitude of their intercorrelations, with each factor representing a different interpretation as advanced by readers with high loadings on that factor. The content of these differing interpretations is described in terms of the factor scores – i.e., on the basis of the scores (ranging from +5 to –5) assigned each of the statements in each of the factors. The scores which follow therefore indicate the extent to which groups of readers of the same type (with different types designated as factors A, B, C, and D) regard specific statements as congruent with (positive) or opposed to (negative) their own interpretations of the novel. The most detailed treatment of *Q* methodology is in William Stephenson, *The Study of Behavior* (Chicago: University of Chicago Press, 1953). Perhaps the best short introduction is by Fred N. Kerlinger, "*Q* Methodology in Behavioral Research," in *Science, Psychology, and Communication*, ed. Steven R. Brown and Donald J. Brenner (New York: Teachers College Press, 1972), pp. 3–38.

Factor A. The distinctiveness of each interpretation can be grasped most readily through an examination of the statements that distinguish each factor from the others. Among the interpretive statements distinguishing factor A are the following, with displayed scores being associated with factors A to D respectively, and with the factor A scores in parentheses (+5 = "most like my interpretation," −5 = "most unlike my interpretation"):

19. (+5) 0 −2 −1 Jack personifies man's beastly instincts unrestrained by rational control.

37. (+5) +2 +2 −5 Ralph appealed to rationality and to order; Jack appealed more to emotions and passions. Golding shows how rationality gives way to appetite.

41. (+5) −4 −2 −4 The theme of Golding's novel is that mankind is inherently savage, that when the thin veneer of civilization is stripped away, we are all little more than animals.

35. (+3) −2 −2 −1 Ralph's participation in Simon's murder indicated that he is subject to the same savage impulses as the others.

The interpretation of the novel rendered by these students has affinities with the psychoanalytic theory of infantile savagery lurking beneath the social and political forms of civilized life, according to which the children on the island have "degenerated into adults."[16] This view was expressed most succinctly in the original review essay of the novel by student No. 1, who displays the highest association with factor A (see Table 1): "Man's basic struggle is within himself, between his conscience, the product of society's moral codes, and his basic drives which are inherited and largely destructive."

Factor B. Persons associated with factor B, on the other hand, seemed to hold a more complicated view of human nature, one in which the individual is considered not to be fundamentally evil but a mixture of both good and bad; moreover, these subjects appeared to be more sensitive to the symbolism involved in the novel, and placed greater emphasis on character than on political superstructure (scores for factor B in parentheses):

1. +1 (+5) +1 −2 Simon is almost Christ-like — a loner, a person of little fear, concerned for the welfare of the littluns, the first to understand the source of fear, the bringer of truth who is killed for his efforts.

32. +3 (+5) +3 −3 Golding shows that the shape of a society must depend on the ethical nature of the individual and not on any political system, however apparently logical or respectable.[17]

13. +1 (+4) −1 +2 Although presented as two separate characters, Ralph and Jack symbolize conflicting tendencies in us all — the capability for both good and evil.

17. +1 (+4) −1 −1 The novel depicts the futility of organizing political systems when the real conflict is untouchable by such external inventions.

The tendency on the part of factor B students to take a more sophisticated and dialectical view of human nature is further revealed in their unwillingness to localize negative characteristics in any single figure or group in the novel, as indicated by their strong disagreement with the following:

25. +4 (−3) +4 +2 The littluns are that great segment of society that has no concern for important matters, that likes to complain, and will go mindlessly to the place where the grass looks greener.

28. −1 (−4) +5 +1 Jack was an antagonist, and from the first showed that he felt himself to be superior. A small-time tyrant, he wanted nothing but recognition.

From the standpoint of these readers, there are no tyrannical or ignorant people *per se:* We are all capable of tyranny and ignorance. The blame, they go on to imply, rests with the forces unleashed by social organization itself; at the same time, they acknowledge the importance of social constraints:

15. −5 (+2) −5 −5 Ralph's initial blowing of the conch, like Adam's eating of the apple, was a kind of original sin signaling the beginning of the end.

31. +1 (+4) 0 0 Roger shows the sadism that exists when man is stripped of society's taboos.

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[16] Claire Rosenfield, " 'Men of a Smaller Growth': A Psychological Analysis of William Golding's *Lord of the Flies*," *Literature and Psychology*, 11 (Autumn, 1961), 93–101.

[17] Item No. 32 represents Golding's own view (see note 14). All of the other statements were drawn from the students' essays.

In short, the response to the novel represented by factor B approximates an uneasy merger of Rousseau and Hobbes – a belief in the existence of that which is good, and acceptance of the need to control that which is not. Since persons composing this factor most warmly embraced Golding's own viewpoint (item no. 32, *supra*), this view probably embodies the dilemma the author was attempting to dramatize.

Factor C. In his original review essay of Golding's novel, subject No. 12, whose interpretation of the novel was highly saturated with factor C (see Table 1), wrote as follows: "The book reflected many of my own personal true-to-life situations where things got fouled up because of someone's selfishness or laziness." Students whose *Q*-sorts define factor C appear to experience *Lord of the Flies* as an hyperbole of life in which the best of intentions are often frustrated by shortsighted pettiness and misunderstandings based on fear (scores for factor C in parentheses):

27. +3 +2 (+5) +3 Lack of attention to the fire indicated that immediate needs, such as hunting for food, had taken precedence over future needs, an example of the short-sighted social decay that was in progress.

21. +2 +1 (+4) +2 What Golding portrays is not the effects of stupidity, but of genuine human tragedy – not of misunderstanding, but of reacting against a fear that one is incapable of understanding.

3. –2 0 (+3) –3 The novel shows how a small-scale society can develop and, through the differences and misunderstandings of man, crumble before it really has a chance to work.

2. +2 +5 (–3) +4 It is no mere accident that Piggy, as the representative of rationality, is the person most ignored by the group, and that it is he who is victimized in broad daylight without regret.

The social disintegration portrayed in the novel is seen by these readers as being due to shortsightedness (item No. 27) based on misunderstanding (No. 3). This misunderstanding is qualified in item No. 21, however: It is not merely misunderstanding stemming from ignorance, but from fear. The apparently random nature of social decay is expressed in No. 2 where Piggy's symbolic role is denied: Presumably any one of the children could have met his fate; that it was Piggy was coincidental.

The pettiness which factor C subjects mention is expressed in their attitudes toward leadership as manifested in the novel:

28. –1 –4 (+5) +1 Jack was an antagonist, and from the first showed that he felt himself to be superior. A small-time tyrant, he wanted nothing but recognition.

10. –2 –1 (+3) 0 Ralph was an ideologist, holding out until the end the idea of rescue, and obsessed with maintaining the fire, the major policy of his regime.

49. –1 –4 (+2) –3 Piggy is the academician – he knows the way, but proceeds with boredom and self-righteousness.

None of the major figures is appreciated: There is no mixture of good and bad, only vanity and other defects in character. As a consequence, perhaps, factor-C readers reveal a disproportionate concern for formal structure as a means for controlling human vagaries:

29. –4 –3 (+2) –4 With no formal army or police or any other means to enforce the rules, society weakens and falls.

The discrepancy between this structuralist and Golding's own psycho-ethical view (see note 17) – also evident in factor C – suggests a conflicted attitude toward human nature which the reading of the novel may have restimulated for these subjects. Unlike factor B readers, who have accepted the inconsistencies of human nature, factor C subjects are struggling to resolve them.

Factor D. Finally, those readers whose *Q*-sorts comprise the fourth factor reacted in a stereotypical fashion, and their projection of unsophisticated ideological categories onto the story results in a most unconvincing interpretation (scores for factor D in parentheses):

23. –5 –5 –4 (+5) Considering the time in which it was written, the novel seems to symbolize the battle between democracy (represented by Ralph and Piggy) and communism (depicted by Jack).

36. –2 –3 0 (+5) The fact that they decided to vote for a leader showed that even a new government has to rely on old methods to assure smooth operation.

11. –4 –3 –4 (+4) The novel has Marxist overtones, of the connections between economic conditions and social structure.

32. +3 +5 +3 (–3) Golding shows that the

> shape of a society must depend on the ethical nature of the individual and not on any political system, however apparently logical or respectable.

The extent to which factor D readers disagree with statement No. 32, which represents Golding's own viewpoint, is a measure of the extent to which they have apparently missed the more fundamental point of the novel.

These four factors point to vastly different interpretations of Golding's novel, from the rather unsophisticated projections of ideology (factor D) and personal conflict (factor C), to the more conventional viewpoint of man's essential evil (factor A) and the more mature interpretation of the duality of human nature (factor B). The source of these diversities no doubt lies in the rich ambiguity of Golding's thought and the diversity of each reader's history and personal development – the dialectical clash, Sartre has said, between an act of creation (writing) and one of perception (reading), the literary object having "no other substance than the reader's subjectivity."[18] Proust had something similar to say:

> In reality, each reader reads only what is already within himself. The book is only a sort of optical instrument which the writer offers the reader to enable him to discover in himself what he would not have found but for the aid of the book.[19]

It would be difficult to deny the subjectivity involved in the reading of literature, but does subjectivity necessarily imply a degree of idiosyncrasy that precludes the kind of inquiry normally associated with science? Lucas might be seeming to say so:

> Let us call the printed poem "P", and the readers "R1", "R2", and so on. When R1 and R2 imagine they are differing about the same object – "P", they are really differing about "P + R1" and "P + R2". Which are clearly two different things.[20]

But R1 and R2, like the readers in the study above, are also not apt to be totally different – i.e., an infinite number of *totally* dissimilar interpretations is not expected, and through the use of *Q*-technique or related procedures, it is quite possible, as has been shown, to identify those persons who have reached basically the same understanding no matter how subjective it may be. In this respect it is theoretically important to distinguish subjectivity from idiosyncrasy.[21]

The four factors point to a certain objective substructure among interpretations of Golding's novel, a "defined set of interpretations" of which Maxwell thought the humanities incapable. Interpretation, then, is not idiosyncratic; it is *not a matter of each person his own* interpretation. Indeed, wherever subjective thought is at issue, as in interpreting literature, there will invariably emerge a limited number of factors of *operant subjectivity.*[22]

Experimental Methods: Impression and Expression

The preceding study illustrates a way in which subjective aspects of literary response can be placed on an operant footing and held steady for purposes of inspection, an instrumental prerequisite to more detailed analyses under experimentally controlled conditions. Prior to illustrating the principles at issue, however, several divergent trends in experimentation require elaboration.

Machiavelli adopted a broad view of experimentation, regarding it as merely a scientist's way of asking Nature a question. Experimentation nowadays, however, is often limited to a laboratory setting and to the measurement of "objective" (i.e., public) events, often of a physiological nature. There are exceptions. McConahay,[23] for example, does not equate experiments with laboratory work exclusively, including under the rubric those field studies in which subjects have no control over the treatments they receive. Lodge and Tursky, on the other hand, emphasize the laboratory analysis of behavior, adopting a framework that "focuses on behavior, not beliefs, feelings, evaluations, or whatever as predispositions, but on behavioral responses themselves,"[24] and ultimately focusing on physiological responses. But what, it may be asked, are beliefs, feelings, or evaluations if not behavior, and how can an experimental analysis of beliefs and feelings be advanced if the subject matter is not allowed? Beliefs and feelings are no doubt complicated phenomena, but the solution to understanding them is not to ignore their existence and to study something else instead, but to examine them more closely under improved conditions.

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[18] Jean-Paul Sartre, *What Is Literature?,* trans. Bernard Frechtman (London: Methuen, 1950), p. 31.

[19] Marcel Proust, *Remembrance of Things Past,* trans. Charles K. Scott Moncrieff and Frederick A. Blossom (New York: Random House, 1932), II, p. 1024.

[20] Lucas, *Literature and Psychology,* pp. 204–205.

[21] It is likewise important to note that this distinction is preserved statistically. Hence, all 45 *Q*-sort interpretations of Golding's novel were subjective, each from the reader's own point of view and no two exactly alike. Factor communality, however, points to aspects of interpretation that are shared; systematic but noncommunal variability is specific to the individual, i.e., idiosyncratic but reliable; the remaining variance is unsystematic and attributed to error.

[22] On the concept of factors as subjective operants, see William Stephenson, "Application of Communication Theory: III. Intelligence and Multivalued Choice," *Psychological Record,* 23 (Winter, 1973), 17–32.

[23] John B. McConahay, "Experimental Research," in *Handbook of Political Psychology,* ed. Jeanne N. Knutson (San Francisco: Jossey-Bass, 1973), pp. 356–382. See also Virginia Black, "Laboratory Versus Field Research in Psychology and the Social Sciences," *British Journal for the Philosophy of Science,* 5 (February, 1955), 319–330.

Taking stock of the total corpus of experimental methods employed in the psychophysics of his time, Beebe-Center[25] found they could be divided into two groups: The methods of impression and those of expression. The former emphasize the relationship of a variable to stimuli, the latter emphasize the relationship to response. Among the methods of *impression* were the order of merit (ranking), paired-comparison, and single exposure. Hence a subject might be requested to rank a series of odors from "most pleasant" to "most unpleasant," or a series of weights from "heaviest" to "lightest." The subject is relating stimuli (odors, weights) to a variable (pleasantness, heaviness). The methods of *expression* seek to establish a relationship between a variable (such as pleasantness) and a response (such as heartbeat). Hence, upon receiving pleasant news, one's pulse may quicken.

Experimentation is therefore a broad method of inquiry not necessarily limited by subject matter, and what Lodge and Tursky have called experimental really only relates to that half of experimentation covered by the methods of expression. Different subject matters, of course, require their own methods, and so far as subjectivity is concerned, this gives rise to the following methodological axiom:

> . . . *objective measurements and observations* [expression] *can, in principle, be made by everyone (or by a piece of apparatus), whereas measurements and observations of a person's subjectivity* [impression] *can be made only by himself.*[26]

Thus, upon reading the concluding pages of Golding's novel, highly involved individuals may have experienced a quickened heartbeat; such a reaction is a matter of expression, a behavior over which they may exercise no control, and comparable to an external standard of "average heartbeat." Their interpretations of the novel, however, are subjective, can be given only by themselves, and cannot be compared without controversy to any objective standard, only to alternative interpretations. Nevertheless, both heartbeat and interpretation are stable and replicable phenomena – e.g., the four Golding factors could easily be replicated through readministration of the same Q-sample – and both, in principle, can be dealt with experimentally.

An Experiment on Mental Imagery

As a discipline, history stands on the boundary between the humanities and the social sciences, and so does its newest subdiscipline, psychohistory. Relying on theories of human motivation, the psychohistorian attempts to reach a deeper understanding of historical events and to transmit this understanding to others. Still, the psychohistorical facts are open to various interpretations, and the effectiveness of the enterprise depends both on the skill of the author and on the predispositions of his audience. What is at issue, therefore, is subjectivity, both the psychohistorian's and the reader's.[27]

In this study, an attempt was made to measure the effect of reading Mazlish's psychohistorical interpretation of Richard Nixon on students' images of the President. A sample of 54 statements, made by or about Nixon, was taken from Mazlish's volume, supplemented by statements from Barber's *The Presidential Character,*[28] and placed in the factorial design shown in Table 2. The psychoanalytic theory of the ego mechanisms, upon which most psychohistorical accounts rely, is outlined in detail by

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[24] Milton Lodge and Bernard Tursky, "The Analysis of Political Behavior: A Bio-Behavioral Approach," *International Studies Newsletter,* Preliminary Issue A (Fall, 1973), p. 58.

[25] J. G. Beebe-Center, *The Psychology of Pleasantness and Unpleasantness* (New York: D. Van Nostrand, 1932).

[26] William Stephenson, "Applications of Communication Theory: I. The Substructure of Science," *Psychological Record,* 22 (Winter, 1972), 17. Italics in original.

[27] See in this connection David Jaffin, "The Methodology of the Historian and Psychoanalysis: Can Subjectivity Be Eliminated?" *International Journal of Offender Therapy,* 12 (No. 2, 1968), 85–89.

[28] Mazlish, *In Search of Nixon;* James D. Barber, *The Presidential Character* (Englewood Cliffs, N.J.: Prentice-Hall, 1972).

Kroeber,[29] who states that for each ego defense there is a coping equivalent that manifests itself in cognitive and impulse behaviors. Hence, there are cognitive defenses (isolation, intellectualization, rationalization) and their coping equivalents (objectivity, intellectuality, logical analysis); impulse defenses (displacement, reaction formation, repression) and their coping counterparts (sublimation, substitution, suppression); and areas in which both impulses and cognitions are involved, engaging both the defenses (denial, projection, doubt, regression) and their equivalent coping strategies (concentration, empathy, tolerance of ambiguity, playfulness). Consequently, when it is said of Nixon that he is "outwardly unruffled and composed, but anger boils inside," we tentatively judge this to reflect denial, a cognitive and impulse defense, hence (*ad*) in Table 2. In a similar way, when it is said that he "experiences some tension but deals with it effectively so that it does not interfere with his ability to think clearly," we judge this to refer to the ability to concentrate, hence (*bd*) in Table 2. In this instance, $m = 9$ replications (statements) of each combination were selected for a statement sample of $m\text{AB} = 54$.

In order to probe into possible psychodynamics involved in political imagery, twelve students in a seminar on political psychology were instructed to operate with the Nixon *Q* sample under a variety of conditions of instruction:

1. *Self:* Give a description of yourself by *Q*-sorting the statements from "most characteristic of me" (+4) to "most uncharacteristic of me" (−4).
2. *Ego ideal:* Give a description of the kind of person you would like to be, from +4 (most ideal) to −4 (least ideal).
3. *Conscience:* Describe the kind of person you were taught that you ought to be.
4. *Perception of Nixon,* $S \rightarrow N$*:* Describe Richard Nixon as you see him, from "most like Nixon" (+4) to "most unlike Nixon" (−4).
5. *Nixon's self-perception,* $S \rightarrow (N \rightarrow N)$*:* Describe how you think Nixon conceives of himself, i.e., the way you believe he would describe himself (from +4 to −4) if he were to use this sample of statements.

The first three instructions, each represented by a separate *Q*-sort using the same statements, were designed to help determine if there were any initial differences among the students that might subsequently be useful in explaining the results of the main study. After the students had read Mazlish's *In Search of Nixon,* instructions 4 and 5 were repeated, resulting in two additional *Q*-sorts from each of the twelve subjects:

6. *Perception of Nixon* (post): Same instructions as No. 4.
7. *Nixon's self-perception* (post): Same instructions as No. 5.

The main intent, of course, was to compare *Q*-sorts 4 and 5 with 6 and 7 in order to determine whether the reading in any way influenced the pre-reading images.

The first 60 *Q*-sorts, representing the first five instructions for each of the twelve subjects, were intercorrelated and factor analyzed, resulting in four simple-structure factors (I, II, III, IV). Examination of the figures indicated the subjects themselves to be of three different types so far as their self-descriptions were concerned:

> Factor I subjects, of which there were three, described themselves as serious-minded, analytic, and mentally on-guard, with high factor scores going to statements such as "Wends his way alertly, always prepared to deal realistically with whatever comes next," and "Anticipates a situation and analyzes it with care and impartiality, letting his mind roam freely over alternatives." These subjects will be referred to as *analytic* in orientation.
>
> Factor II subjects, numbering four, described themselves as active and outgoing. The following item, for example, received the highest score in this factor: "Argues with zest, and otherwise diverts his energy into vigorous activity, enjoying himself immensely in the pro-

Table 2. Factorial Design of Nixon *Q*-Sample

Main Effects	Levels		
A. Mechanisms	(*a*) defense		(*b*) coping
B. Manifestation	(*c*) cognitive	(*d*) cognitive-impulsive	(*e*) impulse

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[29] Theodore C. Kroeber, "The Coping Functions of the Ego Mechanisms," in *The Study of Lives,* ed. Robert W. White (New York: Atherton, 1963), pp. 178–198.

cess." These subjects will be referred to as *extraverted.*

The five factor III subjects were more withdrawn, describing themselves by giving the highest score to the following statement: "Restrains himself from expressing feelings or viewpoints he deeply feels and wants to get across." These subjects will be referred to as *withdrawn.*

The factor results of the Ego Ideal and Conscience *Q*-sorts revealed that virtually all of the students described their ideal and moral selves in the serious minded terms reflected by factor I. This is interpreted to mean that both the extraverted subjects (factor II) and the withdrawn subjects (factor III) would *prefer* to be (the Ego Ideal sort) and were *taught* to be (the Conscience sort) like the kind of character represented by factor I – cautious, realistic, analytic. Factor I subjects, however, have internalized their conscience: Their self, ideal, and conscience are essentially the same, i.e., are all highly loaded on factor I.

The results in the S→N section of Table 3 ("Before" portion) indicate that virtually all of the subjects viewed Nixon in terms of factor IV, although three of them (subjects 1, 6, and 10) viewed him as the opposite of their ideals – with negative loadings on factor I – and one of them (subject 12) viewed him in idealized terms, i.e., positive on factor I. Factor IV, however, was the predominant image. Among the statements distinguishing IV are the following (scores for factors I to IV respectively, with factor IV's scores in parentheses):

3. 0 +2 +2 (+4) Tries to avoid any semblance of defeat or failure.

10. 0 –4 0 (+4) Carefully considers his words – thinks before he speaks, as he speaks, and again after he speaks – overly aware of the situation and of himself in it.

36. –2 –3 –3 (+4) Generally suspicious about the motives of those who oppose him.

27. –2 –4 –4 (+3) Cautious, planned, and almost mechanical in nature. Lacks spontaneity and the capacity to react naturally.

Table 3. Factor Structure of Before-After Perceptions of President Nixon

		S → N				S → (N → N)			
Subjects	**Type**	**I**	**II**	**III**	**IV**	**I**	**II**	**III**	**IV**
					Before				
1	A	(–50)	29	–11	(42)	(67)	11	06	17
2	A	–08	15	–25	36	(54)	01	38	–08
3	A	–21	–16	05	(58)	(84)	–07	03	06
4	E	–35	–08	23	(56)	(76)	–02	–05	24
5	E	–24	–08	–04	(67)	(71)	19	–18	–01
6	E	(–42)	–31	–15	(62)	(67)	–09	01	17
7	E	–03	–09	10	(49)	04	04	–08	37
8	W	–35	08	04	(63)	(45)	15	(49)	19
9	W	14	–16	–14	(67)	(82)	–14	11	20
10	W	(–51)	01	–26	26	(69)	10	–06	–03
11	W	–11	–24	–03	(64)	(49)	05	05	(48)
12	W	(55)	–21	38	19	(73)	–20	27	14
					After				
1	A	–07	–16	–31	(54)	(60)	00	04	05
2	A	–12	–03	–26	29	(72)	15	–05	–14
3	A	–20	–31	–09	(71)	(75)	37	03	–17
4	E	(–46)	–20	–30	(53)	(42)	01	–07	34
5	E	(–43)	–04	–08	(58)	(76)	14	–01	–19
6	E	–23	–08	–14	(54)	(69)	12	–03	–14
7	E	16	07	–25	31	04	24	–16	32
8	W	–03	–12	12	(50)	(78)	24	14	–35
9	W	–13	–32	14	(65)	(49)	–07	21	08
10	W	–37	–10	–26	34	(71)	24	09	–22
11	W	–31	–11	–04	(54)	(42)	19	04	22
12	W	27	28	05	34	(79)	05	23	–07

Subject Type: A = analytic, E = extraverted, W = withdrawn. Factor loadings in parentheses significant ($p < .01$).

Two points stand out from these results. First, it is obvious that these subjects, regardless of their own personality type (analytic, extraverted, or withdrawn), do not consciously identify with Nixon as they think he really is: The description of the "real" Nixon is associated with factor IV, which is orthogonal to the subjects' self descriptions on factors I, II, and III, as well as to their Ego Ideal and Conscience concepts (on factor I). Second, although the subjects at this point had not been introduced to the psychohistorical account of Nixon, it is clear that their descriptions of him, shown in the factor scores above, match the over-controlled and active-negative character types described by Mazlish and Barber.[30]

The results in the S→(N→N) section of Table 3 ("Before" portion) provide further insight into the subjects' images. Whereas they perceive Nixon as a factor IV type (rigid, defensive, and paranoid), they perceive that he sees *himself* in idealized terms, i.e., as a factor I type (cautious, but realistic and analytic). The division is noteworthy: The subjects do not appear to doubt that Nixon sees himself in idealized terms (factor I), i.e., he is not seen as the self-consciously manipulative type represented by factor IV. But the subjects believe that they see through the facade of I to the more pernicious reality of IV, hence their irritation is with Nixon's duplicity and his perceived unwillingness to accept responsibility for, and to come to terms with, what the subjects see as the seamy side of his character.[31]

Following the administration of the first five *Q*-sorts, as described previously, the students read Mazlish's psychohistorical account of Nixon and spent several sessions discussing it. At this point, they were instructed to repeat *Q*-sorts 4 and 5, i.e., to redescribe how they perceived Nixon, S→N; and to redescribe how they thought he perceived himself, S→(N→N). These twenty-four new *Q*-sorts, two from each of the twelve subjects, were then correlated with the factor scores for the original factors, resulting in the figures shown in the "After" section of Table 3. The After results tell essentially the same story as the Before figures: Nixon is still seen primarily in terms of factor IV; his own self-image is perceived primarily in terms of factor I.

Any change in the magnitude of the factor loadings between Before and After could have been the result of several effects, some of sufficient subtlety to require further analysis: (1) The differing subject types (analytic, extraverted, withdrawn) might have reacted in dissimilar ways, e.g., the extraverted subjects, upon reading Mazlish's volume, may have been less susceptible to changing their views of Nixon than the other two types; (2) the two perceptions, S→N and S→(N→N), may have been differentially affected by the reading, e.g., the subjects' views of Nixon may have remained the same while their image of Nixon's self-perception might have changed; (3) there may have been interactions among the various influences, and so forth.

The factor loadings in Table 3 were therefore subjected to an analysis of variance, employing procedures similar to those outlined elsewhere,[32] with the loadings (f) first being transformed into Fisher's $Z = 1.15129 \log[(1 + f)/(1 - f)]$. The data were treated as in a split-plot (repeated measures) factorial design with one between-subjects effect,

A. Subject Type
  A1. Analytic
  A2. Extraverted
  A3. Withdrawn

and three within-subjects effects,

B. Before-After
  B1. Before
  B2. After

C. Perceptions
  C1. S→N
  C2. S→(N→N)

D. Factors
  D1. Factor I
  D2. Factor II
  D3. Factor III
  D4. Factor IV.

The within-subjects effects of the split-plot design are those to which all subjects are exposed. Effect A is a between-subjects effect since each subject is in one group only. Effect B is a within-subjects effect, however, in that all subjects received both the before and after treatment. The split-plot design therefore gives rise to multiple sources of variance: There is error associated with differences between groups of subjects, as well as error associated with the same subjects under differing conditions. Only the Before-After and Perception effects are truly experimental in that they are controlled conditions assigned under the principle of randomization; Subject-Type is an assessed variable, each subject bringing his type membership to the experiment (rather than being assigned to a type); the Factor effect is likewise quasi-experimental, the factor groupings having been discovered as naturally existent during the course of the study.

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[30] Professor Mazlish also kindly consented to provide a *Q*-sort representation of the way in which he believed Nixon would describe himself: That variate was also purely loaded on factor I by an amount 0.78.

[31] For parallel observations concerning the psychological reaction to Nixon, see Leo Rangell, "Lessons From Watergate: A Derivative for Psychoanalysis," *Psychoanalytic Quarterly*, 45 (January, 1976), 37–61.

[32] Thomas J. Cook, Frank P. Scioli, and Steven R. Brown, "Experimental Design and *Q*-Methodology: Improving the Analysis of Attitude Change," *Political Methodology*, 2 (No. 1, 1975), 51–69.

As shown in Table 4, the reading of Mazlish's study had little effect on the students' perception of Nixon or on their perception of Nixon's own self-image – i.e., the Before-After effect is not significant, nor is it significant in interaction with any of the other main effects. The only significant main effects are *C* (Perceptions) and *D* (Factors) which can be ignored since their interaction (*C* x *D*) is also significant. This interaction is itself artifactual, however, since it arises primarily because the factor I response is used to describe Nixon's own self-image, S→(N→N), whereas factor IV is used by the subjects as their image of what Nixon is really like, S→N, all of which is already obvious from an examination of the factor structure reported in Table 3.

By way of summary: 54 self-descriptive statements, made by or about Richard Nixon, were placed in a *Q*-sample to be used by twelve subjects to describe five images: Self, ideal self, conscience, perception of Nixon, and perception of Nixon's own self-concept. Following the reading of Mazlish's *In Search of Nixon,* subjects repeated the last two *Q*-sortings in order to measure the impact of Mazlish's volume on their perceptions of Nixon and Nixon's self-image. The comparative influence of the various experimental effects and their interactions was evaluated through an analysis of variance of the factor loadings shown in Table 3. As the results in Table 4 indicate, the reading of Mazlish's psychohistorical account had no significant affect on the subjects' perceptions.

The analysis of variance of the factor structure of this study has served the limited, but important, function of summarizing the topographical features of the data; I shall postpone to another time a more detailed description of the internal dynamics of the existing factors. The procedures outlined, however, point to the possibility of examining the impact of historical

Table 4. Analysis of Variance of Factor Loadings (Z-Transformations) from Table 3

Source	*df*	*ssq*	*ms*	*F*-ratio
BETWEEN Subjects	11	0.6328		
S Type (A)	2	0.1429	0.0715	1.3143
error 1	9	0.4899	0.0544	
WITHIN Subjects	180	26.3785		
Before-After (B)	1	0.0654	0.0654	4.5417
A x B	2	0.0033	0.0017	0.1181
error 2	9	0.1298	0.0144	
Perceptions (C)	1	1.6009	1.6009	44.1019**
A x C	2	0.0297	0.0149	0.4105
error 3	9	0.3267	0.0363	
B x C	1	0.0000	0.0000	0.0000
A x B x C	2	0.0101	0.0051	0.7846
error 4	9	0.0583	0.0065	
Factors (D)	3	4.5254	1.5085	20.0332**
A x D	6	0.4944	0.0824	1.0943
error 5	27	2.0319	0.0753	
B x D	3	0.1874	0.0625	1.5244
A x B x D	6	0.1306	0.0218	0.5317
error 6	27	1.1067	0.0410	
C x D	3	12.2924	4.0975	48.3766**
A x C x D	6	0.1728	0.0288	0.3400
error 7	27	2.2882	0.0847	
B x C x D	3	0.1657	0.0552	2.6667
A x B x C x D	6	0.2005	0.0334	1.6135
error 8	27	0.5583	0.0207	
TOTAL	191	27.0113		

$^{**}p < .01$

literature on readers of that literature. Although this particular experiment indicated that the work in question had no impact on attitudes toward the former President, it is possible that the effect could be greater on another audience. Furthermore, it must be remembered that at the time of the study, Richard Nixon was deeply embroiled in a controversy about which most members of the public had virtually unassailable viewpoints, so that reading Mazlish's book, for these students, represented just one more input into an already fixed attitude. Nevertheless, it is quite likely that individual historical treatises at certain times will have important impacts on public thinking, and that some variant of the procedures outlined can be employed in their evaluation.

It should be noted that the Nixon study involved a subject matter about which persons could be expected to have prior opinions, making a before-after design a suitable approach. Most political fiction, on the other hand, does not fit this model: The reader can have no prior attitudes toward a fictional account, except, perhaps, by analogy. In the Golding study, for example, a reader unfamiliar with the characters of Ralph and Piggy could have no *a priori* attitude toward them, although he might have an attitude toward those aspects of human nature which the characters represented. A strictly experimental procedure as normally conceived is consequently inapplicable, and it may be necessary under these conditions to adopt *adjuncts* to experimental method. The following study illustrates the lines along which such an approach might follow.

Dynamics of Response to Political Fiction

C. P. Snow[33] has posited an inverse relationship between interest in literature and commitment to science. This theme has been echoed by Waldo, among others, who has suggested that the technical background of the social scientist leaves him in a state of "trained incapacity" and unable to resonate with the humanities, but that "literature helps to restore what the professional-scientific literature necessarily omits or slights: the concrete, the sensual, the emotional, the subjective, the valuational."[34] It is said that the compulsion toward science has led to "a concentration on methods inappropriate to, or insufficient for, the production of significant meaning,"[35] and that the difficulty is due in part to divergent methods of testing reality. As McCurdy has said in this connection, "the social science theory is tested by its descriptive power; the fantasies of fiction are tested through the principle of authenticity, which is a subjective feeling about the essential trueness of a situation."[36]

A major complaint of the opponents of science is that the essence of literature and of life in general is too delicate and subtle, and at any rate is too often sacrificed on the altar of method, with scientific attention being directed toward countable irrelevancies at the expense of the significant. With the romance and song in the air, as e. e. cummings once wrote, "who cares if some oneeyed son of a bitch invents an instrument to measure spring with?" Science cannot permit itself to be governed totally by the dictates of the nonscientific community, of course, but neither can it afford totally to ignore warnings and complaints of substance. The scales, indices, and statistical manipulation of objective attributes that abound in social science, for example, are said to have the advantage of enabling investigators in diverse locations to measure the same thing, but it is also the case that much behavioral analysis of this objective *genre* is inclined to parallelism, i.e., it frequently ends up being a study of those expressionistic epiphenomenal events that occur alongside the impressionistic phenomena of more central interest.

Williams,[37] to provide an extreme example, sought to illuminate the literary styles of Bernard Shaw and H. G. Wells through examination of regularities in their frequencies of punctuation, which were indeed shown to vary systematically. We can, of course, admire the goal of objectivity sought by Williams, and it is doubtless the case that studies of this kind can be useful in certain ways, e.g., in helping to establish authorship. But with respect to political fiction, it is reading and reflecting that are crucial, rather than the number of periods and commas measured by the pound, and it would be to the credit of an experimental science if it could devote more of its attention to these central activities.

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[33] Snow, *The Two Cultures.*

[34] Dwight Waldo, *The Novelist on Organization and Administration* (Berkeley: Institute of Government Studies, University of California, 1968), p. 5.

[35] James F. Davidson, "Political Science and Political Fiction," *American Political Science Review,* 55 (December, 1961), 860.

[36] Howard E. McCurdy, "Fiction, Phenomenology, and Public Administration," *Public Administration Review,* 33 (January/February, 1973), 54.

[37] C. B. Williams, "A Note on the Statistical Analysis of Sentence-Length as a Criterion of Literary Style," *Biometrika,* 31 (March, 1940), 360.

To provide an illustration of what a venture such as this might entail, I shall describe an experimental study of responses to one of Eugene Burdick's novels. Following this, a more intensive study of the response of a single subject is summarized indicating how experimental method can employ the single case for a deeper understanding of the dynamics of literary response.

Burdick's "The Ninth Wave." Eugene Burdick's *The Ninth Wave*[38] is a novel about Mike Freesmith, a rather amoral character guided by a single-minded desire to understand human motivation in terms of its most fundamental determinants. Operating on the principle that "Hate + Fear = Power," Freesmith succeeds in maneuvering his candidate, John Cromwell, through to the Democratic nomination for Governor of California. In the end, Freesmith is drowned by his long-time friend, Hank.

Burdick's novel was read as part of a class assignment in a graduate seminar on political behavior. The students, nineteen in all, were instructed to write a "book review," not of the conventional kind (involving plot, theme, sequence, character delineation, and the like), but a critical review focusing on political implications and including how the novel made them feel.

Following distinctions made previously, the essays were examined and statements of fact were separated from those of opinion. The latter were then gathered and each was typed on a card, the sum total representing the universe of subjective discourse relating to the novel, examples of which are as follows:

1. The wheeling and dealing underlying the political campaign, and the theory of human nature held by Mike Freesmith, were, for me, fascinating.
2. While reading, I felt I might like to try some of Mike's techniques. I admired him because he had such great insights into people, and, in some ways, I might like to be like him.
3. My normative sensitivities were offended. I object to Freesmith's avaricious power tendencies.
4. The political is more complex than Burdick would have us believe. Recognizing the devil in Mike Freesmith only makes us less wary of cuddling up to him in the guise of some of our contemporary political figures.
5. Mike Freesmith seemed too one-sided to be believable. I never really felt that I got to "know" him. He seemed more like a type than a real person. Nevertheless, I found him strangely compelling.
6. My reaction to Mike Freesmith was a mixture of ambivalence and disappointment. At times I found myself actually liking him: He reminded me of persons I have known and, in an uncanny way, to whom I have felt attracted.
7. Ironically, Freesmith created a very peculiar world of his own in which he alone was living. In a sense, he was more alienated than those whose alienation he capitalized on.
8. On the surface, Burdick's thesis has a plausibility I accept. Indeed, empirical evidence indicates that fear arousal is one way of achieving significant degrees of attitude change.

As statements such as these were being collected, certain natural categories, or themes, began to suggest themselves. The expectation was fulfilled that the novel would appeal to some readers, as statements 1 and 2 attest; while others would react negatively, as expressed in statements 3 and 4. Statements 5 and 6, however, indicate that some readers experienced ambivalence, and whereas the students were instructed to present their feelings, statements 7 and 8 indicate that some readers could not avoid being analytic about matters, dissecting the plot rather than experiencing it. It was further noticed that whereas some statements referred directly to the novel (e.g., Nos. 1, 3, 5, and 7), others appeared to include reference to the reader's own world of experience (Nos. 2, 4, 6, and 8) as if the reading of the novel provided an opportunity to reflect on the past or to rehearse for the future.

These possibilities were represented theoretically in the balanced design in Table 5, with

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[38] *The Ninth Wave* (New York: Dell, 1975). Burdick, of course, is not in the same category with Orwell or Golding, but his novel, according to Blotner, "is a competent one in the tradition of tough but intelligent realism. The principal defect is that the story is too episodic, that the events of Freesmith's career give the effect of being spread out so that they fail to build sufficiently to the strong climax which the novel needs." See Joseph Blotner, *The Modern American Political Novel, 1900–1960* (Austin: University of Texas Press, 1966), p. 86.

(A)(B) = (4)(2) = 8 combinations; virtually all of the statements in the population fit easily into one of these categories. Thus, statement 1 above is of the type *ae*, statement 2 of the type *af*, and so on for the other six combinations. This scheme then made it a simple matter to compose a balanced sample of statements under the rules of replication in experimental design.[39] In this instance, $m = 7$ replications were made – i.e., 7 statements of each combination were chosen – providing a representative sample of mAB = 56 statements.

Table 5. Factorial Design of Statement Content

Main Effects	Levels	
A. Reaction	(*a*) favorable (*c*) ambivalent	(*b*) critical (*d*) analytical
B. Reference	(*e*) the novel	(*f*) the reader

The statements were then randomly numbered (from 1 to 56) and, prior to any class discussion of the novel, were given to the students who were asked to provide a *Q*-sort of the items, from those statements "most like my viewpoint" (+5) to those "most unlike my viewpoint" (–5). Each *Q*-sort in this case served as a kind of second essay, each retaining essentially the same degree of self-referentiality as was involved in the essays. The *Q*-sorts so provided were then correlated, factor analyzed, and rotated by varimax criteria to a position approximating simple structure. The results indicated that although the statements were separately ranked by the nineteen students, each from his or her own subjective point of view, only three distinctly different viewpoints were expressed as represented by factors X, Y, and Z.

Factor X: The Normativists. The factor X viewpoint was defined by the responses of six subjects, all of whom were political science graduates. The statement given the highest score (+5) by this group reads: "My normative sensitivities were offended; I object to Freesmith's avaricious power tendencies" – hence the normative characterization. These students generally reacted negatively to their reading, as indicated by some of the other statements which received the highest scores (+5, +4) in factor X:

> Under the best of circumstances, Burdick's narrative is an oversimplified reflection of reality. Could any political opposition be as naive as the one which faced Mike Freesmith? It is possible, but doubtful. . . . Fear and hate are, and always have been, prime components of political power. But to imply that the only possible defense against them is the murder of their most proficient practitioners, as Mike was, is to idealize the negative – i.e., to postulate a world in which non-evil is equated with good. . . . Although I believe Freesmith's formula for power is valid and could be used, I still believe that man can be more rational. I for one have not been moved to emotion or action by any speaker, and I can't help but think there must be more people who feel the same way.

Burdick's presentation is therefore not only regarded as normatively offensive, but as a shallow reflection of political reality that overlooks man's more rational capabilities.

Factor Y: The Identifiers. A glimpse can be gotten of the factor Y response, given pure expression by five readers, through examination of some of those statements attaining the highest positive factor scores, as follows:

> I found myself arguing against Freesmith's insights simply because I do not like to conceive of myself as being that susceptible to manipulation. I felt a conflict between what I "feel in my heart" and what I "know in my head" . . . There is a lesson to be learned, to the effect that one must be careful and reflective in politics – he must be aware of men like Freesmith, and carefully weigh decisions. Only then is control not possible. . . . While reading, I felt I might like to try some of Mike's techniques. I admired him because he had such great insight into people, and, in some ways, I might like to be like him. . . . I was left with truly mixed emotions. On the one hand, I think that Mike and his ideas were perfectly normal in the political arena. On the other hand, I have grave doubts as to the truth of the ideas – or perhaps my doubts concern whether these things *should* be true.

The statements indicate that persons associated with factor Y responded to the novel in an essentially positive way, although these readers were apparently ambivalent toward the character of Freesmith. There is a clear identification with the hero – "I admired him and might like to be like him" – but at the same time there is conflict and "mixed emotions" between "what I feel in my heart and what I know in my head." There is evidence also that these students' reading of the novel served the utilitarian function of a vicariously learned experience to be stored up for future use: "There is a lesson to be learned," factor Y subjects say: "One must be careful and . . . aware of men like Freesmith," apparently in order to avoid being controlled by them. It is important in this latter respect to point out that three of the students associated with this factor were employed in political settings, and during class discussion about the novel they frequently referred to the various Mike Freesmiths with whom they had to contend on a daily basis.

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[39] It is in this sense, as noted previously, that *Q*-technique provides an *adjunct* to experimental method. As Nimmo and Savage have pointed out, the fundamental logic of experimentation can be extended into the field by introducing experimental designs, such as in Table 5, into the data-collection procedure. (See Dan Nimmo and Robert L. Savage, "Image Typologies in a Senatorial Campaign: A Comparison of Forced Versus Free Distribution Data," *Political Methodology,* 2 (August, 1975), 293–318.)

Factor Z: The Emotionally Uninvolved. Readers whose reactions to the novel are represented by factor Z did not sort the statements in a way explainable in terms of the structure in Table 5, i.e., an analysis of variance of the factor scores showed that these subjects were not significantly more favorable, critical, ambivalent, or analytical in their responses, nor did they refer more to the novel than to their own experiences. Nevertheless, the factor represents *some* kind of viewpoint, however unanticipated, a provisional understanding of which can be gained by examining some of the statements receiving high positive factor scores:

> Mike's campaign approach didn't really bother me too much. The politics of getting elected – divisive, competitive, negative – is quite different from the politics of governing which, by its nature, is programmatic, collaborative, and positive. Mike was simply playing his role as a campaigner. . . . It is an interesting novel, and even somewhat absorbing, but only on a level a bit above a conventional detective story. It almost seemed the sort of novel a political scientist might write – subordinating the artistic to the didactic. . . . Under the best of circumstances, Burdick's narrative is an oversimplified reflection of reality. Could any political opposition be as naive as the one which faced Mike Freesmith? It is possible, but doubtful. . . . I felt caught between accepting the essential validity of Freesmith's thesis, yet demanding a recognition by him that people can be rational and aware of attempts at manipulation.

As can be seen, subjects comprising factor Z share the factor X concern that Burdick has not given adequate attention to man's rational capabilities, but they do not see anything to get upset about. They experience neither dislike nor puzzlement, and whereas they do not regard the novel as normatively offensive, neither do they experience a sense of absorption, of "falling into" the novel. They simply consider Burdick to have presented an abstract role type in a novel barely more interesting than a detective story. The response, in short, is one of detachment and lack of emotional involvement.

After reading Burdick's novel, but before being informed of the factor results, the members of the seminar engaged in an animated discussion during which the members of the various factor groupings clashed on issues brought to light in the factor scores: The factor X subjects displayed indignation over the novel, the factor Y readers repeatedly demonstrated their ambivalence toward it, and those on the positive pole of Z remained detached. In short, the behavior of the seminar members was in most respects homologous with the relationships captured in the factor results. This is mentioned since questions are frequently raised concerning the connection between attitudes and behavior, or between "natural" behavior and that which occurs under more controlled conditions. In this respect, Burdick's novel appears to have served as a catalyst, inducing behaviors which these individuals had a prior readiness to express, and which were rendered explicit in the *Q*-sortings. The factor results, in turn, served as a kind of microscope, bringing into focus the structural relationships that might otherwise have gone unnoticed by the unaided eye.

Experimental Analysis of a Single Case. Considerable attention has recently been given to the intensive analysis of single cases as providing opportunities for expanding our knowledge of social and psychological processes, and the scattered applications to literary effect suggest promising results will be forthcoming from this observational standpoint.[40]

An intensive analysis normally seeks to probe beneath the manifest response in an effort to examine the thoughts and feelings which accompany the overt behavior. For example, in the study above, one of the subjects, who will be referred to as M, read

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[40] The most sustained attention to the single case in literature has been by Norman N. Holland, *5 Readers Reading* (New Haven, Conn.: Yale University Press, 1975); and Holland, *Poems in Persons* (New York: W. W. Norton, 1973). An earlier effort is by Robert N. Wilson, "Literary Experience and Personality," *Journal of Aesthetics and Art Criticism*, 15 (September, 1956), 47–57. On the theory of single-case design, see Steven R. Brown, "Intensive Analysis in Political Research," *Political Methodology*, 1 (Winter, 1974), 1–25; and Larry R. Baas and Steven R. Brown, "Generating Rules for Intensive Analysis: The Study of Transformations," *Psychiatry*, 36 (May, 1973), 172–183.

Burdick's novel and, in her Q-sort, gave prominence to the following reactions:

> You could almost feel the tension as Mike began pulling the strings to get Cromwell elected. The maneuvering, scheming, and behind-the-scenes activities were exciting. (score +5)
>
> The wheeling and dealing underlying the political campaign, and the theory of human nature held by Mike Freesmith were, for me, fascinating. (score +5)

What is the source of excitement and fascination? Why does M become absorbed in the novel whereas other readers do not?

According to Holland, literary characters are abstractions from the mind of the reader that are projected into the novel: "The plot or incidents cause me to have certain feelings or wishes or tensions. I feel these tensions from the play as tensions in myself, but, both intellectually and emotionally, I attribute these tensions to the characters as motives: I project or bestow my feelings on the characters."[41] The task of the experimentalist is to probe these possibilities, controlling conditions so as to bring such dynamics into view for more detailed examination.

In pursuit of these implications, M was presented with 50 descriptive adjectives (such as "conscientious," "suspicious," "ambitious," etc.) and instructed to describe various of the characters in Burdick's novel – e.g., by ranking the adjectives from "most like Mike Freesmith" (+4) to "most unlike Mike Freesmith" (–4), thereby producing a word picture of how the leading character appeared to her. In addition, she gave Q-sort descriptions of various aspects of herself, as well as of persons from her real-life experiences. The Q-sorts, once provided, were correlated and factor analyzed. Three factors resulted, two of which are shown in Table 6.

The most striking feature of the results is the close identification on factor M1 between Mike Freesmith, the leading figure of Burdick's novel, and M's idealized father, both of whom she described as consistent, rational, intellectual, and realistic. Her own self-ideal was also highly correlated with this factor. Bipolar on the same factor were her images of the psychologists in the novel, the Democratic Convention, and Miss Bell, Freesmith's high school teacher, all of whom were controlled and manipulated by Freesmith, and who were described by M as obedient, old-fashioned, and timid. Her image of her "father at his worst" was also associated with this group of figures from the novel.

Factor M2 is dominated by M's mother whose relationship to M is strained, as is

Table 6. Factor Structure for M

	Factors	
Conditions of Instruction	M1	M2
Mike Freesmith	(82)	25
M's father	(52)	–23
Cromwell, candidate managed by Freesmith	(45)	10
M's self-feelings upon completing novel	33*	02
M's father "at his worst"	(–79)	–05
The psychologists, outwitted by Freesmith	(–58)	17
Miss Bell, teacher seduced by Freesmith	(–58)	–15
M's mother "at her worst"	–12	(82)
M's mother	–08	(65)
California politics, as portrayed by Burdick	–26	(51)
The drunk Freesmith drove to suicide	09	35*
M's self	10	32*
Hank, Freesmith's closest friend	24	31*
M's ideal self	(66)	(–46)
Person in M's life whom she hated most	(–44)	(63)
The Convention, manipulated by Freesmith	(–57)	33*

Factor loadings in parentheses significant ($p < .01$), $*p < .05$.

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[41] Norman N. Holland, *The Dynamics of Literary Response* (New York: Oxford University Press, 1968), p. 274. The same principle underlies Sartre's idea of *engagement:* "... the literary object has no other substance than the reader's subjectivity; Raskolnikov's waiting is *my* waiting which I lend him. Without this impatience of the reader he would remain only a collection of signs. His hatred of the police magistrate who questions him is my hatred which has been solicited and wheedled out of me by signs, and the police magistrate himself would not exist without the hatred I have for him via Raskolnikov. That is what animates him, it is his very flesh." See Sartre, *What is Literature?*, p. 31.

evidenced in the degree of similarity between M's image of her mother as she usually is and at her worst. This factor also reflects M's conception of California politics as portrayed by Burdick, and to a lesser extent her own self-concept as well as the image of the drunk who Freesmith drove to suicide. This cluster of images is characterized in the factor scores as frustrated, domineering, self-centered, and intolerant.

The third factor was relatively minor in a theoretical as opposed to a statistical sense, being composed primarily of M's images of persons in the novel whose characters Burdick did not fully elaborate.[42]

But in terms of factors M1 and M2, the stage is set for a rich drama between characters in the novel (Mike Freesmith vs. the Democratic Convention and the drunk) who are symbolically related to important objects in M's own life space (father vs. "father at his worst" and mother). We are therefore closer to understanding the sources of M's excitement and absorption in the novel as expressed, for example, in the following statement (from the Burdick *Q* sample):

> While reading, I felt I might like to try some of Mike's techniques. I admired him because he had such great insight into people, and, in some ways, I might like to be like him. (score +4)

M strongly identified with her image of Freesmith which, in turn, was highly related to her own self-ideal and to the idealized image of her father. As the novel unfolded, therefore, it is not unlikely that Freesmith's enemies became M's, and that his successes became hers as well. In a sense, the structure of the novel provided an occasion for M to relive the fantasied triumph of the good figures in her world over the bad, and this may have contributed to the enjoyment she experienced in the course of her reading. As Holland has remarked in this regard, "it is from such deep and fearful roots of our most personal experience that literature gets its power and drive."[43]

We can also speculate that M's enthusiasm for the novel was reinforced in another way: As Table 6 shows, her self-description was originally associated with the more aggressive factor M2 (characterized by such traits as frustrated, self-centered, intolerant, and domineering), indicating M to have had a somewhat low estimate of herself initially; upon completing the novel, however, her feelings about herself had shifted by a significant amount to factor M1 (characterized as rational, intellectual, realistic, etc.). Although this effect may have had multiple determinants, one tentative hypothesis worth pursuing would be that M, however temporarily, came to value herself more as a by-product of having read the novel – i.e., through identifications and counteridentifications with the various fictional characters, she was exposed to positive experiences in fantasy that vicariously enhanced her self-estimate. Psychodynamic interpretations such as this have often been regarded as specious and contrived, and the evidence in this particular case is obviously far from conclusive. Nevertheless, the procedures outlined show the ways in which such outrageous hypotheses can be given operational substance, and provide opportunities for examining phenomena that might otherwise be ignored altogether.

Expanding the Experimental Perimeter

The studies reported here have not focused on the intent of the writer, but on the response of the reader as he or she grapples dialectically with the signs produced, with whatever intent, by the writer. By giving prominence to the subjective response of the reader, I have endeavored to indicate ways in which contemporary social science can complement the recent resurgence of interest in the connections between social life and literature. This renewed interest covers not only contemporary literature, but also directs attention to our political-literary past and to the political thought of prominent literary figures. In addition, there is the vast outpouring of novels which, like Burdick's, deal with political themes.[44]

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[42] In this connection, it is important to recall William James's distinction between what is *me* as opposed to what is merely *mine*. M is deeply implicated in factors M1 and M2 in that the images of her self, her ideal self, and relevant others are significantly interrelated. Factor M3, on the other hand, is composed of images that she entertains without apparent self-involvement: They are *hers*, but are not *her* in a more fundamental sense. See William James, *The Principles of Psychology* (New York: Dover, 1950), I, 291.

[43] Holland, *Dynamics of Literary Response*, p. 30. This dynamic is given more extended treatment by Holland, "Unity Identity Text Self," *PMLA*, 90 (October, 1975), 813–822. See also David Bleich, "Emotional Origins of Literary Meaning," *College English*, 31 (October, 1969), 30–40.

[44] Relevant works in this area are literally too numerous to mention. In addition to the forementioned, representative titles of direct interest to political science include Myles L. Clowers and Lorin Letendre, *Understanding American Politics Through Fiction* (New York: McGraw-Hill, 1973); Thomas R. Edwards, *Imagination and Power: A Study of Poetry*

The term "subjective response of the reader" must be clarified, however, so as to be useful to a developing experimental science. Subjectivity is best conceived in purely empirical terms as a person's own point of view, i.e., in terms of what an individual says about something, whether a political novel, a poem, a political theory, or whatever. It is subjective because it is *his* viewpoint, and, as such, not verifiable in terms of external norms or subject to proof or refutation as normally conceived. Despite its subjective nature, however, a person's viewpoint has one very important characteristic – namely, it is behavior and is consequently suitable material for behavioral analysis.

From this vantage-point, it may be profitable to think in terms of a person "having an interpretation,"[45] i.e., of verbally expressing certain viewpoints about Golding's *Lord of the Flies*, Mazlish's *In Search of Nixon*, and Burdick's *The Ninth Wave*, or about any other writings. And as indicated in the single-case study of M, it is quite possible to examine a person's history in order better to understand the ways in which his or her previous experiences have led to specifiable reactions to certain aspects of a story, or, more in line with Skinner, how certain literary productions preselect certain responses from the reader.

It would be a grave error if a developing experimental analysis of politics were to restrict itself to the study of "objective" behavioral reactions, by which is usually meant those features of behavior examined from the external frame of reference of the observer.[46] In this paper, I have purposely employed a highly subjective phenomenon, literary interpretation, to show that subjectivity need not be ruled out as a suitable subject matter for scientific regard merely because there are no external group norms and controls upon which to base measurement. With respect to subjectivity, each person can serve as his own control, with all measurement ultimately centering on the individual's own universe of existence, on his own operant subjectivity. Indeed, if the political world, like beauty, is in the mind of the beholder, then the subjective standpoint is apt to be the most productive in the long run, in the social sciences as well as the humanities.

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on Public Themes (New York: Oxford University Press, 1971); Martin H. Greenberg and Patricia S. Warrick, eds., *Political Science Fiction* (Englewood Cliffs, N.J.: Prentice-Hall, 1974); Henry M. Holland, Jr., ed., *Politics Through Literature* (Englewood Cliffs, N.J.: Prentice-Hall, 1968); Irving Howe, *Politics and the Novel* (Cleveland: World, 1957); John Lucas, ed., *Literature and Politics in the Nineteenth Century* (London: Methuen, 1971); V. J. Scattergood, *Politics and Poetry in the Fifteenth Century* (London: Blanford Press, 1971); and Robert Penn Warren, *Democracy and Poetry* (Cambridge, Mass.: Harvard University Press, 1975). On the political thought of writers, see David P. Calleo, *Coleridge and the Idea of the Modern State* (New Haven, Conn.: Yale University Press, 1966); Avrom Fleishman, *Conrad's Politics: Community and Anarchy in the Fiction of Joseph Conrad* (Baltimore: The Johns Hopkins Press, 1967); and Alan Swingewood, *The Novel and Revolution* (New York: Barnes and Noble, 1975). The upsurge of interest in literature in related disciplines is exemplified by Lewis A. Coser, ed., *Sociology Through Literature*, 2nd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1972); Ronald Fernandez, ed., *Social Psychology Through Literature* (New York: Wiley, 1972); and John W. Milstead, Martin H. Greenberg, Joseph D. Olander, and Patricia Warrick, eds., *Sociology Through Science Fiction* (New York: St. Martin's Press, 1974). As a methodological sidelight, an additional application of *Q* technique to literary studies, in addition to Stephenson (see note 10), is to be found in Lawrence Kohlberg "Psychological Analysis and Literary Form: A Study of the Doubles in Dostoevsky," *Daedalus*, 92 (Spring, 1963), 345–362.

[45] B. F. Skinner, "A Lecture on 'Having' a Poem," in *Cumulative Record*, 3rd ed. (New York: Appleton-Century-Crofts, 1972), pp. 345–355.

[46] Steven R. Brown and Richard W. Taylor, "Frames of Reference and the Observation of Behavior," *Social Science Quarterly*, 54 (June, 1973), 29–40.

COMMUNICATIONS

The Nationalization of State Policy

TO THE EDITOR:

In an earlier piece in this Review, Douglas Rose has argued that comparative state analyses which are confined to just the state level are not useful or meaningful because states do not act as policy systems.[1] In addition, he argues that such analyses are of little value because state level policy does not differ much since policy has become nationalized and similar.[2] His work represents a potentially general critique of subnational unit analyses, and a possible set of techniques that might be applied to other subnational units of analysis. Given Rose's conclusions, his original work deserves some careful scrutiny. In addition, his work has drawn some critical reactions, which also deserve consideration.[3]

The point of this comment is that both Rose and his critics are arguing about techniques and evidence which are only weakly related to his stated theoretical problem. The issue of how the variance of a single indicator might be calculated, decomposed, and interpreted does not deal with whether particular areal units act as policy systems, and barely touches the complex issue of "sameness" of policy and its causes. These points will be elaborated upon, but the important matter is that Rose's analytics do not deal with his theoretical question. His empirical work and technical arguments are largely irrelevant because he has not formulated specific hypotheses, presented relevant evidence, or dealt with the necessity of linking evidence to hypotheses. His failure to execute this basic aspect of research results in his relying on irrelevant evidence, and leaves us with a series of unsubstantiated assertions.

Rose's stated theses are: (1) that states in a multi-level policy system do not constitute closed policy systems (p. 1162) and (2) that the predominant factor in state policy determination is national policy norms (p. 1169) which creates high policy similarity among states. As I interpret his piece, his focus is therefore: (1) the extent of "systemness" (the degree to which open/closed systems exist) of the areal units of states, and (2) the extent of nationalization of state policy. These concerns can be stated formally. Let Y_i = state policy (e.g., type of tax system, presence or absence of consumer protection legislation); X_1 --- X_n = state factors (degree of political competition, urban representation); and X_{n+1} --- X_{n+k} = external or other level factors (federal legislation, degree of enforcement of federal laws). Using systemness in the sense of some degree of covariation among the bounded set Y_i, X_1 --- X_n, the question of state systemness has to do with whether $Y_i = f(X_1$ --- $X_n)$, $Y_i = f(X_{n+1}$ --- $X_{n+k})$, or Y_i = f (some combination of X_1 --- X_n, X_{n+1} --- X_{n+k}). To the extent that $Y_i = f(X_1$ --- $X_n)$, states are closed, autonomous policy systems. To the extent that $Y_1 = f(X_{n+1}$ --- $X_{n+k})$, states are open policy systems.

The second problem deals with the extent of nationalization of state policy. What this refers to can also be formally stated, though there is some ambiguity because of the various ways in which the term nationalization has been used. The general potential policy relationships of relevance can be expressed as:

$$Y_i = a + b_1X_1 + --- + b_nX_n + b_{n+1}X_{N+1} + --- + b_{n+k}X_{n+k}$$

Nationalization of policy has been used in the sense of declining subnational unit autonomy and/or as meaning things are becoming homogenized, standardized, or "the same." In terms of the previous equation, declining autonomy can be represented as the increasing relative importance of X_{n+1} --- X_{n+k} as determinants of Y_i.[4] Standardization can refer, at a minimum, to the tendency toward equivalence across all units of analysis of: (a) Y_i values; (b) b_1 --- b_n; (c) b_{n+1} --- b_{n+k}; (d) the distribution of X_1 --- X_n; (e) the distribution of X_{n+1} --- X_{n+k}; or (f) some combination of these.

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[1] Douglas D. Rose, "National and Local Forces in State Politics: The Implications of Multi-Level Policy Analysis," *American Political Science Review*, Vol. 67, No. 4 (December, 1973), p. 1162.

[2] *Ibid.*, p. 1168.

[3] William Lyons and David R. Morgan, "Multi-Level Analysis in State Politics," *American Political Science Review*, 70, No. 2 (March, 1976), 159–163; John Wanat and Phillip W. Roeder, "The Relative Impact of State and National Forces," *Ibid.*, 70, No. 2 (March, 1976), 163–166; Douglas D. Rose, "State Public Policy and Variance Analysis: A Reply to Wanat and Roeder and Lyons and Morgan," *Ibid.*, 70, No. 2 (March, 1976), 166–174; and Douglas D. Rose, "The States' Role in Public Policy: A Clarification," *Ibid.*, 70, No. 3 (September, 1976), 926–929.

[4] There are also likely to be interactive relationships between X_1 --- X_n and X_{n+1} --- X_{n+k}, but for purposes of simplicity, only the additive model of relationships will be discussed here.

Rose outlines in a general way that these two problems are his focus. He then argues that states do not constitute systems, and that nationalization, in the sense of low state autonomy and "similarity," predominates. The essential point to be made here is that, while these statements might be true,[5] the evidence which is presented is not relevant for assessing the statements. To examine state systemness Rose would have to determine the relative significance of X_1 --- X_n as determinants of Y_i as compared to the relative significance of X_{n+1} --- X_{n+k} as determinants, either cross-sectionally or longitudinally. He does not present such evidence. When speaking of nationalization, he does not specify which meaning he is using. Regardless, the evidence he does present is only for the notion of nationalization defined as (a) above, and the data he uses are aggregated individual attributes, which seem to have very little relationship to state policy. No evidence is presented on the other types of nationalization.

In general, the logic of analysis and the evidence offered are not related to the stated theoretical focus. The evidence involves an odd assortment of per capita measures analyzed by an odd analysis of variance technique. Though some comments are in order on what empirical analysis he has done, the essential point here is that the exchange over the multiple means of calculating analysis of variance, while interesting and perceptive, skips over the prior questions of what is a relevant test, and what is appropriate evidence.

The analysis is inappropriate and misplaced, and suffers from several specific flaws. First, the indicators used appear to only dubiously represent state policy; states do differ a great deal on some policies, such as aid to local governments, or the distributional mix of their tax systems, and these policy differences are not examined. Second, one of Rose's main points appears to be that individual-level data aggregated to the state level show little variation, so that, in a case of guilt by association, units of that level of analysis become undifferentiated, and comparative state analyses become meaningless.[6] It is rather that *individual-level indicators aggregated to that level* are of little value.[7] Rose has confused the utility of aggregating certain indicators to higher "levels" with the utility of higher levels of analysis. Third – and this is particularly important – in using variance components analyses, equivalence or nonequivalence of group means, or finding low within or between group variances should be interpreted with great caution. Any result could be explained by multiple combinations of (b) through (e), as discussed above, and only multivariate analyses will tell what similarities or dissimilarities of (b) through (e) are associated with patterns of variations in Y_i. The point is that the behavior of Y_i cannot be used with any certainty to infer patterns of (b) through (e), which is what Rose seems to be doing.[8] Fourth, the evaluation of the significance of within-state variations is confusing. Rose regards them as the important policy analysis focus, but he seems to do his empirical analysis as if within state distributions of benefits are not associated with state action. That is, in decomposing variance, he regards the between state variance as reflective of state impact, and the within state variance becomes "other" or "individual level," apparently regarded as not being associated with state policy. But if states differ in their efforts toward equalization of benefits, then differences in within state distributions may certainly be due to state policy. For example, in an ANOVA of five states with cities as the unit of analysis, and per capita intergovernmental aid as the variable, differences in within state aid distributions (or violations of the equal standard deviations assumptions) may reflect state policy differences; therefore it may be incorrect to regard only between-group variances as reflecting state differences.

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[5] I have considerable sympathy for the statements, and have argued for the importance of their examination in Jeff Stonecash, "Urban Policy Analysis, Systems Assumptions, and Multiple Levels of Influence," *Publius*, Vol. 7, No. 1 (Winter, 1977).

[6] I am indebted to Heywood Sanders, The Institute of Government and Public Affairs, The University of Illinois, for contributing to my understanding of the statistical analysis in Rose's work.

[7] In regard to this point, and the first, it may be that I am in error in criticizing his analysis for it may be that he is purposefully doing this to demonstrate that prior analyses taking this approach using such aggregated individual level data are incorrect. While that is possible (see p. 1166), it does not seem to be the case on the basis of the data and analysis used on pp. 1164–1165. I am unable to determine what is finally meant regarding the use of such data.

[8] In a personal communication with me, Doug Rose has indicated that he has gone beyond the limitations of variance components analyses and examined structural relationships in Douglas D. Rose, "Citizen Preference and Public-Policy in the American States," in *Perspectives on Public Policy-Making*, William B. Gwyn and George C. Edwards III (eds.), Tulane Studies in Political Science, Vol. XV (New Orleans: Tulane University, 1975).

The result of all this is that Rose, while producing a piece with some very interesting ideas, and some intuitively appealing statements (p. 1171) in a research area which is highly complex, has not examined, or supported his basic hypotheses. We still need sophisticated analyses to determine state systemness and the nature and extent of nationalization.

JEFF STONECASH

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Party Leadership Change in the House of Representatives: 1910–1976

TO THE EDITOR:

One of the problems faced by the profession today is the frequency with which calls for further research go unanswered. This problem, coupled with a hesitancy to report negative or inconclusive findings, creates the possibility that a number of questions of importance may not be answered. The purpose of this note is to answer a call for research by Robert Peabody on a question of party leadership selection. It also coincidentally reports a negative finding.

The impact of elected congressional leaders is generally recognized by political scientists.[1] However efforts to study leadership change in the House have been constrained by the extreme stability of the elected leadership. Peabody has suggested a number of interesting factors for explaining periods of instability in congressional leadership selection during the period 1955–1966.[2]

Professor Peabody has called for research over a broader period of time in order to explore these factors in greater detail.[3] The findings which will concern us here are those which relate to the Republican minority of 1955–1966.[4]

For the purposes of this analysis Peabody's concept of "revolt" will be combined with his next most violent category of leadership change, challenge to the heir apparent to form the category "deviant election." Since there is a well-established pattern of succession in which the majority or minority leader assumes the office of Speaker when it becomes vacant because of the retirement of the incumbent or a change in electoral fortunes which does not apply to any other office over time, only the offices of Speaker and majority and minority leader will be considered. A Speaker will be expected to become minority leader if his party loses the majority. If the Speaker is not re-elected the majority leader will be expected to fill the spot. Only successful revolts and challenges will be considered.

Using the above criteria one finds only four deviant elections during the period 1911–1975, all occurring in the Republican Party.[5] Although the low number of cases makes it impossible to establish a statistical correlation between the suggested independent variables and deviant elections, there is an approach which may give a rough estimate of the importance of the variables. Instead of emphasizing conditions in the rare deviant elections, one should examine the variation in the factors during the entire period. We can hypothesize that the greater the amount of fluctuation in a variable during periods of leadership stability, the less the effect of the variable on instability.

I have examined two of the factors Peabody suggests may affect leadership stability, minority or majority status and the gain or loss of seats, from the 1910 election to the present.[6] Peabody's first conclusion regarding the Republican minority of 1955–1966 is that it is more prone to intraparty leadership change through contests. A look at the data from 1910 to 1976 suggests that this statement may be expanded into the generalization that *the Republican party is more prone to leadership change through contests regardless of minority majority status.* There were four Republican deviant elections compared to none for the Democrats. The deviant election of 1919 occurred with a Republican majority of 55 per cent. The Republicans were barely in the minority in 1931 with slightly more than 49 per cent of the House. More importantly, there are no deviations for the Democratic party during the period when it is in the minority.

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[1] See Robert L. Peabody, "Party Leadership Change in the United States House of Representatives," *American Political Science Review*, 61 (September 1967), 675–693; Charles O. Jones, Joseph G. Cannon and Howard W. Smith: An Essay on the Limits of Leadership in the House of Representatives," *Journal of Politics*, 30 (August 1968), 617–646; Barbara Hinckley, "Congressional Leadership Selection and Support," *Journal of Politics*, 32 (May 1970), 268–287; and Randall B. Ripley, *Party Leaders in the House of Representatives* (Washington: The Brookings Institution, 1967).

[2] Peabody, "Party Leadership Change," p. 686.

[3] Peabody also deals almost exclusively with the post-1955 period in his recent book, *Leadership in Congress* (Boston: Little, Brown and Company, 1976).

[4] Peabody, "Party Leadership Change," p. 693.

[5] Gillett over Mann, 1919: Snell over Tilson, 1931; Halleck over Martin, 1959; and Ford over Halleck in 1965.

[6] The source of figures up to 1970 is *Historical Statistics of the United States Colonial Times to 1970* (Washington: U.S. Government Printing Office, 1975). After 1970 the Congressional Directory is used.

Peabody's second finding is, "the longer the period of minority status, the more prone the minority party is to leadership change through revolt."[7] (It should be noted that in this study the concept of revolt has been expanded to include breaks in the pattern of succession.) Analysis of the deviating elections over time suggests that this finding does not hold. Although the two deviations which occurred in the period examined by Peabody did follow a prolonged period of minority status, this is not true for the two earlier cases. The Republicans were in the majority during the 1919 election. True, they were in the minority for the previous eight years, but they made significant gains in 1914 and 1916 before winning the majority in 1918. Republicans controlled the House from 1895 to 1911. The deviation of 1919 was not caused by prolonged minority status. The deviation of 1931 occurred with the Republicans barely in the minority after twelve years in the majority. Since the Republican party has been in the minority for the period since the 1965 deviation, one would have expected the deviations to have occurred with even greater frequency from 1966 to 1975. Such was not the case. *While long-term minority status may contribute to instability, it does not appear to be a major factor.* This finding is given additional support by the fact that the Democrats withstood twelve years in the minority (1919–1931) without a deviating election.

Peabody's third finding was that "revolts are most likely to occur following congressional election disasters," defined as the loss of thirty or more seats. Only in 1919 did a deviation occur after a gain of seats. The Republicans dropped from 267 seats in 1929 to 214 in 1931, from 200 in 1957 to 153 in 1959, and from 177 in 1963 to 140 in 1965. While five election disasters occurred during the period from 1911 to 1975 without a deviation, and one deviation without a disaster, the occurrence of an event as rare as a deviation during three out of eight disasters strongly suggests that *this factor has a major effect on leadership stability for Republicans in the House.* An examination of the Democratic party, however, reveals six disasters with no deviating elections.[8]

In conclusion, the data suggest that the Republican party in the House is more likely to change its leaders through contests than is the Democratic party. This tendency does not appear to be linked to the party's current minority status. The occurrence of a disaster in the general election strongly increases the probability of a deviation in the Republican party. This factor, however, does nothing to explain the instability of the Republican party relative to the Democratic party since the Democrats faced a comparable number of election disasters without a deviation. It appears that the most fruitful direction for further research into the stability of elected leaders would be an examination of the differences in structure and internal characteristics of the congressional parties themselves.

VAUGHN ALTEMUS

University of Wisconsin, Madison

The Changing Shape of the Electoral Universe: Education and "Issue-Voting"

TO THE EDITOR:

While I do not wish to become directly embroiled in the merits of the entire Burnham v. Converse-Rusk debate (symposium on "Political Change in America," *APSR*, 68 (September 1974), 1002–1057), I am sure that all the participants to that debate would like to see as sound an empirical base for argumentation as is possible. I was therefore interested to note that Rusk's objection to Burnham's assumption of extensive pre–1896 issue voting:

> does not derive from a historical analysis alone. Current survey information shows that education is positively correlated with issue salience. If one can extrapolate this notion to time past, then it is difficult to explain how the earlier American electorate could have been so thoroughly issue conscious in the virtual absence of much formal education (Rusk, "Comment," p. 1034).

Although several objections to this statement may be raised, I would like simply to note that current survey information does *not invariably* show education to be positively correlated with issue salience.

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[7] Peabody, "Party Leadership Change," p. 693.

[8] The Republicans suffered election disasters in 1912, '22, '30, '32, '48, '58, '64 and '74. The Democrats in 1914, '20, '38, '42, '46, and '66.

Granted, the positive relationship noted by Rusk is the usual one. A while back, for example, I postulated a causal model of voting defection, in which the decision to defect from one's standing party attachment is a function of issue- and candidate-based dissonance from standing party attachment with strength of attachment controlled. Applying the model to 1968 Survey Research Center (SRC) presidential election data ("Parameters of 'Rational' Voting," *Journal of Politics*, February 1975, 202–234), I indeed found that the path coefficient for the direct link from issue dissonance to voting defection was .25 for respondents with high education, but only .15 for respondents with low education. Likewise, the total (direct and indirect) impact of issues on the voting decision was .51 among high-education respondents but only .36 among low-education respondents (*ibid.*, 220).

Recently, however, I have been applying this model to earlier presidential elections. In doing so, a rather interesting result emerged in connection with the 1960 presidential contest: the usual relationship between formal education and issue voting reversed. Thus, calculating the 1960 path coefficients (and, for comparison, recalculating the 1968 coefficients so that operationalization is identical in both years [see my "Parameters of Responsible Voting: Issues and the American Electorate, 1952–1968," Ph.D. dissertation, The Johns Hopkins University, 1975, chapter 4]), we find the following:

In general, among post–World War II American electorates, higher education seems to increase a voter's ability to link policy evaluations with vote choice. Analysis of SRC survey data from 1960 indicate that this is not always the case, however. It would seem plausible to speculate that powerful issues – presumably the desirability of a Catholic president in the 1960 instance – can at times interact with educational level, party attachment, and voting behavior in a manner that suppresses the usual relationship.

The implications of these data are, of course, obvious: it is at least superficially plausible that an earlier American electorate may have been issue conscious "in the virtual absence of much formal education." Of course, the nature of the issues that become salient to voters with less formal education may differ systematically from the nature of the issues that become salient to voters with more formal education (see, e.g., the discussion of "position" and "style" issues in Berelson et al., *Voting* [Chicago: University of Chicago Press, 1954], 184ff). If so, Burnham's arguments may need further revision. But normative disdain in regard to the "seriousness" or "wisdom" of style issues aside, we are still left with a contemporary, survey-based anomaly in the "usual" relationship.

While on the topic of contemporary anomalies in voting theory, we might also note that Rusk, citing the "usual" contemporary findings of comparatively low interest and turnout among rural populations, argues that:

> The prototype of Burnham's enlightened electorate ... was the rural folk who supposedly were ... bourgeois and modern in outlook.... [Here] Burnham's assumptions of intrinsic interest are most severely tested – since rural residents suffered more from the handicaps of little education and lack of access to communications media than did most other groups in society.... What Burnham is asking us to believe is that an earlier electorate characterized by these same handicaps, only more so, behaved in just the opposite fashion from their contemporary counterparts (Rusk, pp. 1034–5).

While Rusk's observation here strikes me as being on firmer ground than his previous statement, Sidney Tarrow ("The Urban-Rural Cleavage in Political Involvement: The Case of France," *APSR*, 65 [June 1971]) has documented a number of contemporary exceptions to conventional wisdom in this area. Tarrow found rates of electoral participation among

Issue Path Coefficients by Education and Year

Year	Education	*N*	Direct Path From Issue Dissonance to Defection	Total Impact of Issues
1960	Training beyond high school	302	.14	.35
	Failure to complete high school	326	.22	.44
1968	Training beyond high school	417	.37	.56
	Failure to complete high school	277	.17	.37

rural Frenchmen that were equal to or higher than those among urban Frenchmen; indeed, "higher, in some cases, than those with more education" (p. 344). Higher French rural turnout, moreover, could not be attributed to partisan involvement (a probable contradiction to Burnham's analysis, we might note), nor to social conformity. Among the explanations explored by Tarrow, one in particular is potentially relevant to the Burnham-Rusk dispute: rural participation was encouraged by widespread local office-holding. It would seem plausible to me that the late nineteenth-century rural American electorate may also have been characterized by comparatively high local office-holding (although not nearly so high as in 1960s France). I have no data here. But such data should be obtainable, and I would urge the scholars directly involved in this controversy to perform the appropriate analysis.

In conclusion, I want to reaffirm my opening statement: I do not wish to "take sides" in the debate here. I have "picked on" Rusk simply because I felt that I had some relevant information. Rusk's "legal-institutional" argument, of course, remains as persuasive as before. Burnham, Converse, and Rusk have all made a number of good points and, likewise, they have all relied on a number of questionable assumptions. It is to be hoped that further refinements of each position will be forthcoming.

THEODORE F. MACALUSO
Miller and Byrne, Inc., Rockville, Maryland

TO THE EDITOR:

I see two problems with Doctor Macaluso's letter. First, the discovery of a single deviation from the accepted statistical relationship between education and issue salience cannot justify questioning the general validity of that relationship. Second, the use of contemporary French data does not seem relevant or appropriate for questioning certain assumptions about the nineteenth-century American electorate, especially when these assumptions are based on empirical information about the American voter.

Current survey evidence shows that education is positively correlated with issue salience. It also shows this to be the usual relationship between these two variables. These are the observations I made in my September 1974 essay. Doctor Macaluso, however, finds a reversal of this relationship when he analyzes the 1960 election. His data presentation actually shows the relationship of education to issue voting rather than to issue salience, but, for the sake of further discussion, I will assume that the relationship between education and issue salience would also be reversed in 1960 if the appropriate data had been presented.

To oppose the arguments in my 1974 essay, Doctor Macaluso has to assume that his discovery of this single deviant case calls into question the validity of the usual relationship between education and issue salience. And yet this is difficult to do, given the wealth of survey evidence suggesting otherwise. As a result, Doctor Macaluso vacillates between the two positions. At one point, he observes that "the positive relationship noted by Rusk (between education and issue salience) is the usual one," while, at another point, he states that "the implications of my data are, of course, quite obvious: it is at least superficially plausible that an earlier American electorate may have been issue conscious in the virtual absence of much formal education."

Doctor Macaluso would like to believe that his one deviant case not only invalidates the accepted empirical finding between education and issue salience but also proves that the usual relationship between these two variables is negative rather than positive. If this were the empirical situation in contemporary times, he could then extrapolate this negative relationship to earlier historical periods just as I extrapolated the usually expected positive relationship to time past. Extrapolation of any survey finding to time past is, of course, fraught with difficulty. But I feel a scholar is on safer ground extrapolating a finding repeatedly proven in survey research than one which is not. Just as a deviant case should not be used to declare a time-proven survey relationship to be invalid, so should it not be used as an empirical candidate for extrapolation to earlier times.

One final question remains on this subject. Why did Doctor Macaluso find the 1960 case to be deviant in the first place? The social sciences have always stressed "deviant case analysis" – the attempt to explain why deviant cases occur. Often this is very difficult to do, but in the case of the 1960 election, one explanation that comes to mind might fit the situation. One must remember that the relationship between education and issue salience has always been stated and tested only for "position" issues. Probably this is the only proper test of this relationship. Yet 1960 was an election year dominated by a "style" issue – Kennedy's religion. Doctor Macaluso himself admits that the powerful issue in the 1960 campaign was "presumably the desirability of a Catholic President." Macaluso thus finds himself actually testing the relationship between education and issue salience (more appropriately, the relationship between education and issue voting, since the difference in dependent variables here is important) in an election dominated by an issue which might have more easily influenced the voting decisions of less-educated people. The Michigan election analysis shows that two groups of less-educated people were heavily influenced by Kennedy's religion – Catholics in the North and Protestants in the South. Whether style issues, in general, cause deviant cases such as this is uncertain. If my explanation here is correct, perhaps only style issues focusing on controversial social characteristics of the candidates have this effect. The Al Smith candidacy of 1928 might then be another election that would have shown an anomalous finding similar to what Doctor Macaluso observed in 1960.

The relationship between education and issue salience has only been tested with position issues for a reason: Many scholars do not regard style issues as issues. They either have only one side to them or else they predominately emphasize the negative aspect of a potentially two-sided issue. They also require little or no sophistication to grasp – often being emotionally laden moral charges or negative group referents demanding no enduring political interest or involvement from the voter.[1] Knowledge of such "issues" would not characterize an electorate as "enlightened" or "sophisticated." Only a knowledge of position issues would do that. It is in this context that I could not imagine Burnham's pre–1900 voters to be enlightened. They did not have the education necessary to be aware of position issues or to sustain a long-term interest in them. If they were involved in one or more style issues by default, they obviously could not be considered to be the "best" electorate in our nation's history. If "issue salience" is loosely defined as an awareness of and interest in issues in general – including style as well as position issues – one still imagines that the higher a person's educational standing, the more he will be interested in issues. Only in specific campaigns in which style issues predominate might one find a number of less-educated people who deviate from this general relationship, but these occurrences probably do not happen very often and are even rarer in causing a reversal of the usual relationship between education and issue salience.

Doctor Macaluso, in the second part of his letter, questions my characterization of America's early rural electorate. This characterization is largely based on survey evidence about the contemporary rural electorate in America, the relation of education to issue salience, and the effects of mass media on issue salience and issue voting. Of course, there is always some risk in extrapolating these findings to an earlier historical period. I feel, however, that using this data base and making the corresponding extrapolation constitute a more solid foundation than using data from a completely different culture as a basis for characterizing what our early American electorate was like. Contemporary French data would seem to have little relevance for describing the essential features of our nineteenth-century rural electorate.

The French data Doctor Macaluso mentions indicate only that rural electorates are more likely to vote in France than urban electorates in the 1960s period. Such data do not prove that rural residents have a greater interest in French politics than urban residents. They also do not prove that rural French citizens are more aware of national issues and are more likely to cast their votes on the basis of these issues. There is no evidence here to substantiate the claim that the rural French electorate might be "issue conscious" and "enlightened" in the way Burnham thought our early American rural electorate was. The French data Doctor Macaluso uses from Sidney Tarrow's study do not even prove that greater local officeholding of rural French citizens is a reason for their higher voting turnout. No correlation between the two factors is ever presented in the Tarrow study. I take it as a commonplace that local officeholding is more widespread in rural French areas simply because these areas have fewer people than urban areas do. Probably widespread officeholding exists in rural America for the same reason. Yet rural American electorates do not participate in elections to the degree that rural French electorates do. If the two electorates have different voting rates but similar officeholding rates, then it is difficult to see officeholding playing a role cross-nationally as a causal variable. A variable which has little effect in stimulating rural voting turnout in America contemporarily would also not be of much use in explaining the political behavior of this country's rural electorate in earlier historical times.

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[1] There is yet another reason why I do not regard style issues as issues. I believe what is often referred to as a style issue should instead be labelled a candidate, group, or party image. Kennedy's religion, for example, could be seen as a "candidate characteristic" – part of his candidate image to the general public. Its net effect, of course, was negative. Kennedy lost more votes than he gained as a result of his religion, almost costing him the election. (See Philip E. Converse, Angus Campbell, Warren E. Miller, and Donald E. Stokes, "Stability and Change in 1960: A Reinstating Election," *American Political Science Review*, 55 (June 1961), 269–280.) Kennedy's religion could also be perceived as part of a group image, evoking prejudicial feelings against Catholics and other minority groups. It might also be seen as part of the image of the Democratic party in that particular election. All of these images have in common an emotionally charged negativism which often engages the more politically apathetic, less-educated segment of the American electorate.

In summary, I feel that Doctor Macaluso's arguments in no way weaken the points made in my 1974 essay. Contemporary survey evidence paints the type of picture of electorates that I have projected backward in time. I will grant that extrapolation of current data to earlier time periods has certain inherent risks, but this is not an issue between Doctor Macaluso and myself since he also engages in such activity. The difference between us lies in the type of data Professor Macaluso projects backward in time. The data he uses either do not have a strong empirical foundation or else do not seem relevant to the arguments in my 1974 essay. I find it difficult to believe that a statistically deviant case finding or a data finding from a completely different culture will help us in our task of understanding the early American electorate.

JERROLD G. RUSK

University of Arizona

Party and Policy in West German Cities: A German View

TO THE EDITOR:

Although an American political science journal seems a distant forum to argue the influence of political parties on policy in West German cities, Professor Fried's recent article[1] has posed such serious questions to those of us on this side of the Atlantic that it cannot pass without comment. We certainly won't dispute Professor Fried's claim that "data on many aspects of municipal government in Germany have been systematically collected and published for many decades." The question that arises, however, when one wades into that welter of statistical data, is what exactly do all those numbers tell us. Precisely as in the case of the cat in Mark Twain's fable of the hot stove, one must be careful not to extract too much from both a hot experience and a list of numbers.

Before addressing the statistical section of Professor Fried's article, we should first like to comment on the two preliminary themes of his article:

(1) the strong ideological basis of the local political parties, and

(2) the decision-making power of the city council.

Instead of reviewing the actual party programs to ascertain the differences, Professor Fried rests his analysis almost exclusively upon two sources. The one is a speech by a CDU politician (G. Triesch) during an election campaign.[2] The other is the monograph of O. Ziebill that focuses upon local political parties. The problem with Professor Fried's reference to Ziebill, is that the reference describes the political programs during the Weimar period when the parties were in fact strongly ideological. How applicable a reference from that period is to present German local government is questionable. Having later quoted H. Kaack, he should have known that the ideological fixation ("Entideologisierung")[3] of the parties in the Federal Republic has lessened.

The focus upon relative or absolute council majorities as the basis for his "measure" of "party politics on policy" necessarily implies that the city council is the key to the decision-making process. Yet the analyses of German local government from this side of the Atlantic (which Professor Fried leaves out of his consideration) present the city council, regardless of majority, as what it really is – the organ that legitimates the decisions reached through negotiations between the city bureaucracy and various council committees. One need only look at the votes in the city councils to realize this, since approximately 90 per cent are unanimous (which should also say something about those so-called "strong differences between the *Kommunalpolitik* of the CDU/CSU and those of the SPD"[4]).

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[1] Robert C. Fried, "Party and Policy in West German Cities," *American Political Science Review*, 70 (March 1976), 11–24.

[2] Guenther Triesch, "Kommunalpolitik und Parteiarbeit," in *Dokumente zur parteipolitischen Entwicklung in Deutschland seit 1945*, Ossip. K. Flechtheim (ed.), Berlin, 1965, vol. 4, 208–221. At the time, 5 of the 11 states of Germany were preparing for city elections, and Triesch's speed must be analyzed in that campaign context. Flechtheim in his preface warns of depicting the German political parties as varying extremes. His sentence in German reads, "Insofern unterscheiden sich die Parteien nicht allzu sehr voneinander," p. xxii.

[3] Otto Ziebill, *Politische Parteien und kommunale Selbstverwaltung*, Stuttgart 1964. In addition to the inapplicability of a Weimar reference Ziebill himself describes the programs of the parties at the local level as both wanting in context and lacking in precise formulation. To quote the original, "Sehr ergiebig waren also die Forderungen der Parteien für die kommunale Selbstverwaltung nicht," p. 32.

Professor Fried flirts with this concept of bureaucratic power near the conclusion of his article, but he stumbles over his limited "measure" of bureaucratic power – "the rate of expansion in the functionary group." Bureaucratic power extends far beyond the mere desire to increase personnel. One need only recall the analysis of another German urbanologist, Max Weber, who described the breadth of bureaucratic influence without mentioning any notions of numerical aggrandizement.

Yet those empirical indicators that supposedly are the measures of municipal output truly deserve scrutiny. In his rush to use the mass of data that the German Municipal Association has gathered, Professor Fried neglects to tell his American audience that many facets of policy that are municipal in the American context are state or federal functions in Germany. Not only does this major distinction threaten his larger aspiration to compare urban policy at the international level, but it also gravely weakens his singular analysis of German cities. For example, in his grouping "Form of Performance-Activism," three of his six empirical indicators can be summarily excluded since they are beyond the reach of city government.

Outside of a few cities in South Germany, all police functions are undertaken by the state governments. Some 80 per cent of German cities have absolutely no say in determining the percentage of police per residents in the city.[5] Similarly, education lies outside the purview of the cities. The various states determine the number of teachers in the schools. The city merely provides maintenance personnel and buildings.[6] Finally, while the cities must finance welfare payments from their budgets, they have little discretion over the amount to be disbursed. The federal government has established uniform welfare rates, and the cities must conform in their disbursements.[7] The measure of per capita welfare spending thus becomes totally inapplicable; the cities are actually executing national policy in this area.

Similar misunderstandings exist in Professor Fried's second category, "Collectivism." Publicly assisted housing is in large measure supported through state funding. Both private agencies and cities may sponsor public housing, and both may act as landlords, but the financial assistance comes from the state.[8] Actual party differences do arise in this area of policy – not over the question of whether to sponsor public housing, but of how to build it. The SPD has favored the larger complexes, the CDU/CSU smaller, family-sized buildings. These fine but very important differences, needless to say, are not presented in the cited statistics, and therefore totally ignored by Professor Fried.

To understand the area of public hospitals one must first understand the German national health insurance system, and the structure of hospital care. Under the national health insurance, which had its roots in the Bismarck era, the overwhelming majority of patients is provided with low-cost medical and hospital care; the rest – the higher income-group – can choose private medical care. Thus most hospitals are so administered as to accommodate these two types of patients. The overarching element of unity in this structure – in both public and nonpublic hospitals – is this duality of treatment. That means that regardless of the type of the hospital – city, church, state, university, etc. – the access is equal. Public hospitals exist not because of the ideological zeal for public ownership, but because they must supplement the deficiencies in the nonpublic hospitals.

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[4] The tendency to avoid political conflicts in the council has been described as "Gesetz der Grossen Koalition." See R. R. Grauhan, "Der politische Willensbildungsprozess in der Grossstadt," in *"Grossstadt-Politik,"* ed. Rolf-Richard Grauhan, Guetersloh 1972, p. 152. Also Rølf Zoll, *"Wertheim III, Kommunalpolitik und Machtstruktur,"* Munchen 1974, p. 64f., 74.

[5] In the cited year, 1964, among the 75 largest German cities, only 15 had independent police forces. The other 60 were served entirely by State supported and directed police forces, *Statistisches Jahrbuch der Deutschen Gemeinden,* 1964, pp. 420–421. Bavaria and Hesse have local police forces, while Baden-Württemberg allows each community with over 75,000 inhabitants the option of maintaining a local force. See R. R. Grauhan, *Politische Verwaltung,* Frieburg 1970, pp. 184–185. It is not astonishing therefore that in his Table 8 the white-collar SPD cities have such an extraordinarily high percentage of police in contrast to the proletarian SPD or CDU cities: five of the six are situated in Bavaria or Hesse.

[6] In Cologne, the fourth largest German city, school personnel comprised a mere 3.5 per cent of all city bureaucrats, the smallest group in the city administration. There were more *Beamte* and *Angestellte* engaged in street maintenance than in the Cologne schools. *Statistisches Jahrbuch* der Stadt Köln, 1964, pp. 165–166.

[7] Welfare payments are regulated through the *Bundessozialhilfegestzes (BSHG).*

[8] The *Wohnungsbaugesetz (WoBauG)* regulates all public housing in the Federal Republic.

To demonstrate this point, one need only compare the number of residents per hospital bed in Professor Fried's Table 6 of CDU and SPD cities. In the CDU cities there were approximately only 50 inhabitants per bed while there were nearly 85 in the SPD cities.[9] That means that the absence of public hospitals in the CDU cities is not a measure of party ideology as Fried claims but rather a measure of the sufficiency of hospital care.

Finally, the ratio of municipal employees per resident is an equally questionable indicator, since many municipal services extend beyond the city boundaries to the neighboring small communities. Thus some communities may have a larger municipal utility work force, not through party wishes for socialization, but because the service territory is far larger than the city boundaries. Complicating this factor further are the varying forms of organizational structure that municipal utilities take. Many cities organize their utilities as corporations, thereby retaining the majority of stocks, the ownership, and thus control, while removing the need for constant city intervention in the daily operation of the utilities. In these cities the employees are excluded from the list of city employees, making this category extremely questionable for Fried's analysis. In this area of the city personnel Professor Fried obviously overlooked the explicit warning of the statistician who gathered the data, that comparisons among cities should be avoided in this area, because individual city structures and functions differ.[10] Again, at least three of the six of Professor Fried's indicators must be strongly questioned.

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[9] The *Statistisches Jahrbuch Deutscher Gemeinden* (1970) shows the following statistics on the ratio of residents to beds in the selected CDU and SPD cities, p. 142 ff.*

[10] Compare Friedrich Reiff, "Personal der staedtischen Verwaltungen," in *Statistisches Jahrbuch Deutscher Gemeinden,* 1964, p. 402: "Vergleiche koennen nicht allgemein, sondern nur auf individueller Grundlage, d.h. unter Beruecksichtigung der gemeindlichen Struktur und des Aufgabenbereiches angestellt werden. Die Bevoelkerungsgroesse kann niemals alleiniger Massstab fuer ein Urteil sein, ob eine Gemeinde im Stellenplan oder im Personalbestand aufwendig oder sparsam ist."

Ratio of Residents to Beds in Hospitals*

City CDU Majority	Beds Total	Beds Public Hospitals	Population	Residents per Bed
Aachen	3003	–	176,582	59
Bonn	4750	–	133,246	28
Koblenz	2112	380	103,585	49
Moenchen-Gladbach	2925	183	153,599	55
Muenster	4371	–	203,110	46
Neuss	2212	–	114,883	51
SPD Majority				
Bochum	3949	–	354,165	88
Dortmund	9103	2100	645,844	78
Frankfurt	9685	1121	665,505	69
Gelsenkirchen	3791	250	355,095	94
Hannover	8076	2208	526,975	65
Herne	1186	–	104,158	87
Kassel	3815	1001	212,920	56
Muelheim	1206	–	189,286	157

Ziebill describes the ideological fluidity of the political parties in this sphere quite clearly. The CDU voices no objection to the building of city hospitals when private beds are lacking; and the SPD will tolerate a private hospital if necessary for the community. Ziebill, *Politische Parteien,* p. 52.

His last area, "Expansionism," contains his famous measure of bureaucratic strength through numerical growth. As we mentioned earlier, one must look beyond statistics to assess the power of the bureaucracy in a democratic society. The secure tenure of the German municipal bureaucrat, his reservoir of specialized information, and the esteem accorded him by German society, give him a tremendous tactical advantage in any conflicts with members of the German city councils. The added fact that the city council members serve only as "honorary" city officials, receiving no monetary compensation and virtually no staff assistance, also displays their weak position vis-a-vis the administration. The increasingly active role of the local bureaucracy in structuring decisions and thereby affecting output is in the forefront of current German political analysis, yet Professor Fried totally disregards these works, and adheres to his statistical index of bureaucratic influence – numerical growth.[11]

Despite the statistical weaknesses, however, Professor Fried's article does contain a kernel of value, for it shows that American political science has reverted to that cold formalism that Americans have traditionally regarded as the exclusive province of German political science. Instead of looking at constitutions, institutions, and statutes for guidance on the description of the political process – that renowned German trademark – Professor Fried substitutes the equally static base of statistics from the German Municipal Association.

Without attempting to see either the significance or the process behind the statistics, Professor Fried casually assumed that outputs that are municipal in the United States are also municipal in Germany. A mere listing of data in a municipal yearbook is not proof that the factors enumerated emanate from city legislatures. Those highly praised statistical data can only yield analytical fruit after they are combined with an understanding of the mechanisms and processes of the municipal policy process. In a double sense it is ironic that Professor Fried concludes his article by calling for more research in this same area, "in identifying the mechanisms or processes involved in the urban policy process," for he at the same time excludes these very processes from his own analysis. On the other hand, the leading German urbanologists, Grauhan, Mayntz, and Nassmacher to name a few, have adopted the American penchant for empirical methods, thereby breaking the shackles of institutional description and have focused precisely upon the processes that determine policy. All of which means, if Professor Fried's article is representative, that the urban analysts from both countries have changed methodological directions without sharing the results of their labors at the intersections.

GEORGE OTTE

RUEDIGER ZUELCH

University of Cologne

TO THE EDITOR:

I am happy to reply to the critique of Messrs. Otte and Ruediger. West Germany is one of the few countries in Western Europe in which, with some exceptions, no important work has been done in the field of *comparative* urban politics. Perhaps this exchange of perspectives will stimulate German urbanists to look more closely at what comparative urbanists in the U.S., Canada, Sweden, Norway, Italy, France, Switzerland, and the U.K. have been doing in recent years. We have been going beyond case studies of individual cities and beyond assumptions of uniformity to compare urban political systems in terms of their performance. Research has been guided by the focusing question: what causes urban governments to differ in what they attempt and in what they achieve? The assumption is that valid generalizations about comparative urban politics and government cannot be made on the basis of single case studies, nor deduced from ideological axioms about the urbanization process.

The lack of comparative urban research in West Germany is ironic, of course, for the Germans have long been collecting and publishing the best comparative municipal statistics of any country in the world. In the yearbooks of the German Municipal Association, comprehensive and carefully prepared data can be found on most aspects of city performance since 1890. Statistics cover elections, demography, education, culture, finance, transportation, health, industry, social welfare, housing, utilities (if, unfortunately, not crime). These statistics provide a comparative panorama of urban activity available for no other cities in the world. And yet this incomparable bank of comparative urban data (the like of which does not exist for U.S. cities) is seldom tapped by German social scientists.

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[11] For works that focus upon the strength of the bureaucracy in decision making, see, among others, Rainer Frey, "Kommunale Demokratie und Ratsfraktion," *Die Demokratische Gemeinde*, Jg. 27 (1975), pp. 357–362; Rolf Richard Grauhan, *Politische Verwaltung*, Frieburg 1970; Dietrich Fuerst, *Kommunale Entscheidungsprozesse*, Baden-Baden 1975.

In the eyes of Messrs. Otte and Ruediger, to use the statistics of the German Municipal Association is "static" and "coldly formal," while to ignore them is "empirical." My critics and I share a feeling that "cold formalism" is bad and "empiricism" is good, but we seem to cast the praise and perjoratives at different objects. The comparative urbanism exemplified in my study of German cities attempts to be (in my sense) empirical: that is, it attempts to find evidence (largely but not exclusively statistical evidence) about the relationships and processes that cause German municipalities to be similar to each other in some ways and differ in others. The assumption is that hypotheses about the impact of municipal bureaucracies, party ideologies, demographic trends, state and federal pressure, etc., need to be demonstrated, rather than assumed or asserted.

The correlative assumption is that the proper way to challenge research based on empirical data is by replication. The scientific virtue of studies such as mine is that the findings are subject to disconfirmation – but disconfirmation based on hard evidence rather than assertion. Messrs. Otte and Ruediger, instead of attempting to show that my findings and conclusions are invalid by adducing new data, deny their validity simply by asserting the existence of "facts" about German urban politics that are (somehow, but not empirically) "well known." What they say may be true: German city councils may have no power; the bureaucracies may run the cities; there may be no ideological differences between the parties. Such statements, however, require more than mere assertion to become valid generalizations. They need to be proved in research and exposed to the hazards of empirical disconfirmation.

My critics and I differ in one vital way. Examining the data on West German municipal performance, I see variance among the cities and seek to find the determinants of that variance. My critics, since they do not confront the comparative data, assume that performance is uniform. Thus, for them, a case study of one city can be used for valid generalization about all German cities. Similarly, ideological disquisitions on the nature of urban development under monopoly capitalism can lead to valid generalization about all cities under that form of economic system. Again, assertions about the role and power of state governments and municipal bureaucracies can be used for valid generalization about all German cities, since all are subject to bureaucratic rule, whether federal, state, or local. In this way, by assuming or asserting uniformity, 100 per cent of the variance in performance can safely be left unexplained.

Comparative urbanists, especially those like myself who have worked predominantly with the cities of Western Europe, seldom assume that municipal governments, even in the most decentralized systems, operate strictly on their own and in response to local stimuli only. My assumption in dealing with West German cities is that German federalism has become a *Marmorkuchen* at least as marbled, and surely as full of delectable surprises as its counterpart in the United States. It is difficult, increasingly difficult in federal systems to pin the blame for urban performance on any particular level of government, since so many functions are produced in partnership. It is undoubtedly true that some of the functions (police, hospitals, for example) used in my study as indicators of municipal enterprise are not in every *Land* municipal functions but rather functions of state government or even of the semi-public sector. I am not sure that this invalidates the use of such indicators as indirect tests of local enterprise; surely one of the major functions of local officials is to stimulate adequate performance on the part of whoever happens to be responsible for the quality of some segment of urban life. The general public may be less concerned with the legal niceties in the assignment of functions than with the quality of urban life. In fact, when German city governments (e.g. Nuremberg, Augsburg, Dortmund, Karlsruhe) make surveys of how their citizens evaluate urban performance, they include in their questionnaires the entire range of urban life, not merely the aspects for which they, as municipal officials, are legally responsible.

When they make general and unproved assertions about the nature of German urban politics or misuse the concept of empiricism, I find little of value in what Otte and Ruediger have to say. On the contrary, their discussion of the merits of particular indicators is valuable; such discussions are essential if comparative urban studies are to be refined and advanced. I should say that one of the reasons why I burdened my readers with *seventeen* measures of performance for German cities was my feeling that, given the ambiguity of all indicators, generalizations about performance should be based on as many indicators as possible.

One final point, regarding party ideology. In characteristic fashion, Messrs. Otte and Ruediger assert that the lack of ideological differences between West German local parties is so well known that the fact requires no substantiation in empirical research. Again their tendency is to assume that what is "well known" is empirically demonstrated and that the behavior of the local parties across the face of the Federal Republic is uniform. Where they take *Entideologisierung* as proven and uniform, I take it as unproved and possibly subject to variance from place to place and issue to issue. It is reasonable, after all, for the political scientist to assume that when political forces claim to be different (and West German parties *do* claim to be different), one should take the claim seriously – seriously enough to see whether claimed differences produce differences in behavior and outputs. Ideology may be dropped from programs, but may remain in characteristic responses to policy alternatives. To find this out, we must do comparative studies of the policy outputs of West German cities and not remain in the realm of the uniform and of the "well known."

ROBERT C. FRIED

University of California, Los Angeles

A Reply to James David Barber

TO THE EDITOR:

In his intemperate rebuttal of James Qualls's article in the March issue of *APSR*, James David Barber clearly identifies me, without actually naming me, as one of the referees for the original manuscript that Qualls submitted to *APSR* several years ago. The charade by means of which Barber helps the reader to guess my identity is offensive enough. Of far greater concern, however, are his charges that I approved publication of Qualls's manuscript because it contained many flattering references to my own critical review of Barber's *Presidential Character* and that I behaved improperly in not disqualifying myself as a referee because of my prior criticism of the book in question. These charges, of course, rest upon the assumption that I recommended Qualls's article for publication. It happens that I did *not* so recommend it. It happens also that Barber had reason to know of my generally negative reaction to the Qualls manuscript on the basis of information I furnished him in early 1975 in circumstances detailed below.

Notwithstanding the facts which have been in his possession for such a long time, Barber insists in language dripping with sarcasm that Qualls's criticisms ("sheer gas") found their way into *APSR* because I, influenced by "page after page in fervent praise" of my work and by "fulsome paragraphs in which Mr. Qualls pours perfume" on my head, decided on publication. He permits himself to parade before *APSR* readers his hostile fantasies about my reactions as I read the Qualls piece, portraying me as so uncontrollably greedy for applause and unscrupulous in pursuit of it that I would abandon my self-respect and moral scruples to promote the appearance of a few honeyed words in print.

Before proceeding, I wish to vigorously protest the fact that I was not given the opportunity to set the record straight in the same issue which contained Barber's insulting allegations. It is obviously unfair to oblige a person to wait three months for what by then cannot be an entirely satisfactory opportunity to undo the damage caused by such shocking personal accusations as those leveled against me by Barber.

Following are the facts: in the summer of 1974, Nelson Polsby, editor of *APSR*, requested me to review a manuscript, which he sent me with the author's name removed, for possible publication in *APSR*. The article, highly critical of Barber's *Presidential Character*, struck me as unfair to Barber. I expressed my reservations in a letter to Polsby, the text of which follows in full:

August 1, 1974

Professor Nelson Polsby
APSR
Political Science Department
University of California
Berkeley, California

Dear Nelson:

I have read the *ms.*, "Barber's Typological Analysis of Political Leaders," with much interest. While I have indeed learned from it – in that it is an instructive methodological critique of the kind that would have helped me had I taken a good Scope and Methods course – I remain quite ambivalent regarding its suitability for *APSR*.

As my annotations on the margins of the copy indicate, the author is in danger of misrepresenting, certainly grossly exaggerating, the extent to which Barber in *Presidential Character* claims that the theory of character advanced in that book is *validated* by what he published in *The Lawmakers*. There is the barest reference to *Lawmakers* in *P.C.* and, it is true, it is in context of citing antecedent research of various kinds that support what he presents in *P.C.* But, given the overtly exploratory and speculative nature of *Lawmakers*, it does not seem to me justified for the author of the paper to set it up as a source of validation for *P.C.* in order then to justify a detailed,

> systematic refutation of it on scientific grounds. I am not saying that such a critical commentary on *Lawmakers* is unjustified on methodological grounds; nor am I saying that it lacks utility. Rather, I question its justification and utility *as a critique of Presidential Character.* If my questioning of it on the latter ground strikes you as reasonable, then you have to consider whether publication of the article will not seem to many readers as an unfair example of "overkill" of *Presidential Character.*
>
> Two other thoughts: (1) Some other referee will have to attest to the soundness of the procedures your author employs to reconstruct, reanalyze, and critique Barber's analysis of his interviews in *Lawmakers;* so far as I can judge these technical matters, this part of the review is adequate. (2) Given my own role as an earlier critic of Barber's *P.C.*, I must be "involved" in some way in responding to this new critique of it. I do not know how to assess my personal involvement other than to say that it may reinforce my intellectual ambivalence toward this article.
>
> Best regards,
>
> Alexander L. George
>
> ALG:lr
>
> P.S. In lieu of a separate comment, you may transmit the second paragraph of this letter to the author if you think something in addition to my annotations of his/her article should be passed along.

I enclosed with the foregoing letter to Polsby the manuscript he had sent me which now contained my handwritten critical comments in the margins.

Several months later – I believe in late February or early March, 1975 – in the course of a telephone conversation on other matters, Dave Barber indicated that he knew I had reviewed a manuscript which picked his work apart because he had recognized my handwriting in the margins of the manuscript, a copy of which the *APSR* office had sent him (through "a clumsy clerical error in the *APSR* office," I am advised by Nelson Polsby). Barber expressed annoyance with me for having enthusiastically approved its publication.

My anonymity as a referee having been confounded and as there obviously existed a serious misconception which I wished to correct and also because I hoped to preserve our long-standing friendship, I told Barber that I had *not* approved publication and had *not* expressed enthusiasm for the piece. On the contrary, I told him, I had expressed major reservations regarding its fairness and relevance and had questioned its suitability for publication. Barber insisted that he had been told that *both* referees had enthusiastically recommended publication. Shortly thereafter, at a meeting in New Haven, I showed him a copy of my letter of August 1, 1974 to Nelson Polsby (see above). Barber read it and said he had no problem with it. I heard nothing further from him on this subject until I saw his attack on me in print.

I heard nothing further from Polsby either: he did not inform me of his decision to publish Qualls's manuscript (which, indeed, he was not obliged to do) and did not ask me to read Qualls's revised article. Until recently I did not know who the author was, and saw his revised version and Barber's reply only shortly before their publication. (They were given to me not by Polsby but by one of his students.)

Although Barber describes me as Qualls's "mentor" and "godfather," to the best of my recollection I have never met him or been in touch with him or with any of his professors about him.

In his irresponsible attack on me, Barber claims that Qualls's original manuscript was full of flattery inserted to gain my support. Barber's theory is that having succeeded in so intoxicating me that I cast professional ethics and ordinary human decency to the winds and recommended publication, Qualls (ungrateful fellow!) toned down his praise in the revised version. Barber has the facts twisted backwards: Qualls's original manuscript contained very few references to me and they weren't all flattering, either. He stated plainly in that draft that he had been unable to make much use of my article in *World Politics* on Barber's book because his essay had been almost completed before it came to his attention.

Barber castigates me for not having disqualified myself as a referee who favored publication of the Qualls manuscript in view of my own previously published critique of his *Presidential Character.* Indeed my letter reminded Polsby of my review. However, since my reaction to the Qualls manuscript was generally negative and therefore I could not be sensibly suspected of acting out of an anti-Barber bias, I saw no reason to disqualify myself as a reader.

I am astonished and saddened by Dave Barber's nasty accusations and misrepresentations. He has been an esteemed colleague and valued friend of many years. When asked to review *Presidential Character* in *World Politics* some years ago, I felt a professional obligation to state my criticisms. I did so in as fair and balanced a manner as possible, stating my disagreements without lapsing into personal innuendo. Further, I submitted my review article to Barber for his comments prior to its publication in order to avoid inadvertent misrepresentation of his views, and made a number of changes in response to his comments. Both privately and publicly I have expressed respect for his contributions and his person.

I deeply regret any distress Mr. Qualls may feel at the publication of my letter of August 1, 1974. In ordinary circumstances, it would have remained confidential. I trust he realizes that my comments were made in a constructive scholarly spirit. And it should be noted that my criticism of his article was directed at the original manuscript and may be less germane to the substantially revised and expanded version which has now been published.

ALEXANDER L. GEORGE

Stanford University

To Judge with Justice

TO THE EDITOR:

Professor Daynes's review of my book *To Judge With Justice: History and Politics of Illinois Judicial Reform* (June, 1976) is a confused and error ridden analysis which fails to distinguish between judicial reform in the structure and administration of the State's judicial system, accomplished by constitutional amendment in 1962, and the controversial issue of appointive versus elective judges, on which issue alone the contending political forces in the 1970 Constitutional Convention waged their furious battle. It also represents an astonishing misconception of the evident purpose of the book.

Let me be more specific. Professor Daynes: "His [Cohn's] major charge against the pre–1970 judicial system is that it was diverse, inharmonious and unpatterned. Cohn's reform model corrects these weaknesses, providing an orderly, structured, and administratively efficient system, but one which concentrates control in the hands of the state supreme court and the legal profession." I have read and reread this statement in total disbelief. It is not the pre–1970 judicial system which was diverse, inharmonious, and unpatterned. It was the pre–1962 system which was so characterized (pp. 3–8). That characterization dealt solely with the structure and administration of the judicial system and not with the issue of judicial selection. The 1970 judicial reform left the 1962 changes intact (with minor exceptions) and was supported by the Chicago-Cook County Democratic Organization, a fact which wholly eludes Daynes (p. 140). When Daynes's analysis thus refers to "Cohn's reform model" he is palpably oblivious that it is not a proposed model he is analyzing but a reality in effect since 1962. To this egregious misconception Professor Daynes adds untruthful distortion in his reference to "Cohn's reform model" concentrating "control in the hands of the state supreme court and the legal profession." The 1962–1970 structural provisions of the judicial system do indeed vest administrative control in the Supreme Court where it belongs, but I challenge Professor Daynes to substantiate his conclusion that the legal profession is given any role, no matter how insignificant, in those reforms. What Daynes has in mind is relevant to the controversy over judicial selection, but that issue is clearly not involved in "Cohn's model" as Daynes perceives or rather misperceives it.

An even more glaring distortion is Daynes's statement that, "a major strength of the new model [note again the reference to 'new model'] Cohn explains, is that it will allow for the appointment rather than the election of judges, guaranteeing total isolation of the judiciary from elective politics." In other references to my positions Daynes cites page and verse. In this last allegation there is none and for the good reason that I made no such statement and I again defy Daynes to supply verification for this outrageous attribution. If Daynes has in mind the generalized analysis of the prevailing attitudes of the proponents and opponents of the appointive and elective systems (pp. 16–18) he will not find therein, as indeed he will not find *anywhere in the book,* a single reference to my personal stand on this issue. To imply as he does that I have *in the book* engaged in advocacy in support of the appointive method is crude and dishonest. I have indeed long been identified as a supporter of judicial reform, and this is quite clearly stated in the biographical sketch on the back cover of the book. It should come as a surprise to the *Review* that in this particular criticism, and indeed in almost all of his charges that I am "too much the advocate of reform and too little the objective analyst of reform," Daynes has been reviewing not the content of the book but the biographical sketch of the author.

Near the conclusion of his review, following his garbled analysis of "Cohn's model," Professor Daynes comments: "Never once does he suggest what impact such a scheme [appointment of judges, I assume] unchecked by the election process, might have on the quality of justice and on democratic government in general." In this criticism Professor Daynes displays an incredible misconception of the purpose of the series of books – mine is one of six or seven – published for the Institute of Government and Public Affairs by the University of Illinois Press. Had he read the explanatory preface written by Professors Pisciotte and Gove, as any competent reviewer would routinely have done, he could not have indulged this last spurious criticism. Nowhere in this preface is it suggested that the personal views or philosophy of the author were relevant to the analysis of the political forces and influences, organizational and individual, which shaped the constitutional document. Indeed such an approach was clearly intended to be precluded. Thus Professor Daynes's complaint is based on an unforgivable miscomprehension of the purpose of the book. The book takes this issue of appointive versus elective judges, dissects its components as viewed by its supporters and opponents, and details the methods and tactics by which the respective protagonists seek to achieve their (not my) predetermined objective. If yet another philosophic polemic is to be added to the millions of words which have inconclusively dealt with the merits of the elective and appointive systems for judges as this affects "the quality of justice and democratic government in general" (two fairly simple concepts), I leave this potboiler responsibility to the superior wisdom of Professor Daynes. Such a polemic was light-years removed from the objective of the book, as Daynes should have known had he exerted a little mental effort to read the preface and assess its evident meaning.

Professor Daynes states that I evince absolute reverence for anything and anyone associated with judicial reform, show general disdain for opponents of reform and contempt for the Chicago-Cook County Democratic Organization. In this charge Daynes displays both an uncritical reading of the book and a selective and palpably discriminatory use of statements in isolation from the total context. Only space limitations imposed by the *Review* prevent me from conclusively refuting these absurd allegations.

Finally, Daynes accuses me of finding the tactics used by the Chicago Democrats "totally distasteful" (the words are his) since they are "ruthless," show signs of "organized support," and provide voting unity against judicial reform while failing "to see that the coalition supporting reform also rely on those same 'distasteful' tactics to defeat the Chicago Democrats and other opponents of reform on numerous critical votes." Again the misinterpretations and incorrect conclusions. Indeed I do refer to the Chicago-Cook County Organization as "ruthless" as well as "politically relentless and coldly efficient" (p. 118), and as "disciplined and powerful" (p. 118), and by other such descriptions. I do so not because I find these tactics "distasteful" as Daynes suggests but more in awed recognition, even admiration, of the efficiency of an extraordinary political mechanism. And to suggest that equally "distasteful" tactics when employed by the coalition are not so viewed by me is grossly superficial.

The coalition supporting judicial reform was composed of a group of political neophytes possessing none of the political muscle and organizational strength and unity of its rival. That it bested the organization on several critical roll calls is thus the core of the startling drama of the contest. If I find the coalition's success on certain parliamentary strategies worthy of praise it is because the imbalance of political power between the contending factions made such a result totally unpredictable. Daynes sees none of this. With wondrous naivete he ponders the nonexistent contradiction of my failure to see the "distasteful" aspects of the coalition's tactics which are the "same" as that of the Chicago-Cook County Organization.

Professor Daynes's review is the book's first negative critique. I have no quarrel with that fact in itself. It is unfortunate, however, that a publication as prestigious as the *Review*, devoted to excellence in scholarship, is, on occasion, unavoidably trapped into publishing so unscholarly and inept a book review as that by Professor Daynes.

RUBIN G. COHN

University of Illinois at Champaign-Urbana

TO THE EDITOR:

Professor Cohn is fortunate that *The Review* allowed him such a lengthy response to my review of his book, *To Judge with Justice* (*APSR*, June 1976). I was, unfortunately, permitted only 500 words for my original review and could not deal at length with all the issues Professor Cohn has mentioned. Yet the crux of the matter is not the numerous issues Professor Cohn has raised, though each of these could be countered, but is rather how well Professor Cohn has accomplished the stated intent of his book.

As Professor Cohn reminds us, all competent reviewers begin by examining the intent of the volume. In the Foreword Professors Pisciotte and Gove, the editors of the series of which Professor Cohn's book is a part, call for "more probing analyses to deal with questions which are answered now mostly by speculation" (p. ix). They see the Sixth Illinois Constitutional Convention as offering an ideal opportunity to study the Constitutional revision process. The Institute of Government and Public Affairs at the University of Illinois then determined that this reform convention was worthy of a series of monographs about various facets of constitutional revision. The major aim of the series was to:

> . . . *recount* – in breadth and detail – the events, personalities, strategies, conflicts, and resolutions which resulted in a new basic law for Illinois. Neither the convention nor these studies were conducted in isolation from the political environment of the state, hence, the first of several secondary goals of the series is to *contribute to the general knowledge of the politics of Illinois.* And finally, it is our hope that the series will *lend itself to comparative studies on state constitutional revision* and ultimately to the *development of descriptive and theoretical literature* in this area (p. x – italics mine).

It seems to me that the value of the book depends on the fulfillment of these aims. Professor Cohn does indeed fulfill the first of these aims: he does *recount* and even does so rather wittily. In fact, events, personalities, strategies, conflicts, and resolutions are the essence of his narrative. But it can seriously be questioned whether this book contributes very much to a general knowledge of the politics of Illinois when it so totally lacks analysis of the events Professor Cohn recounts. For the general reader who knows little or nothing about state politics and constitutional conventions, the book may be informative. It is very readable and may hold the general reader's interest. But it holds little interest for the informed political scientist who searches for insights and a "more probing analysis" of the political system. Professor Cohn suggests in his response to my review, for example, that he found the victories of the reformers on certain roll calls startling and "totally unpredictable." It is totally unpredictable, even in hindsight, because he attempts no explanation of the reformers' victory. Here is an event crying for a "more probing analysis" to replace mere speculation. Professor Cohn is so concerned with *recounting* the victory that he misses the opportunity to deepen our understanding of Illinois's politics and politics in general.

Nor does Professor Cohn's book lend itself to the development of theoretical literature. Such a case study will make a limited contribution to a theoretical framework. He has already offered an interpretation through his selection of data, based not on a systematic construct but on his own personal predilections. Professor Cohn seems oblivious to the limitations of his style, but his very word choices and starkly drawn characterizations in his book reveal his personal, rather than scholarly and dispassionate, judgments. As I pointed out in my original review of his book, judicial reformers such as delegate Wayne Whalen of Hanover, Illinois, are described as "politically wise" (p. 47), "brilliant" (pp. 47 and 79) effecting "remarkable" victories (pp. 49 and 79), while antireform delegate Dwight Friedrich of Centralia, Illinois, is referred to as a "hardened and shrewd political professional. . ." (p. 33) who used "sledgehammer tactics. . ." (p. 33) and made "crude" attacks on opponents (p. 33). And when the "crude" and "shrewd" confront the "bold" and "brilliant," the confrontation becomes particularly misleading for the student of politics. Chicago Democrats are seen as wielding "awesome political power" (p. 48) and putting intense pressure on delegates (p. 82), using "ruthless" methods and intolerantly rejecting the arguments of the opposition (p. 98). But Professor Cohn unwittingly reveals that reformers were also quick to reject arguments of their opponents, worked toward packing the committees with supporters of reform (p. 37), coalesced and organized together (p. 49), engaged in "buttonholding and cajolery, overtly if necessary, of indecisive colleagues, . . ." (p. 48) to win victories from the opposition on critical votes. He does not point out, however, that these tactics were exactly the same as those used by the Chicago Democrats whose tactics he decries. And he fails to note that the reformers, apparently, won not because of the merits of their arguments but because they were better able to manipulate the political processes. He thus misses a crucial opportunity to shed light on the process of reform. Having a definite point of view need not be a handicap, though, as in this instance, it does not produce an unbiased account which would facilitate the building of theoretical constructs. And his definite point of view also prevents the sort of analysis that would interest the professional.

Professor Cohn has indeed fulfilled, and fulfilled well, the first of the stated purposes of the series: he has recounted events at the constitutional convention. We have, however, none of the "probing analysis" asked for by

Professors Pisciotte and Gove, and, I fear, none of the insights useful to the political scientist.

BYRON W. DAYNES

DePauw University

EDITORIAL COMMENT

More on Page Charges. Political scientists who keep a sharp eye on Association expenditures have occasionally inquired whether the *Review* might not join the ranks of scientific journals that levy page charges against the authors of articles. Many learned journals stay financially afloat by this means, it is pointed out, and frequently the charge is passed right through to agencies – especially government agencies – that make grants for research.

We have in the past resisted instituting page charges on grounds that it is a tax that would fall unevenly on branches of the discipline, and would be especially onerous for the young, the nonmainstream, and the less well placed. Moreover, political science research is on the whole not well funded, and especially not by the government, so the opportunities to subsidize the *Review* by tapping the government after the fashion of our neighbors in chemistry, biology, and physics are dim at best.

Now a further deterrent has surfaced. *Science* magazine reports that in obedience to a law that has been on the books since 1912, the U.S. Postal Service has recently sent letters to two journals, *Plant Physiology* and *Astrophysical Journal,* pointing out that the custom of page charges renders the contents of these journals legally indistinguishable from advertising matter.[1] This changes the rate at which the scientific journals in question may be sent through the mails. There is also a requirement that articles for which page charges have been levied must be labeled as "advertisements."

Evidently, other journals are soon to receive similar letters. Since the *Review* does not use this particular method of financing, it seems unlikely that we shall receive one. The whole chancy enterprise of scientific information exchange is, however, of continuing concern to all of us, and it seems likely that this latest development will at some point lead to a further clarification of the status of scholarly journals. More and more, as some of our astute colleagues have pointed out, the rights, duties, immunities, and obligations of scientific and scholarly enterprises are being tested and defined at law. Here, evidently, fairly close to home, is another example.

On Access to the Review. Most readers are undoubtedly aware that the *Review* prints several different kinds of material: articles, comments, rejoinders, correspondence, book reviews, and so forth. One consequence arising from these differences is that slightly different rules apply to each type of material in determining what gets into print. Articles are unsolicited and undergo the full-scale refereeing process that has been described so often. Book review essays, though usually solicited, also move through the refereeing process. Correspondence is normally unsolicited, is read in the *APSR* office and is sometimes refereed, sometimes not. Book reviews are solicited, read in the office, are occasionally refereed, but mostly not. Comments and rejoinders are sometimes solicited, sometimes volunteered, and are usually read by the editors, but not refereed. The annual presidential address to the association and editorial comments are never refereed.

All material accepted for *APSR* publication is checked for the accuracy of citations and quotations by an editorial intern, and is read by the manuscript editor, who offers assistance to authors in matters of clarity, grammar, length, and style. This assistance is frequently accepted cheerfully, but is sometimes rejected. Readers of the *Review* can sometimes tell the difference.

In general, it has been our goal to try to do whatever we have to do with a light touch. The presidential address, for example, we consider the province of the outgoing APSA President. We endeavor to give the address as pleasant a sendoff as possible by preceding it with the ceremonial introduction delivered on the occasion of the President's oral delivery of the address, and then clearing out of the way so that the President can have her say. Likewise, we consider that it is courteous to offer space – preferably in the form of a "comment" – to scholars whose work is mentioned prominently in these pages, and especially if they believe that their work has been misunderstood. On such an occasion, words that an editor might otherwise feel a shared responsibility for smoothing down are permitted to pass unquestioned into print as the honest expression of an individual author's views.

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[1] Constance Holden, "Scientific Journal Publishers are Perplexed and Alarmed About Threatened Postage Hike," *Science* 194 (October 29 1976), pp. 502–503.

Scholars who find themselves situated similarly to these two sorts of contributors may discover that, unlike most political scientists, they receive a free ride in the *APSR*. This is also, of course, true of the managing editor, who gets to say whatever he likes in editorial comments. Under the circumstances, as a matter of policy, we do not comment adversely or argumentatively on the contents of any *APSR* article in editorial comments. We think this small exercise in self-denial is more likely to preserve the clear channel between authors and readers that is the *Review*'s central reason for being.

Jeffrey L. Pressman 1943–1977. In our last issue we had occasion to mention the name of Jeffrey L. Pressman, Associate Professor of Political Science at the Massachusetts Institute of Technology, among those former *APSR* interns to whom the entire profession owed thanks. Now we must record, with sorrow, his passing (the first among the interns) in Boston, Massachusetts, at age 33.

Jeffrey Pressman was a leader of the very earliest group of interns. He and a few of his friends in their generosity and high spirits made being an intern something special in the fragmented Berkeley graduate community of the early 1970s, and indeed gave the interns at the outset an élan they never lost. Jeffrey brought this same touch of fun to every task he undertook, in his incessant political campaigning, at the Oakland Project, from which two of his books came, at Dartmouth, where he made his first successes as a teacher, and at MIT, where he had just been awarded tenure when he was stricken with the unexplainable and profound melancholia that bore him away.

He was an amusing and kindly man, greatly gifted with the capacity to see, and enjoy, the foolish as well as the noble side of human endeavor. Many of us thought that his gifts would carry him – and therefore all of us – far toward an enriched understanding of how politicians meet some of the vexing problems of our age – the delivery of services to expanding constituencies, the sorting out of responsibilities among levels of government, the renewal and reform of rules of the game. On these and a dozen other subjects Jeffrey Pressman made a substantial intellectual contribution – just a down payment, we thought, on a full career of distinguished achievement. For a time, we shall be inclined to measure our other tribulations in the light of this loss.

Articles Accepted for Future Publication

Joel D. Aberbach, University of Michigan, "Power Consciousness: A Comparative Analysis"

Paul R. Brass, University of Washington, "Party Systems and Government Stability in the Indian States"

Paul Burstein, Yale University, "Social Cleavages and Party Choice in Israel: 'A Log-linear Analysis' "

Melissa A. Butler, The Johns Hopkins University, "Early Liberal Roots of Feminism: John Locke and the Attack on Patriarchy"

Neal E. Cutler, University of Southern California, "Demographic, Social Psychological, and Political Factors in the Politics of Age: A Call for Research in Political Gerontology"

Lloyd S. Etheredge, Massachusetts Institute of Technology, "Personality Effects on American Foreign Policy, 1898–1968: A Test of Interpersonal Generalization Theory"

Richard F. Fenno, Jr., University of Rochester, "Congressmen in Their Constituencies: An Exploration"

John A. Ferejohn and Roger G. Noll, California Institute of Technology, "Uncertainty and the Formal Theory of Political Campaigns"

Morris P. Fiorina and Charles R. Plott, California Institute of Technology, "Committee-Decisions Under Majority Rule: An Experimental Study"

Mark N. Franklin, University of Strathclyde, and Anthony Mughan, University College (Cardiff, Wales), "The Decline of Class Voting in Britain: Problems of Analysis and Interpretation"

Norman Frohlich, Joe A. Oppenheimer, Jeffrey Smith and Oran R. Young, University of Texas at Austin, "A Test of Downsian Voter Rationality: 1964 Presidential Voting"

Norman Furniss, Indiana University, "The Political Significance of the Property Rights School"

Dante Germino, University of Virginia, "Eric Voegelin's Framework for Political Evaluation in His Recently Published Work"

Robert T. Golembiewski, University of Georgia, "A Critique of 'Democratic Administration' and Its Supporting Ideation"

George D. Greenberg, Jeffrey A. Miller, Lawrence B. Mohr and Bruce C. Vladeck, University of Michigan, "Developing Public Policy Theory: Perspectives from Empirical Research"

John G. Gunnell, State University of New York at Albany, "The Myth of the Tradition"

Douglas A. Hibbs, Jr., Massachusetts Institute of Technology, "Political Parties and Macroeconomic Policy"

Barbara Kellerman, Tufts University, "Mentoring in Political Life: The Case of Willy Brandt"

Samuel Kernell, University of Minnesota, "Explaining Presidential Popularity"

James H. Kuklinski, Wichita State University, "Representativeness and Elections: A Policy Analysis"

Arthur J. Lerman, Yeshiva University, "National Elite and Local Politician in Taiwan"

Richard D. McKelvey, Peter C. Ordeshook and Mark D. Winer, Carnegie-Mellon University, "The Competitive Solution for N-Person Games Without Transferable Utility, With an Application to Committee Games"

Abraham H. Miller, University of Cincinnati; Louis H. Bolce, Fordham University at Lincoln Center; and Mark Halligan, Northwestern University Law School, "The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation Theory Using Both Objective and Perceptual Indicators"

David W. Moore and B. Thomas Trout, University of New Hampshire, "Military Advancement: The Visibility Theory of Promotion"

Edward N. Muller and Thomas O. Jukam, State University of New York at Stony Brook, "On the Meaning of Political Support"

Garrison Nelson, University of Vermont, "Partisan Patterns of House Leadership Change 1789–1976"

John M. Orbell, University of Oregon, and L. A. Wilson II, University of Nevada, "Institutional Solutions to the N-Prisoners' Dilemma"

A. Kenneth Organski, University of Michigan, and Jacek Kugler, Boston University, "The Costs of Major Wars: The Phoenix Factor"

David E. Price, Duke University, "Policy-making in Congressional Committees: The Impact of 'Environmental' Factors"

Steven J. Rosen, Institute of Advanced Studies (Canberra, Australia), "A Stable System of Mutual Deterrence in the Arab-Israel Conflict"

Howard Rosenthal, Carnegie-Mellon University, and Subatra K. Sen, University of Rochester, "Spatial Voting Models for the French Fifth Republic"

Barry S. Rundquist, University of Illinois, Urbana; Gerald S. Strom, University of Illinois at Chicago Circle; and John G. Peters, University of Nebraska, "Corrupt Politicians and Their Electoral Support: Some Theoretical and Experimental Observations"

Lester M. Salamon, Duke University, and John J. Siegfried, Vanderbilt University, "Economic Power and Political Influence: The Impact of Industry Structure on Public Policy"

Arlene W. Saxonhouse, University of Michigan, "Comedy in Callipolis: Animal Imagery in the *Republic*"

Lars Schoultz, Miami University, "The Socio-Economic Determinants of Popular-Authoritarian Electoral Behavior: The Case of Peronism"

Thomas Schwartz, Carnegie-Mellon University, "Collective Choice, Separation of Issues and Vote Trading"

Donald D. Searing, University of North Carolina, "Measuring Politicians' Values: Administration and Assessment of a Ranking Technique in the British House of Commons"

Goldie Shabad, University of Chicago, and Sidney Verba, Harvard University, "Workers' Councils and Political Stratification: The Yugoslav Experience"

Barbara Deckard Sinclair, University of California, Riverside, "Party Realignment and the Transformation of the Political Agenda: The House of Representatives, 1925–1938"

Donald T. Studlar, Centre College of Kentucky, "Policy Voting in Britain: The Colored Immigration Issue in the 1964, 1966, and 1970 General Elections"

Robert Weissberg, University of Illinois at Champaign, "Collective vs. Dyadic Representation in Congress"

Marvin Zetterbaum, University of California, Davis, "Equality and Human Need"

The Georges' Wilson Reexamined: An Essay on Psychobiography*†

ROBERT C. TUCKER
Princeton University

On Psychobiography

Forty-five years have passed since Harold Lasswell wrote into the first paragraph of *Psychopathology and Politics* the ringing declaration that "political science without biography is a form of taxidermy." The kind of biographical study needed in the discipline, if taxidermy was not to be its fate, was not conventional biography but biography as "life history." Biography as "life history," or "natural history" as Lasswell also called it, would be "concerned with facts which are *developmentally* significant,"[1] i.e., what nowadays is generally described as "psychological biography."

The distinction between psychological biography (or psychobiography for short) and conventional biography is real though not absolute. Conventional biography has been characterized by one of its scholarly practitioners as ". . . the simulation, in words, of a man's life, from all that is known about that man."[2] That is to say, it is a form of portrait painting, where the subject is a person's life and the medium of expression is language. As such it has had not only a very long history but one that shows many significant changes in life-writing conventions. Thus English biography in the nineteenth century, unlike much biography in England and elsewhere in the twentieth, observed the convention that "the fundamental reason for writing a man's life was that he was admirable."[3] Whatever the prevailing conventions, however, conventional biography has rarely been devoid of some psychological characterization of its subject – typically as an informed mind's intuitive judgment of character expressed in terms of everyday discourse.

What is distinctive of psychobiography as a scholarly enterprise is that the biographer is attempting to make sense out of the subject's life-course, or key phases of it, *in terms of a consciously thought-out psychological interpretation of that subject's personality.* This is not to say that he will be oblivious of the role of circumstance in the life course, or that his study will not contain descriptive passages or whole sections which might just as easily appear in a regular biography of the same individual.

It follows that all psychobiographies have in common a mooring in personality theory, either in one of its particular forms (Freudian, post-Freudian, or non-Freudian) or in some eclectic combination of interpretive-theoretical orientations. Beyond this, we can draw a distinction between two different ways in which the psychobiographer may make use of the theoretical position he has selected in the effort to "make sense" out of the subject's life-course psychologically. He may, on the one hand, treat the subject as an illustrative example of a general psychological formula or paradigm that he believes is applicable, e.g., the "compulsive personality" as conceptualized in Freudian thought or the "paranoid personality" as described in clinical textbooks. Or, having used one or another formula or paradigm as a *springboard* for interpretive analysis of the individual case at hand, he may work out an individualized psychological theory or set of

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*Alexander George and Juliette George, *Woodrow Wilson and Colonel House: A Personality Study* (New York: John Day, 1956) reprinted with a new preface (New York: Dover, 1964).

†I am indebted to Professors A. E. Campbell, Fred I. Greenstein, and Arthur S. Link, and to Drs. Ellen Siegelman, Joseph Wm. Slap and Edwin A. Weinstein, all of whom read this essay in its original draft and kindly responded with critical comments and suggestions. Responsibility for remaining errors and inadequacies is wholly mind. I also wish to thank Juliette and Alexander George for a letter of response explaining the reasons they adopted the interpretive position that they did.

[1] Harold D. Lasswell, *Psychopathology and Politics*, new ed. (New York: Viking Press, 1960), p. 9. Here and throughout italics are in the original unless otherwise indicated in footnotes.

[2] Paul Murray Kendall, *The Art of Biography* (New York: Norton, 1965), p. 15.

[3] A. O. J. Cockshut, *Truth to Life: The Art of Biography in the Nineteenth Century* (New York: Harcourt, Brace Jovanovich, 1974), p. 16.

hypotheses concerning the personality of his subject – referring, for example, to that person's life-goal or goals as rooted in a particular character structure, pattern of motivation, and modes of response to people and situations. It seems to me that psychobiography as a form of scholarship is developing, and fruitfully so, from the first to the second sort of approach. As Alexander George has expressed it, "to tag the subject of the biography with a label or to pigeonhole him in one of a number of existing categories does not in itself provide what the biographer will need most: namely, a discriminating 'theory,' i.e., a set of assumptions or hypotheses, as to the structure and dynamics of his subject's personality system."[4]

Since, in either of these two approaches, personality theory will give guidance to the psychobiographer's interpretation of his subject, the question arises whether it matters very much which personality theory or whose psychological perspective is chosen for this purpose, so long as the psychobiographer adheres to it as rigorously as possible and follows it as far as it will take him in his interpretive quest. In short, isn't any one of the major psychological perspectives acceptable in principle as a guideline, and what possible basis is there for preferring one over another?

One could argue that it is best to avoid a comparative analysis of different theoretical perspectives, given the overlap among them and given the inevitable difficulties of adjudication. Alternatively, one may take the view, as I do, that despite difficulties, the critical task in psychobiography is to compare alternatives and try to discover which of various possible theoretical perspectives is most fruitful for dealing with the case at hand. But then the question arises: by what criteria do we decide which theoretical perspective can do more, interpretively or explanatorily, than the alternative preferred and used by the psychobiographer whose work is under consideration?

Two criteria appear particularly applicable. First, which of the two leaves the psychobiographer with a smaller residue of refractory material, of actions, characteristics, etc., that do not easily submit to explanation in terms of the given perspective? Second, which of the two is more effective for interpreting the key points or key patterns in the subject's life, those phenomena which are most significant for the subject's influence upon people and events and hence most in need of explanation by the psychobiographer of a political leader? From the standpoint of a psychobiography that attempts to be, likewise, a psychohistory – that is, a contribution to history writing as well as to life-writing – the matter of key points or patterns is often very important; and this is where the comparison of alternative modes of psychological interpretation may be most needed and most useful for scholarship.

Getting off the Ground

Lasswell proved excessively optimistic in his early belief that biography as "natural history," or psychobiography, was close at hand.[5] Progress has in fact been slow. We might compare the recent past and present in psychobiography in political science with the dawn of the age of the heavier-than-air flying machine. Such a contraption had been conceived by various minds, inspired by a belief in its possibility, and here and there an effort was being made to construct one that would fly. Finally, a success occurred: the Wright brothers, at Kitty Hawk, got their machine off the ground and it flew. Alexander and Juliette George have in a way been the Wright brothers of psychobiography in political science. Their *Woodrow Wilson and Colonel House* has been widely appraised as a psychobiographical effort that succeeded.[6]

It may be helpful to pinpoint the sense in which the Georges succeeded. There has not been anything approaching a consensus among political scientists that a political leader's personality is a factor of potentially great or even decisive importance in explaining his conduct in the leader-role that he acquires. Many believe that the pressures and role requirements of political office, along with the political logic of the situations encountered, suffice to explain the leader's conduct; and that an individual not disposed to be responsive to these pressures and situational demands is also not likely to rise to power at all, owing to the screening process involved in any society's mode of leader selection.

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[4] Alexander L. George, "Some Uses of Dynamic Psychology in Political Biography: Case Materials on Woodrow Wilson," in *A Source Book for the Study of Personality and Politics,* ed. Fred I. Greenstein and Michael Lerner (Chicago: Markham, 1971), p. 80.

[5] In his "Afterthoughts – Thirty Years Later," appended to the 1960 new edition of *Psychopathology and Politics,* he himself noted that "... the study of politicians (and of politics in general) by methods largely inspired by psychoanalysis has made but modest progress to date" (p. 290). Since 1960, however, the psychobiographical study of political leaders has shown notable signs of progress, in Erik H. Erikson, *Gandhi's Truth: On the Origins of Militant Non-Violence* (New York: Norton, 1969), and a number of other studies.

[6] For example, according to Betty Glad, "Contributions of Psychobiography," *Handbook of Political Psychology,* ed. Jeanne N. Knutson (San Francisco: Jossey-Bass, 1973), p. 298, "the systematic use of psychobiography for the exploration of the interface between personality, attitudes, and political behavior did not really begin until 1956, with the publication of the Georges' book – *Woodrow Wilson and Colonel House* – and Smith, Bruner, and White's *Opinions and Personality.*" Fred Greenstein devotes most of a chapter of his *Personality and Politics,* 2nd ed. (New York: Norton, 1975) to an analysis of the Georges' study as a model of procedure in psychobiography.

The Georges demonstrate impressively, in the single but significant case of Wilson, that his performance in a succession of leader-roles was deeply affected by the psychodynamics of his personality; and portentously so, since what was involved in his personality-determined conduct in the nation's presidency was ultimately nothing less than United States entry into the League of Nations. As one early reviewer appropriately remarked in the light of such considerations, "this book is a good antidote to the irrepressible tendency of political scientists, as well as laymen, to think of high politics as something carried on by disembodied spirits, whose larger actions may vary in degree of shrewdness and foresight but are seldom affected by submerged and hidden passions."[7]

I fully share the view just cited, and propose to reexamine the Georges' *Wilson* in the belief that it is a work of outstanding scholarly merit and a path-finding achievement which has done much to inaugurate the psychobiographical enterprise as a vital part of political science. Moreover, I believe any genuine classic requires to be re-reviewed from time to time as scholarship develops sufficiently to permit fresh assessment. My treatment will focus on three themes: the book's underlying Lasswellian hypothesis about political man; the Georges' application of the hypothesis; and a comparison of the resulting psychological interpretation of Wilson's character and political conduct with an alternative possible interpretation based upon Karen Horney's conception of the neurotic personality.

Lasswell's Assumptions About Political Man

Woodrow Wilson and Colonel House was the product of fifteen years of study and research. From the outset, it was conceived as a psychobiography as defined above. In the early phase, the Georges worked with the hypothesis that Wilson was a "compulsive personality" as that type has been conceived in Freudian theory. At length, they found this approach inadequate and adopted a new theoretical point of departure in Lasswell's psychological characterization of political man.

Over many years, Lasswell's effort to give the study of politics a psychological foundation issued in a search for a psychologically defined "political type," a *"homo politicus."* In *Psychopathology and Politics,* political man was characterized as one who displaces "private affects" onto "public objects" and rationalizes this in terms of the public interest.[8] Eighteen years later, in *Power and Personality,* Lasswell moved to a more Adlerian position in his continuing effort to conceptualize political man. In Alfred Adler's post-Freudian "Individual Psychology," a person with one or another form of congenital weakness or organ inferiority is seen as compensating for the resultant inferiority feelings by developing a life-goal of superiority and a corresponding "style of life." Adapting this idea to his purposes as a political scientist, Lasswell now hypothesized that, apart from displacing private affects onto public objects, political man seeks power "as a means of compensation against deprivation," and specifically that: *"Power is expected to overcome low estimates of the self,* by changing either the traits of the self or the environment in which it functions."[9] The Georges adopted this "compensation" hypothesis as the basis for psychological interpretation of Wilson. In their words, "power was for him a compensatory value, a means of restoring the self-esteem damaged in childhood."[10]

In a later methodological essay, Alexander George has spoken in broader terms about the Lasswellian formula, attributing to it a general usefulness to the political scientist concerned with psychobiography. While the hypothesis provides, he writes, a "relatively 'shallow' account of the origins of the need for power," still:

> The problems of interest to the political scientist do not require the same level of explanation in matters of this kind that the psychoanalyst is interested in. Moreover, the political scientist lacks the data, observational opportunities and

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[7] Bernard Brodie, "A Psychoanalytic Interpretation of Woodrow Wilson," in *Psychoanalysis and History,* ed. Bruce Mazlish (Englewood Cliffs: Prentice-Hall, 1963), p. 123. This review appeared originally in *World Politics,* 9 (1957), 413–422.

[8] Lasswell, *Psychopathology and Politics,* pp. 74–75.

[9] Harold D. Lasswell, *Power and Personality* (New York: Norton, 1948), p. 39.

[10] *Woodrow Wilson and Colonel House,* p. 320.

> diagnostic skills for making fuller in-depth reconstructions. Under these circumstances, attempts to do so are likely to be difficult, frustrating and unduly speculative – as well as often being unnecessary.[11]

I am not convinced that the political scientist as psychobiographer is in any better position to dispense with fuller in-depth reconstructions than any other psychobiographer,[12] although it may be true in some instances. Moreover, Lasswell's conception of the politician as one who seeks power to overcome low estimates of the self, useful though it may be up to a point, presents a serious problem.

The conception has value insofar as it takes us beyond the old stereotype of the politician as one who seeks power simply for power's sake. It makes allowance for what is probably the frequent fact that people seek the power of political office for motives other than power *per se.* What needs to be called into question is the assumption that the power-drive of would-be political leaders derives from low estimates of the self. This seems *prima facie* doubtful. "Low estimates of the self" would take the form of conscious feelings of self-depreciation or self-belittlement. Very possibly, there have been exemplars in political history. But it would seem that in most cases, political power-seeking is a form of activity that appeals to expansive personalities, to people who possess very considerable (if not unusually strong) self-esteem and self-confidence, reflecting high rather than low estimates of self. Those in whom feelings of self-belittlement or self-depreciation are salient would tend to shun active involvement in the political arena, and not to be successful if they ventured into it.

Curiously, Lasswell himself, in one place, recognizes or half-recognizes this. He writes that "the accentuation of power is to be understood as a compensatory reaction against low estimates of the self *(especially when coexisting with high self-estimates)* . . ."[13] But he does not explain, psychologically, the paradox of high self-estimates coexisting with low ones. He could have done so, however, had he referred to the work of Karen Horney on the neurotic personality. This form of personality, as she analyzes it, shows high *and* low estimates of the self coexisting.

In the revision of psychoanalysis made by Horney, human beings are endowed with a growth tendency or urge toward actualization of their capacities and potentialities as persons. When, however, psychologically adverse experience in early childhood results in a condition of "basic anxiety," defined as a "feeling of being isolated and helpless in a world conceived as potentially hostile," the child typically responds by, among other things, forming an "idealized image" of himself to make up for his inadequate sense of personal identity and insecurity about his personal significance.[14] The idealized image is a picture of himself as a perfect or superhuman being along one or another line as dictated by the tendency to move *against* others in a drive for "mastery," *toward* others in search for "love," or *away from* others in quest of "freedom."

Unless the psychic conditions generating the basic anxiety change for the better, the individual in time takes a fateful further step: he identifies the idealized image as the person he really is. In Horney's terms, the idealized image turns into an "idealized self." When and if this happens, the energies available for growth or self-realization are henceforth invested in the effort to prove in action, i.e., to demonstrate to himself and others, that he *is* in fact the idealized self. Since any failure along this line incurs painful anxiety, the effort to actualize the idealized self (Horney calls this the "search for glory") takes on a compulsive character, meaning that the person in question feels driven involuntarily to prove himself (i.e., the idealized self) in action. Failures are, however, unavoidable given the individual's human fallibility, on the one hand, and the godlike or absolute character of the idealized self on the other.

Awareness of such discrepancies causes the person – again compulsively – to turn in a fury of self-accusation, self-condemnation and self-hatred against his "empirical" self, which begins to seem to him an alien being. Hence the neurotic personality is above all the victim of an inner conflict, a person "at war with himself." Being thus at odds with himself, moreover, he is virtually certain to be at odds with others. For others, if only unwittingly, are likely to inflict painful blows upon the neurotic person's "pride system" by failing, in word or deed, to treat him in accordance with the claims of the idealized self. Whence his hypersensitivity to criticism (an implicit denial of the idealized self's lofty attributes or perfection) and the vindictive hostility aroused in him toward those enemies (as he perceives them) who have given him affront by criticism or other forms of failure to affirm the idealized image. To gain a vindictive triumph over them by hitting back at them is his way of attempting to restore the injured pride. Furthermore, one of his unconscious ways of dealing with the self-hatred aroused in him by his violations of his impossibly high standards is to "externalize" it, i.e., to project it outwards and experience it as his hatred of *other* persons – against whom it is realistically possible to fight and win a pride-restoring triumph. This increases the likelihood that the neurotic's inner conflict will impair, at least at certain times in life, his relationships with others.

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[11] Alexander L. George, "Power as a Compensatory Value for Political Leaders," *The Journal of Social Issues,* Vol. XXIV, no. 3 (July, 1968), 30.

[12] Greenstein has reasoned along similar lines in *Personality and Politics,* pp. 68–69, 92–93.

[13] Lasswell, *Power and Personality,* p. 53. Italics added.

[14] Karen Horney, *Neurosis and Human Growth: The Struggle Toward Self-Realization* (New York: Norton, 1950), pp. 18, 22. The following exposition of the concept of the neurotic personality is a summary of Horney's analysis in this book.

Here, then, in the neurotic personality, we see the coexistence of both unnaturally high and unnaturally low estimates of the self. The high ones take the form of an idealized self-image possessing attributes of imagined genius, perfection, or greatness along one or another line; the low ones, in a despised empirical self experienced as an offensive stranger because of its inevitable inability to fulfill the superhuman demands and specifications of the idealized self. Through such inner mechanisms as repression, externalization, and rationalization, the neurotic does his best to preserve the pride system intact and act out successfully the role of greatness and glory in which he has cast himself in his self-idealization. But no matter how herculean his efforts along this line, he is virtually condemned to suffer a series of experiences which bring the despised self into the forefront of his self-consciousness or threaten to do so. On the one hand, he strives by every possible means to keep the "high" self-estimate salient in his experience of himself; on the other, the "low" one keeps threatening to intrude.

Although references in *Power and Personality* show that Lasswell was at the time of writing familiar with the views Horney had expressed in various writings then available, his parenthetical mention of coexisting high and low self-estimates did not betoken an attempt to make use of her theory of the neurotic personality for his purposes. His formula for "the political type" remained the one already cited: the power-seeker pursues power in order to "overcome low estimates of the self."

The Georges' Application of Lasswell's Hypothesis

The first question I want to raise is: how consistently do the Georges follow Lasswell's compensation hypothesis in their psychological interpretation of Wilson, and how well does it serve them? They do postulate, as already indicated above, that Wilson's desire for power and for a career of political leadership was motivated by a desire to overcome the low self-estimates generated during his early childhood. Wilson's father humiliated the boy by regularly making him the target of a caustic wit. The son "never retorted and he never rebelled. Instead, he accepted his father's demands for perfection, tried to emulate him, and interpreted his stinging criticisms as humiliating evidence that, try as he might, he was inadequate."[15] The Georges note that the boy did not learn the written alphabet until he was nine and couldn't read readily until he was eleven. Wilson's recollections of his youth, the authors go on,

> furnish ample indication of his early fears that he was stupid, ugly, worthless and unlovable. . . . It is perhaps to this core feeling of inadequacy, of a fundamental worthlessness which must ever be disproved, that the unappeasable quality of his need for affection, power and achievement, and the compulsive quality of his striving for affection may be traced. For one of the ways in which human beings troubled with low estimates of themselves seek to obliterate their inner pain is through high achievement and the acquisition of power. The trouble is that no matter how dazzling, their accomplishments are likely to prove only momentarily satisfying because the deep-seated low estimates persist and in short order begin to clamor anew for assuagement.[16]

Thus for the Georges, Lasswell's compensatory hypothesis explains the urge for power and achievement which made Wilson dissatisfied with being a professor (in this connection, they quote him as saying: "I want to *do* something!") and drove him to win a succession of important leader-roles: the presidency of Princeton University (1902–1910), the governorship of New Jersey (1910–1912), and finally the presidency of the United States (1913–1921). Lasswell's compensation hypothesis serves more as a generalized explanation of Wilson's lifelong quest for leader-roles than as a means of explaining specific actions taken along the way. From time to time, however, the authors invoke the hypothesis in concrete explanatory contexts. For example, they write in one place that the need to struggle against the self-depreciating tendencies from Wilson's early years "crippled his capacity to react objectively to matters at hand."[17] Again, in dealing with his troubled relations with Congress in 1913, they suggest that what dictated his leadership tactics was "his need for domination as a means of countering his low self-estimates."[18]

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[15] *Woodrow Wilson and Colonel House*, p. 6.

[16] *Ibid.*, p. 8.

Wilson's impressive successes in winning leader-roles were counterbalanced and ultimately overshadowed by a number of failures in his performance as a leader, most notably the long and losing battle with Dean West at Princeton over the location of the projected new Graduate College and the battle with Senator Lodge and others over ratification of the Versailles Treaty by the United States Senate. Even in his short career as governor, his successful passage of reforms in the New Jersey legislature of 1911 was followed by what Arthur Link calls "the debacle of the session of 1912," which was (writes Link) "the result of his own failures and personal limitations." Apart from his absences from the state at the time, the failure of leadership reflected Wilson's "temperamental inability to cooperate with men who were not willing to follow his lead completely; he had not lost his habit, long since demonstrated at Princeton, of making his political opponents also his personal enemies, whom he despised and loathed."[19]

The explanation of this pattern of recurrent leadership failure is an obvious major challenge for any psychobiography of Wilson. The Georges recognize this fact. They begin their concluding "Research Note" in *Woodrow Wilson and Colonel House* by citing Edmund Wilson's description of the career of Wilson:

> As President of the United States, he repeated after the War his whole tragedy as president of Princeton – with Lodge in the role of West, the League of Nations in the place of the quad system, and the Senate in the place of the Princeton trustees. It is possible to observe in certain lives, where conspicuously superior abilities are united with serious deficiencies, not the progress in career or vocation that carries the talented man to a solid position or a definite goal, but a curve plotted over and over again and always dropping from some flight of achievement to a steep descent into failure.[20]

Alexander George calls this the "self-defeating pattern" and "self-defeating dynamism" in Wilson's career.[21] One of the authors' chief concerns in their study of Wilson is to explain this pattern.

Their way of doing so involves a certain shift of explanatory perspective. Lasswell's compensatory hypothesis has been invoked to explain Wilson's quest for power, his need to dominate people in order to assuage inadequacy feelings. The self-defeating pattern manifested itself, however, *after* Wilson had attained various leader-roles and was exercising the authority that they conferred upon him. Specifically, when as leader he was confronted with determined opposition to a project which he had proposed and was trying to put through – the Graduate College location issue at Princeton and the League issue are the two principal instances – he would grow rigid and uncompromisingly insistent upon having his way *in toto* and upon utterly defeating and humbling those who led the opposition against him – even though this meant the loss of the project itself in a form that intrinsically ought to have been acceptable to him.

The Georges recognize that Wilson's conduct in these recurring situations of challenge to his authority as leader calls for a more concrete, a more *ad hoc* explanation than the generalized urge for power and achievement which they see as stemming from low estimates of self. In constructing this explanation, they shift their emphasis from a need for domination to a need for "freedom *from* domination" or a "fear *of* being dominated."[22] This in turn is speculatively derived from the vicissitudes of Wilson's childhood relationship to his father. The argument runs as follows: children generally experience a certain amount of resentment of their parents, and this tendency would have been intensified in Wilson's case by his father's harsh ridicule of him. But the anxiety aroused by these hostile feelings caused Wilson to repress them from consciousness and assume an attitude of "surpassing devotion" to his father, so that "there is not a shred of evidence that he ever once openly rebelled against his father's authority. Instead, he submitted and became an extravagently devoted son," so much so that "even in successful maturity, he retained a feeling of incompetence in his father's presence."[23]

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[17] *Woodrow Wilson and Colonel House*, p. 114.

[18] *Ibid.*, p. 151.

[19] Arthur S. Link, *Woodrow Wilson: The Road to the White House* (Princeton: Princeton University Press, 1947), pp. 206–207.

[20] *Woodrow Wilson and Colonel House*, p. 317.

[21] George, "Some Uses of Dynamic Psychology," p. 92.

[22] *Woodrow Wilson and Colonel House*, pp. 43, 12. Italics added.

The final step in the argument is that the Dean Wests of Wilson's career as a leader – those who opposed and threatened to defeat his cherished projects – were father-figures against whom he was determined to express the rebellious and hostile feelings that he had suppressed in relation to his real father, on the implicit motto of "never again." The authors' statement of the argument runs as follows:

> He bristled at the slightest challenge to his authority. Such a characteristic might well have represented a rebellion against the domination of his father, whose authority he had never dared openly to challenge. Throughout his life his relationships with others seemed shaped by an inner command never again to bend his will to another man's. He seems to have experienced men who were determined to make their viewpoints prevail against his own – men like Dean West at Princeton or, later, Senator Lodge – as an unbearable threat. They seem to have stirred in him ancient memories of his capitulation to his father and he resisted with ferocity. *He* must dominate, out of fear of being dominated.[24]

The fact that the authors have introduced a new line of reasoning (outside of or going beyond the Lasswellian compensatory formula) to explain Wilson's self-defeating pattern is not objectionable, although it would seem desirable, if possible, to account for the latter in terms of the same *system of explanation* that applies to Wilson's other characteristic patterns of behavior. The new line of reasoning does, however, present some problems. As acknowledged in the first of the statements cited above, it is speculative. There is no direct evidence for "the violence he had once felt but never ventured to express in response to his father's overwhelming domination," and the only indirect evidence – his notable slowness to learn the alphabet and to read – is presumably interpretable in more than one way. As the Georges emphasize, Wilson's lifelong demeanor toward his father was admiring and deferential; in short, anything but hostile, violent, rebellious. They interpret this as signifying repressed violent feelings against the elder Wilson.[25] This is no more subject to disproof than it is to proof. Certain questions might, however, be raised. If Wilson so successfully (through reaction-formation) managed to suppress his underlying rebelliousness against his dominating father, might he not have shown a like tendency in relations with father-like dominating figures encountered later in life, the very Wests and Lodges against whom he fought so uncompromisingly? Or, if we allow that he was driven to act out against other would-be dominators the rebellious violence that he had repressed in himself with reference to his own father, why do we not see a pattern of rebellious violence against numerous potential father-figures encountered earlier in life, e.g., school teachers, or superiors in academic life with whom he had dealings prior to his presidency at Princeton? Why did the suppressed aggressive violence come out in him only in these few instances in middle and later life?

None of what has been said here invalidates the Georges' interpretive approach to Wilson. At most it suggests that the problem of working out a fully satisfactory psychological interpretation of him remains unsolved, and that an alternative to the Georges' approach may be worth trying out. It seems to me that the most promising basis for such an alternative is the conception of the neurotic personality presented by Karen Horney in her final and most important work of synthesis, *Neurosis and Human Growth*. That Woodrow Wilson was a neurotic personality and that much of his behavior which is of special interest to students of politics invites interpretation on this hypothesis will be the burden of the argument in what follows, and the evidence will be drawn from the Georges' study itself.

An Alternative Interpretation

In his review of the Georges' book cited above, Bernard Brodie writes: "Wilson was intensely neurotic – though the authors refrain from using this or like terms throughout the book – and to get into the dynamics of his actions inevitably requires recourse to concepts and insights that have developed out of the discoveries of Sigmund Freud."[26] Brodie was correct, I believe, in saying that Wilson was "intensely neurotic," although the Georges avoidance of the term apparently reflects doubt about this on their part. The Horneyan conception of the neurotic personality, at any rate, was not, in their view, fully applicable to Wilson. As Alexander George says elsewhere,

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[23] *Woodrow Wilson and Colonel House*, p. 9.

[24] *Ibid.*, pp. 11–12.

[25] Noting the importance placed by the Georges on the pattern of reading retardation in boyhood, Greenstein finds that the Georges' interpretation of Wilson's idealization of his father involved "reaction-formation" (*Personality and Politics*, p. 84). This is fully plausible although the Georges do not use the term itself. In Freudian theory, reaction-formation represents an individual's attempt to defend against a certain repudiated tendency in himself by going to the opposite extreme.

> If Wilson is not the simple clinical stereotype of a compulsive, neither can he be regarded as a full-blown neurotic. True, one cannot read, for example, Karen Horney's insightful and penetrating descriptions of neurotic drives and of the neurotic character structure without being struck by the applicability of much of what she says to Wilson. But these descriptions are applicable only to a certain point and, upon reflection, one is on balance equally or more impressed with the extent to which Wilson's behavior and career *diverge* from those of her patients. This divergence from the clinical picture concerns precisely the critical question whether the neurotically disposed individual is able to deal adequately with his conflicts and hence retains the ability to function effectively. For Wilson was, after all, a highly successful person. He was able to overcome a severe disturbance in childhood development; thereafter, not only did he keep fairly well in check the compulsive and neurotic components of his personality but he succeeded in large measure in harnessing them constructively to the achievement of socially productive purposes. To the clinical psychologist, therefore, Wilson is interesting as much because he was able to overcome childhood difficulties and to perform as successfully as he did in public life, as he is because of the pathological pattern of self-defeating behavior he tended to repeat on several occasions during his public career.[27]

I have quoted this statement at length not only because it sets forth a distinctly skeptical, not to say negative, view of the applicability of Horneyan theory to Wilson, but also because it raises an important general question.

This is "the critical question whether the neurotically disposed individual is able to deal adequately with his conflicts and hence retains the ability to function effectively." Professor George implies that Horney would answer in the negative – a position that I believe is mistaken. Horney at least allows for the *possibility* of effective functioning on the social plane. In her view, as I read it, neurosis is often a mighty motive force for achievement, including achievement of what for many would be "socially productive purposes" in the political realm. Notwithstanding the egocentricity of the neurotic individual's "search for glory," he may in some instances be spurred on to impressive political success, even triumph, by the driving inner need to prove the idealized self in action and gain public recognition of it.[28] Especially in those societies whose cultural norms or mores place a high value on power, fame, etc., individuals of exceptional ability who are motivated by neurotic ambition may be highly successful in the achievement of their goals and may thereby, through very much of their careers, find in this external success at least the mirage of a solution for their inner conflicts. Of course, the person who is inwardly conflicted ("at war with himself") because of neurosis will pay a high psychological price for this form of solution. Moreover, by virtue of the psychodynamics of neurosis, summarized earlier, the neurotic's inner conflicts will place a heavy strain on his relations with others. They may, in fact, impair his effectiveness in an organizational role. But the neurotically generated social animosities or conflict may at times be, or appear, socially "functional" depending upon the nature and situation of the organization itself, e.g., if in some sense it is a fighting organization with an ideology of militance in terms of which an enmity born of neurosis can credibly be rationalized.

Whatever position we take on this general issue, when we turn to Wilson the case against the thesis that he was a neurotic personality is weak. For here we clearly see both sides of the coin. On the one hand, enormous ambition combined with extraordinary political talents drove him to dizzy heights of success in a career of leadership that ran in a single decade from the presidency ol a university to the presidency of the United States. None of this success is incompatible with the possibility that Wilson and his ambition were neurotic in character. On the other hand, his stunningly successful career was bedeviled by repeated searing personal conflicts in which Wilson's response to those who incurred his enmity by opposing his projects comprised a "pattern of self-defeating behavior" which Professor George himself calls "pathological." Such a duality of accomplishment and failure, of greatness come to grief, would suggest the possibility of neurosis even if we knew nothing more about the individual in question. To argue the proposition in positive terms, however, we must turn to the evidence.

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[26] Brodie, "A Psychoanalytic Interpretation of Woodrow Wilson," p. 115.

[27] George, "Some Uses of Dynamic Psychology," p. 83. From one of the references to this article (p. 97) it appears that the quoted comment on Horney was based on a reading of two of her earlier works: *The Neurotic Personality of Our Time* (New York: Norton, 1937) and *Our Inner Conflicts* (New York: Norton, 1945). The latter work presaged in many ways the fuller and more systematic account of the dynamics of the neurotic personality presented in *Neurosis and Human Growth.*

[28] A notable historical example which I have attempted to document in detail, in terms basically of Horneyan theory, is Stalin. See Robert C. Tucker, *Stalin As Revolutionary 1879–1929: A Study in History and Personality* (New York: Norton, 1973), chaps. 3–4 and 12–14.

Horney discusses the neurotic personality in general terms and at the same time, as indicated earlier, suggests a threefold typology of neurotic personalities. She distinguishes (1) those who seek "expansive" solutions in one or another form of mastery; (2) those who seek "self-effacing" solutions in love; and (3) those whose solution is "resignation" through aloofness from others or freedom. She also suggests that these various "solutions" are at best dominant trends in a neurotic life-course and that many cases (if not all) are mixed ones. Those who seek expansive solutions are, moreover, divided into "narcissistic," "perfectionistic," and "arrogant-vindictive" subtypes characterized respectively by the need for admiration, extreme perfectionism, and the need for a vindictive triumph over those perceived as enemies. A neurotic who makes a career of political leadership will almost self-evidently belong to the expansive category, although it has been hypothesized that the appeal of mastery through political accomplishment could be accompanied in some instances by self-effacing tendencies expressing the appeal of love.[29] That this hypothesis applies to Wilson may be suggested by such a statement as the following in a letter to his first wife: "I have an uncomfortable feeling of carrying a volcano about with me. My salvation is in being loved. . . . There surely never lived a man with whom love was a more critical matter than it is with me!"[30] Yet Wilson's dominant solution was certainly the expansive one, with elements of all three subtypes or subtendencies present. As we shall see, the last of these, i.e., the need for a vindictive triumph over enemies, is especially relevant to the self-defeating pattern.

On the basis of Horney's generalized description of the neurotic process and the detailed biographical material provided by the Georges in their book, Wilson was a classic case of the neurotic personality. It is quite clear from their account of the boy's early life, of "the crushing feelings of inadequacy which had been branded into his spirit as a child,"[31] that he experienced the basic anxiety that in Horney's view fosters neurosis. Second, we see in the young Wilson ample evidence of the self-idealization which Horney explains as the anxiety-ridden youngster's method of finding a sense of personal worth and significance in such circumstances. As a boy at the dinner table, he was heard to say, "papa, when I get to be a man, I'm going to have a lofty position." "He spent his childhood and youth," the Georges write, "industriously preparing to be a great man."[32] Also, he soon began to visualize the political realm as the scene of his destined greatness. At sixteen, while conversing with his cousin Jessie, he referred to a portrait on the wall and said: "That is Gladstone, the greatest statesman that ever lived. I intend to be a statesman, too."[33]

Young Wilson created an image of himself as a future great statesman, and eventually identified himself with it; the idealized image became, in Horney's terms, an "idealized self," which he felt driven to prove in practice. Since the idealized self was a statesman-self, the goal of his compulsive striving to demonstrate it in practice *can* be described as "power," but it would be more accurately designated a quest for great political accomplishment or "search for glory." In the Georges' words, "what attracted him, apparently, was the prospect of exercising leadership *per se*. He was . . . , throughout his career, a leader in search of a cause. One feels, almost, that the various causes for which he fought so passionately in later years were in themselves almost incidental to him. A man cannot exercise power in a vacuum, after all."[34] An indication of the importance of seeing the search for glory as the driving force of Wilson's life is that he especially sought public acknowledgment and applause, which need not have been the case if power *per se* were his object. From boyhood, his ambition was to become a great orator, and as a political man he was, all his life, above all a magnificent *performer.* This was true of him as a teacher as well: "Wilson's performance at Princeton, from the very start, was superlative. His class lectures were celebrated. . . . Sometimes, at the end of a particularly brilliant performance – and they were *performances* – the students would impulsively burst into applause."[35]

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[29] This hypothesis has been suggested by Mr. Harry Hirsch of the Princeton Politics Department in a seminar discussion. For Horney's classification of neurotic personality types and analysis of the main types and subtypes, see *Neurosis and Human Growth*, chaps, 8, 10 and 11.

[30] *Woodrow Wilson and Colonel House*, p. 21. One's interpretation of this statement would turn on what one took to be the precise meaning that Wilson placed on "love." It could have carried, for example, the connotation of admiration or adulation, which in turn would be consonant with the "expansive" solution.

[31] *Ibid.*, p. 114.

[32] *Ibid.*, pp. 8, 9.

[33] *Ibid.*, p. 3.

[34] *Ibid.*, p. 29.

What is salient in this personality is a lofty self-estimate, not a low one, although the latter may have lurked below the surface in the form of repressed or semirepressed self-condemnatory tendencies arising from the empirical Wilson's inability always and wholly to *be* the idealized self in actual practice. In short, we have here that very combination of high and low self-estimates which was discussed earlier. Public plaudits, and in particular those of the political communities over which he successively presided as leader, would have represented to Wilson a proof of success in fulfilling his ambition to be a new Gladstone. Political success, as attested by public approval, was confirmation of the idealized self, and all the more necessary because the suppressed self-doubts and self-condemnatory tendencies inevitably made him insecure in his lofty self-estimate and hence in need of constant support in the form of praise and admiration.

Speaking of neurotic pride, Horney writes:

> With all his strenuous efforts toward perfection and with all his belief in perfection attained, the neurotic does not gain what he most desperately needs: self-confidence and self-respect. . . . The great positions to which he may rise, the fame he may acquire, will render him arrogant but will not bring him inner security. He still feels at bottom unwanted, is easily hurt, and needs incessant confirmation of his value.[36]

Wilson emerges in the Georges' account as just such a person. They speak of "that undercurrent of restless dissatisfaction which was the man's fundamental mood."[37] Elsewhere, they write that "Wilson's thirst for accomplishment was unquenchable. A project successfully completed might for a moment still his inner doubts – but only for a moment."[38] Horney's line of reasoning, based on the concept of the idealized image of self and the never-ending and yet never fully successful quest to actualize and gain confirmation of it, explains all this better than the Lasswellian formula does.

The same is true of Wilson's way of responding to approval and admiration from those around him or from strangers, and his way of responding to disapproval or disagreement. Because of the inevitable insecurity of the pride structure, the neurotic personality is, as Horney explains, in constant need of reassurance from others that he *is* the paragon that he believes himself to be; and he is hypersensitive to criticism because it threatens to mobilize and bring him face to face with his suppressed *self*-critical, *self*-condemnatory tendencies. Consequently, he tends to react with vindictive hostility to the source of the criticism or opposition, upon whom his self-condemnation is projected. Wilson's enormous need for reassurance and admiration, and his resulting manner of responding to flattery, of which Colonel House took such active advantage – all this is abundantly documented in the biographical facts cited by the Georges. So is his hostility against his detractors or those who in any way threatened to outdo him in a leader-role, as Dean West did at Princeton. "All of Wilson's close friends – the men, the women, the professors, the politicians, the socialites – shared one characteristic: they were or at least had to seem to him to be *uncritical* admirers of the man and of everything he did."[39] Further, the authors note as "truly remarkable" the "vast number of letters Wilson wrote to friends fervently expressing his gratitude for their faith and affection. So far from reducing his energy for this type of letter-writing, the pressure of public business, particularly when he was under attack, seemed to intensify his need to turn for solace to those who restored him with their uncritical approval."[40] Naturally, he was most in need of this uncritical approval when "under attack," because public attack would intensify the insecurity of his idealized self.

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[35] *Woodrow Wilson and Colonel House,* p. 28. During his Princeton period Wilson made this statement, which I believe was unwittingly self-revelatory: ". . . I am covetous for Princeton of all the glory that there is, and the chief glory of a university is always intellectual glory. The chief glory of a university is the leadership of the nation in the things that attach to the highest ambitions that nations can set themselves, those ideals which lift nations into the atmosphere of things that are permanent and do not fade from generation to generation" (Link, *Wilson: The Road to the White House,* p. 44).

[36] Horney, *Neurosis and Human Growth,* p. 86.

[37] *Woodrow Wilson and Colonel House,* p. 24.

[38] *Ibid.,* p. 119.

[39] *Ibid.,* p. 31.

[40] *Ibid.,* p. 122.

The Georges' way of explaining Wilson's compulsive need for uncritical admiration and agreement with his views differs from the Horneyan explanation suggested in the foregoing paragraph. As they see it:

> Intellectual disagreement or the feeling that a friend disapproved of some project he had in hand aroused intolerable anxieties, the echoes of indelible boyhood impressions. For the boy Wilson had learned that if he did not earn his father's approval by instantly accepting his every opinion and behaving accordingly, he stood in danger of forfeiting his father's love. To the man Wilson, identity of opinion and love were inseparably linked. . . . To him, if a friend disagreed with him about a matter of importance, it signified that the friend no longer cared for him. He reacted in the way he once had feared his father would react in similar circumstances – he broke the relationship.[41]

To me this explanation is not cogent: it simply transposes to the adult Wilson a way of relating to persons that corresponds to his father's way of relating to *him* when he was a child. But it is not self-evident that Wilson would have taken over this trait from his father; he might have rebelled against it. Whereas in terms of Horneyan theory, we do get a dynamic explanation of the behavior in question. Approval of him and his projects, especially by those whose opinions mattered most to him, was a vital need because it ministered to his craving for reassurance that he *was* his "true self," i.e., the idealized one. It was for this reason that he required "the soothing balm of flattery," as the Georges put it in describing Colonel House's way of approaching Wilson.[42] Significantly, the Georges themselves seem at times to see the behavior in these terms:

> He needed his friends to confirm his faith – so easily shaken by outer attack because so savagely preyed upon from within – in *his great destiny*, in *his human worth*. He needed their tributes to his *selfless idealism*, particularly when the detractors rudely stripped away his carefully wrought rationalizations and, with cruelly used insight, broadcast the power-seeking, self-centered, arrogant aspects of his behavior.[43]

The Self-Defeating Pattern: Two Views

We come, finally to Wilson's repeated rigidity and vindictiveness in response to challenges to his authority as expressed in such cherished projects at his Graduate College conception at Princeton and the League of Nations Covenant. I have summarized the Georges' interpretation of this behavior as Wilson's effort to gain "freedom from domination" by figures whom he may have experienced as reincarnations of his domineering father, and I have questioned that view, believing that an alternative interpretation in Horneyan terms is more persuasive.

The Georges draw a useful distinction between "Wilson the power-seeker" and "Wilson the power-holder." As power-seeker, he was "free to devote every ounce of his intelligence and energy to waging a realistic campaign to attain his goal." Indeed, "he proved himself a practical politician without peer, a prodigy in the art of elevating himself to power."[44] Certainly, Wilson demonstrated notable flexibility in the process of power seeking, and a capacity to adapt his policy positions to the requirements for success. This was shown, for example, in his shift from a conservative political stance before winning the governorship to the reformism which powerfully assisted him along the path to the presidency. His flexibility was repeated in his quick shift to an antibossism posture in state politics after encouraging the Democratic party bosses of New Jersey who groomed him for the governorship to believe that he would remain loyal to them. His troubles always emerged *after* he had won a position of power. Of Wilson the power-holder the Georges write that "having attained an opportunity for exercise of power, first as President of Princeton and finally as President of the United States, he was no longer able to suppress his inner impulses toward aggressive leadership."[45] Further: "Having legitimized his drive to exercise power by laborious self-preparation and by adopting worthy goals, Wilson felt free to indulge his wish to force others into immediate and complete compliance with his demands."[46]

But Wilson the power-holder did not become immediately and indiscriminately aggressive in his leadership tactics. As the Georges take care to point out:

> In each of the major executive posts he occupied during his life there was an initial period during which the type of leadership he exercised in response to his inner needs coincided with the type of leadership the external situation required for impressive accomplish-

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[41] *Woodrow Wilson and Colonel House*, pp. 31–32.

[42] *Ibid.*, p. 126.

[43] *Ibid.*, p. 122. Italics added.

[44] *Ibid.*, pp. 58, 116.

[45] *Ibid.*, p. 116.

[46] *Ibid.*, pp. 117–118.

> ment. He drove the faculty and trustees at Princeton to accomplish an unprecedented series of reforms. The New Jersey legislature of 1911 was a triumph of productivity in his hands. Later, he was to exact a brilliant performance from the Sixty-Third Congress of the United States.

They go on:

> Wilson's difficulties arose when he encountered opposition, often evoked partly in reaction to his own driving demands, and when the chance for further success hinged upon his ability to alter his tactics. . . . Indeed, he was usually least capable of flexible responses in the situations which most required them. For angry opposition only intensified his anxieties and the more surely dictated a stubborn determination to subjugate his foes.[47]

This passage suggests that the opposition to which Wilson responded in this manner was wittingly or unwittingly provoked ("often evoked partly in response to his own driving demands") by the man himself. The Georges reinforce this inference when they say a little further on: "His *provocative behavior* attracted the personal hostilities of his opponents. They, then, attacked him and his programs in a vindictive spirit."[48]

Now if, in the initial periods, Wilson as leader was capable of prodigious accomplishment (and the flexibility it requires), the distinction between Wilson the power-seeker and Wilson the power-holder, for all its usefulness as a starting point, does not take us to the core of the problem. What has to be explained is not a generalized aggressiveness in Wilson's enactment of his leader-roles, but specifically his opposition-provoking behavior *in office* and the vindictiveness which this opposition typically aroused in him – with politically self-defeating consequences. Approaching this problem in terms of the hypothesis that Wilson was a neurotic personality in Horney's terms, a solution suggests itself along the following lines. His motivation, as has been argued, was never a desire for power *per se;* it was the search for glory, and more concretely the drive to prove the idealized Wilson in action through leadership success. The prime test of such success was winning a leadership role that would bring him fame, honor, acclaim and acknowledgment of his talents, along with the opportunity to reap more such rewards by demonstrating in the office won his greatness as a leader.

Accordingly, no sooner had he obtained a particular leadership role than he would take advantage of it to espouse and strive to put through new ideas, new departures in policy, new programs which, if successful, would bring greater glory and possibly, also, open the way to a still higher leadership role with its still greater opportunities for further leadership success. Since in his initial periods in office he found not only real needs for change but also real opportunities to persuade the given establishment and community of the desirability of the proposed reforms, the initial scale of Wilson's accomplishment was impressive. But he would be driven by his compulsive need for glory, reinforced now by the initial leadership success, to press for further dramatic and acclaim-producing leadership projects despite the inevitable growth of resistance to further change; or else, as in the governorship, he would lose interest after exploiting the potentialities of the post for his purposes and thus allow the success in the legislative session of 1911 to give way to the failure of that in 1912.

To cite an especially important instance of Wilson's motivation as leader, we may refer to his espousal of the League of Nations. Colonel House appealed to Wilson's desire for glory in this connection: "All through the war, in seeking to stimulate Wilson's interest in international affairs, House shrewdly assured him that he could achieve immortality by pioneering a world organization which would insure peace."[49] And further: "He had always wanted – *needed* – to do immortal work. Devising a peace settlement which would prevent future wars was a task which appealed to everything within him which strove for self-vindication through accomplishment. For what greater good could a man do than engineer the end to human strife?"[50] But the moral motivation was inextricably intertwined with the egocentricity of Wilson's search for glory. Once he had taken up the project, Wilson was highly possessive about it. As House confided to his diary:

> I wish again to call attention to the selfishness which seems to lurk in the minds of those in authority. The President is anxious to state his

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[47] *Woodrow Wilson and Colonel House,* pp. 118–119. On these points the Georges are in full agreement with Link. See Link, *Woodrow Wilson: The Road to the White House,* p. 45.

[48] *Woodrow Wilson and Colonel House,* p. 121. Italics added. See also the authors' statement in the Introduction (p. xx) that: "As President of the United States, his provocative behavior all but invited the catastrophic defeat he suffered when the Senate refused to ratify the Versailles Peace Treaty with the League of Nations in it."

[49] *Woodrow Wilson and Colonel House,* p. 189.

[50] *Ibid.,* p. 197.

> peace terms before Lloyd George and Clemenceau have an opportunity to forestall him. It is not team work.... It is the thing I have complained of so often in this diary, that is, that it is not so much general accomplishment that those in authority seem to desire as accomplishment which may redound to their personal advantage.[51]

Whoever rose to challenge Wilson in such a situation, by opposing the plan or Wilson's particular version of the plan, aroused him to a vindictive fury as compulsive as the need for glory which underlay his espousal of the plan. For such a challenge was implicitly a challenge to the validity of Wilson's exalted self-appraisal, his idealized statesman-self and its attributes of wisdom, nobility, farsightedness. Having difficulty in imagining that the plan (and hence he, himself, as its sponsor) was deficient in any way, Wilson could only interpret the opposition as motivated by the ugly ulterior designs of an evil opponent. Such a rationalization was his only means of defending the idealized self in that situation.

Consequently, Wilson became not only utterly intransigent toward the challenge, he also became enraged against the challenger. Very likely this rage was compounded by the projection upon the opponent of Wilson's own repressed self-hate. His only recourse now was to do everything possible to expose the opponent as malign and to defeat him humiliatingly. Gaining such a pride-restoring vindictive triumph eventually became more important to Wilson than putting through the project itself, no matter how much effort he had invested in it. As Horney notes in discussing such motivation in the neurotic personality, "compulsively he has to drag his rival down or defeat him."[52] I submit that the psychodynamics of Wilson's self-defeating pattern find explanation in these terms.

To conclude with a speculative postscript to this argument, let us recall the Georges' distinction between Wilson the power-seeker and Wilson the power-holder and their reference to his "provocative behavior" while in power. We might hypothesize that underlying the provocative behavior was not only the search for glory through great leadership that led him to drive too hard, but also *an unconscious need to provoke opposition in order that, by overcoming it, he could experience the winning of the leader-role again,* for example by appealing to his mass constituency over the heads of his establishmentarian opposition. As the Georges say in their final paragraph, "Wilson sought vindication."[53] This meant, we might add, not alone the sought-for vindictive triumph over the evil challenger, but the rewinning of legitimate power in the very process of gaining that political victory.

This reasoning would suggest that Wilson had an unconscious fantasy of being challenged and of overcoming the challenge gloriously, thereby gaining vindication both of himself and some immediate threatened project expressive of his greatness as a leader. His provocative behavior may be seen from this viewpoint as a way of creating, while in the leader-role, situations that made it necessary for him, as it were, to win it all over again. His tendency, then, was *not* to defeat himself, but to produce challenges to himself that would enable him to win the leadership, and the plaudits, again in the face of the very resistance that he himself had provoked. So, *after* winning a leader-role, Wilson was driven, while enacting that role, to go on reenacting the experience of winning it, to go on being a "power-seeker."[54]

This re-examination of the Georges' psychological interpretation of Woodrow Wilson has been accompanied by the outlining of an alternative approach based on the hypothesis that Wilson was a neurotic personality. Earlier, some criteria were suggested for measuring the relative explanatory values of differing psychological approaches in psychobiography. Perhaps the most fitting words with which to bring the essay to a close would be those of the Georges: "Let the reader consider whether these patterns of behavior become more consistently comprehensible in terms of the explanations herein offered than in terms of other explanations. That will be the best test of their usefulness."[55]

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[51] Quoted in *Woodrow Wilson and Colonel House,* p. 189. By "those in authority" House, of course, meant the president.

[52] Horney, *Neurosis and Human Growth,* p. 198.

[53] *Woodrow Wilson and Colonel House,* p. 315.

[54] I am indebted to Dr. Joseph Slap for elucidating in a personal communication the way in which unconscious fantasies can underlie certain repetitive forms of conduct or lead an individual to recreate situations which activate such forms of conduct.

[55] *Woodrow Wilson and Colonel House,* p. 12.

Khrushchev Remembers: A Review Essay*

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Now that the authenticity of *Khrushchev Remembers* (hereafter referred to as *KR*) has reportedly been established, its value to scholarship may be assessed. Basic questions are: How fresh and reliable is the content? Does it contribute to our understanding of the issues and currents of thought which inform Soviet politics? Is it an accurate portrayal of the alignment of major forces at work in the supreme leadership of the USSR?

Unfortunately, little of what was occurring behind the scenes in the Kremlin during Khrushchev's career and was hitherto unknown has surfaced.

A great deal of the material, especially in the first volume, is hardly novel. This is true of the recital of major crimes of the Stalin era, circumstances surrounding Stalin's death, sycophancy in Stalin's entourage, Stalin's view of Mao Tse-tung and lieutenants as "margarine Communists," extreme Russian nationalism of the Leningrad party group purged in 1949, the way in which Beria's agents staged a street accident to murder the Yiddish theater actor Mikhoels, and Soviet decision making in the Cuban missile crisis. *KR* merely echoes what is known about these phenomena from Khrushchev's Secret Speech of 1956, Svetlana Alliluyeva's *Twenty Letters to a Friend,* Milovan Djilas's *Conversations with Stalin,* Herbert Feis's *China Tangle,* N. Rutych's *KPSS u Vlasti,* Ilya Ehrenburg's *Memoirs* and NBC's taped interviews with Khrushchev in retirement. Small wonder that doubts were raised about the authenticity of the first volume shortly after its publication. In any event, some of the stories cannot be accepted as truthful solely on the grounds that they are too fantastic to have been invented, which is an earthy criterion of credibility to be found in the literature of Sovietology.

Many points of detail are questionable. In spite of what *KR* says, Soviet military observers doubtless gave direction to the army of North Korea in 1950; Stalin's daughter has denied that she was physically abused by her father during his drinking bouts with associates; Western delegates to the 15th (1960) UN General Assembly session did not bang on their desks and make noise; the Soviet currency reform that went into effect in 1961 was not "simply a matter of convenience" which "did nothing to raise the price of products" (II, 145) – worker unrest and arrests are known to have ensued in the hinterland; and far from serving as a model of "proletarian internationalism," the USSR virtually cut off its aid to North Vietnam during the 1962–64 period.

Khrushchev's sketches of onetime critics in the ruling group are quite misleading. Premier Malenkov in 1953–54 was not "completely without initiative" (I, 393) but was actually one of the first to see wisdom in conducting a policy of accommodation toward the domestic consumer and Western powers. *KR* obscures the fact that its author, like Stalin before him, at that time used the international situation to prove a political aim – notably, Malenkov's removal from office for blindness to the danger of war – and later adopted Malenkov's "mistaken" viewpoint as his own. Similarly, P. F. Yudin is blamed for the onset of discord in Sino-Soviet relations, although when appointed Soviet ambassador to Peking in 1953, he was the fourth envoy sent there in less than two years and obviously pursued no line of his own.

The falsehoods are sufficiently abundant to make hazardous such exercises as the drawing of lessons about Soviet perceptions of the outer world from Khrushchev's words. In particular, the margin for error in these perceptions might

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**Khrushchev Remembers,* with an Introduction, Commentary, and Notes by Edward Crankshaw. Translated and Edited by Strobe Talbott. (Boston: Little, Brown and Company, 1970, pp. 639. $12.95) and *Khrushchev Remembers. The Last Testament.* Translated and Edited bv Strobe Talbott, with a Foreword by Edward Crankshaw, and an Introduction by Jerrold L. Schechter. (Boston: Little Brown and Company, 1974. Pp. 602. $12.95).

be judged great once the untrained eye reads that

> When Stalin died, we felt terribly vulnerable. The United States was then conducting an arrogant and aggressive policy toward us, never missing a chance to demonstrate its superiority. The Americans had the Soviet Union surrounded with military bases and kept sending reconnaissance planes deep into our territory, sometimes as far as Kiev. We expected an all-out attack any day. Therefore we had no choice but to commit enormous resources to defense in order to avoid another world war. The memory of World War II was still fresh in the minds of our people. Not until we had equipped our armed forces with modern weapons could we contemplate diverting some of the defense money into other areas (II, 220).

Actually, "we" in 1954 meant the factional clique of Khrushchev, which indeed urged a buildup of Soviet military power; it did not include Malenkov, who along with some others thought differently and was at first ascendant. It would be enlightening to learn just how the tables were turned, but this process is not revealed. The complex of motives and outlooks which underlies Soviet policy making cannot therefore be appreciated if one peruses *KR* without a solid grounding in contemporary Soviet history.

KR probably gives an oversimplified account of the mechanics of power under Stalin and his successor. Some authorized memoirs published in the USSR tend to support the hypothesis of a system of court politics during the rule of Stalin. *Zapiski Ministra,* written by Stalin's commissar-minister of finance, Zverev, for example, tells of a dispute over a credit reform plan which occurred in 1941 between Zverev and N. A. Bulganin, chairman of the State Bank. Before the scheme was discussed at a meeting of the Bureau of the Council of People's Commissars, chaired by Stalin, Bulganin is said to have in effect lobbied among Politburo members for support of his viewpoint. The incident suggests that some orderly political procedure survived after Stalin rose above his colleagues.

In contrast, *KR* depicts Stalin's administration of general policy as wholly chaotic. Stalin on the eve of World War II allegedly "tried to supervise our manufacturing of munitions and mechanized equipment all by himself, with the result that no one really knew what state our arsenal was in" (I, 158). Soviet historians, however, aver that at that time Politburo members conferred with armament experts, jointly took decisions for an expansion of military industry, and in order to verify the combat readiness of the armed forces created a special commission that was headed by top party organizer Zhdanov and main technocrat Voznesenskiy. *KR* likewise claims in regard to the shaping of Soviet foreign policy in the postwar period: "Stalin had never gone out of his way to take other people's advice into account, but this was especially true after the war" (I, 361). Soviet party literature, on the other hand, states that in 1948–53 there was a permanent commission of the Politburo for questions of foreign policy. It is even possible – this ice is very thin – that Stalin in his last years had to mediate between competing power groups at the center. *KR* sheds no light on this mystery.

Khrushchev's authority inside the party command after the purge of June 1957 seems to be exaggerated. Signs of leadership instability arose at the time of the U-2 incident in May 1960, and Khrushchev in retirement told an American visitor that his power position was shaken that year. *KR*, however, shows the Chairman fully in control of events surrounding the Paris summit meeting, with colleagues acting like stage hands. *KR*'s version of the author as instigator of the policy of economizing in agriculture for the sake of bolstering military might is also dubious in view of his radical pro-farming speeches in party forums during the early 'sixties. The upshot is promotion of a questionable assumption about Soviet politics since Stalin: the effective leader has stood above policy and power conflicts at the top level rather than being involved in them.

While *KR* deserves low marks for overall novelty and reliability, it does occasionally attest to the soundness of Kremlinological theories about the real stuff of Soviet politics and the analytical method which has uncovered policy issues beneath the smooth surface of government in the USSR. For example, distribution of influence inside the ruling group has always been the overarching issue of the politics of totalitarianism. Khrushchev almost certainly wanted to become a dictator and just after his ouster a foreign Communist delegation which inquired about the event at CPSU headquarters was told that criticism was directed especially against the Chairman's "violations of the principle of collective leadership," expressed in "impulsive behavior in a number of instances, promotion of various insufficiently thought-out proposals, loss of personal modesty, and growing intolerance of criticism." (*Tiden*, No. 7–8, Copenhagen, 1964). *KR* seems to confirm the charge and admit its utmost gravity by virtue of its many strained references to such things as

Stalin's mastery of "the collective leadership" in the early 'thirties; Beria's pulling a lot of weight in "the collective leadership" at the end of that decade; and the shaping of Soviet policy during the Cuban missile crisis "from the outset" in "the collective leadership." Under a system in which the power prerogatives of highest leaders are not legally delineated, a struggle for influence is inevitable, and it follows that importance attaches to the slightest sign of disarray in the rigging of authority symbols. *KR*'s harping on the theme of leadership collectivity – or sharing of influence at the top – gives the reader every reason to believe that this venerable axiom of Soviet policy analysis is still worthy of respect.

There is considerable disagreement among scholars about the Party-State relationship in the USSR: some fail to see any real tension between these bureaucratic engines; others view the distinction as fraught with serious conflict and implications for the working of the political system. In the light of Bolshevik history, the best evidence would seem to have been the official charge that Khrushchev's rivals in 1957, concentrated in the state machine, "waged a struggle against . . . the directing role of the Party" (*Pravda,* November 12, 1958). More recently the Tass agency notified Soviet audiences that Romanian leaders had created a special bureau in their party's Executive Committee "to insure the operative coordination of Party and State activity," headed by the General Party Secretary – a hint that if anything, the stresses between party technicians of power and state managers of technology are becoming more acute in the Communist world. *KR* is informative on this score:

> . . . it has become the rule in our country for Party organizations virtually to dictate orders to farms and factories. And where do those orders originate? All too often they come from on high, from uninformed central authorities who end up doing more harm than good. . . . (II, 136)
>
> In our system, the number one man is always the Party leader. But the Party should play a strictly *political* role, and technical questions should be left to the experts. In this age of increasingly complicated technology, no political leader can keep abreast of the latest developments. As the Americans have shown us, our administrators must be professionals and specialists if we want to catch up (II, 137–138).

These remarks accord with a statement that Khrushchev made to the Party central committee in 1964, commenting on a dispute over rival methods of farm technology: "I as C.C. First Secretary and Chairman of the Council of Ministers do not wish and cannot say . . . since here we need a competition of minds, ideas and scientists" (*Pravda,* February 15, 1964). Many other times while in power, Khrushchev gave orders to or clashed with members of the technical intelligentsia. What engages our attention here is the recognition of the existence of this fundamental problem of Soviet communism, and not Khrushchev's erratic approach to its solution.

KR does not spell out at length another key aspect of the Party-State division, which is varying approaches to the choice of instruments to spur productivity. Party organizers as a rule have emphasized "moral incentives" (Communist discipline) and State experts have usually favored "material incentives" (financial reward). But this policy divergence is implicit in a caustic remark in *KR* which indicates the heat this issue generates: "I realize that by publicly advocating material incentives I'm opening myself up to those know-it-alls who will say our people should be motivated not by money but by ideological considerations" (II, 114). Communist doctrine serves to justify the predominance of Party functionaries in Soviet society, and the public statements of their highest representatives make clear whom Khrushchev was talking about. All this would scarcely require mention were it not that many American Sovietologists, unlike European counterparts, refuse to acknowledge that certain basic dilemmas of government are nowadays unique to the Soviet polity.

Similarly, *KR* on politico-military affairs provides a healthy corrective to the school of thought which focuses on the unifying trend in Soviet leadership and neglects dividing and balancing trends. In this area, as throughout the two volumes, fiction is mixed with fact. No credence can be put in the version of Marshal Zhukov striving for a military takeover in 1957; his demise was preceded by a sign of impatience to square accounts with Stalin's civilian accomplices in the military purges of 1937–38. Even Khrushchev the anti-Stalinist had to appreciate the danger raised by that sort of military activism. Nor is it likely that Marshals Konev and Sokolovski were retired in 1960 on grounds of poor health; circumstantial evidence leaves small doubt of their opposition to the ill-fated military reshuffles that Khrushchev then made. Nevertheless, *KR* illuminates the issues at stake in the Party-Army relationship and tends to support the view of commentators who see the Soviet military as playing an influential role in Kremlin decision making. It puts into sharp relief the controversy over allocation of resources between civilian and military sectors of the planned economy. Khrushchev reflected that "after fifty years of Soviet power, we're still suffering from shortages in the vital areas of meat and eggs," "the living standard of the country suffers when the budget is overloaded with allocations to unproductive branches of consumption" and "today, as yesterday, the most unproductive expenditures of all are those made on the armed forces" (II, 142 and 540). Yet, Army leaders can "intimidate" the politicians with gloomy estimates of the strategic situation and "pressure" them to increase defense spending. The military, it is implied, has a voice in setting policy on arms control and in the general conduct of foreign relations. Ultimate decision rests with the party chiefs, but they include civilian militarists, to judge from Khrushchev's rebuke of government leaders who, like the generals, "don't seem to get it into their heads that in the next war, the victor will be barely distinguishable from the vanquished. A war between the Soviet Union and the United States would almost certainly end in mutual defeat" (II, 541).

These words were very timely in 1973–74, when the Soviet Union experienced still another round of public debate over whether victory in nuclear war is possible and the exchanges of rhetoric concluded on a decidedly promilitary note. Significantly, the Brezhnev leadership in the wake of this controversy scoffed too at *KR*'s advice "not to idolize the military" and glorified that symbol of military professionalism and assertiveness, Marshal Zhukov. If *KR*'s reflections on the making of Soviet defense policy are taken seriously, these developments warrant an inference that the officer corps under Marshal Grechko was a strong political force on the Soviet scene, regardless of its constant oaths of loyalty to the hallowed principle of civilian control of the defense establishment.

The Stalin issue during Khrushchev's tenure in power had many ramifications, with divergent assessments of the old dictator's legacy reflecting policy quarrels about whether to emphasize persuasion or coercion, production or consumption, centralized direction or local initiative, rivalry or conciliation in relations with the non-Communist world. *KR*, naturally enough, avoids feeding the speculation that Khrushchev used this issue as a weapon in power struggles to discredit opponents who were more closely involved than himself in the crimes of the supreme leader. But at the root of Khrushchev's action program was the loathing of Stalinist abuses which the memoir catalogues at great length. Khrushchev's replacement of the doctrinal slogan of "the Party of the working class" by "the Party of the whole people" and his liberalizing of the conditions for Party membership point to Khrushchev's evolution from adherent to Stalinist sectarianism to almost statesmanlike figure. *KR*'s insistence that all of Stalin's victims be rehabilitated harks back to a discussion in ruling circles prior to Khrushchev's overthrow. Like Trotsky and Bukharin in the 'twenties, *Pravda* at the close of the Khrushchev period deplored hypertrophied secrecy and bureaucratic centralism as norms of political behavior. Moreover, a local party secretary for ideology in 1966 attacked "zealous critics" of Stalin who wished to resurrect the ideas associated with Bolshevik Opposition leaders who were eliminated on Stalin's order.

These crosscurrents serve to warn against a view of Soviet politics as little more than a series of power games devoid of social meaning or hair-splitting disputation over administrative forms. And lest Khrushchevism be regarded as a closed chapter in the book of Soviet history one should recall the drumfire of official criticism which has since been directed against Khrushchev. Clearly, Khrushchev's movement towards a looser variety of dictatorship was in part a response to the inner need of Soviet society for trust and cooperation between the governing political elite and other classes of the population. *KR* helps us realize that until a reconciliation of these social formations is achieved in the USSR there is likely to be recurrent debate over how far to push the Stalinist line of narrow-minded party exclusiveness and cynical manipulation of varied public interests.

Khrushchev's memoirs, however, may give an erroneous impression of his regime's break from essential continuities of the Soviet political tradition, Khrushchev's rule was a departure from Stalinism, but not from Leninism. As First Party Secretary he was not known to have given any thought to imposing constitutional resttraint on the leadership of the single-party state and its bureaucratic machinery. Bolshevism's disdain for constitutional niceties made unavoidable an immoderate use of secret police and press censorship in Russia from Lenin to Brezhnev. Despite *KR*'s polemics with Soviet "conservatives" over a host of policy issues, Khrushchev, like his inner-Party critics, was a traditionalist in upholding the fundamentals of authoritarianism inside the USSR and its European client states. Khrushchev's practicality was limited to developing methods for enlisting the cooperation of social strata which Stalin had driven to excessive passivity. In perspective, Khrushchev was a pragmatic conservative, and his more extreme rivals were orthodox conservatives; the main line of division was over method of government rather than institutional and legal framework. This way of looking at power alignments of the Khrushchev period may be more appropriate than another which posits a reformist-conservative split, largely on the basis of the rhetoric used by personalities in the Khrushchev camp.

In sum, *KR* is valuable to scholars but not to scholarship. It confirms for practitioners of the art of content analysis their assumptions about the focal points of controversy and movement of opinion in the Soviet hierarchy, as well as the effectiveness of their methodology. This is extremely important today, when the struggle for influence in Moscow is waged more secretly than it was under Khrushchev, because Soviet leaders are clearly worried about social discipline and aware that obedience to their decisions can be exacted more easily if an impression of unity "above" is given to those "below." At the same time, *KR* is so studded with factual errors and misrepresentations that no professional would venture to cite it as a source of new insight about particulars. In other words, a feeling for atmosphere and substance should not be confused with more or less hard evidence of what is really going on within the Kremlin.

Comment

GEORGE BRESLAUER
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It is a pleasure to comment on Sidney Ploss's review of Khrushchev's memoirs. Since we can now be certain that the tapes upon which these memoirs are based were in fact recorded by Nikita Khrushchev, Mr. Ploss is to be commended for having devoted his attention to questions other than authenticity. I found especially useful his citation of numerous historical inaccuracies in the account, and agree with his conclusion that we ought not "cite it as a source of new insight about particulars." I also agree with Mr. Ploss that certain issues (collectivity of leadership, Party-State relationships, material versus moral incentives, budgetary allocations, and political integration) were "focal points of controversy" in the post-Stalin years, and that these will continue to be issues in Soviet politics. Mr. Ploss uses citations from Khrushchev's memoirs as evidence that these issues were salient, and scolds those who refuse to acknowledge that they are inherent in Soviet political and social organization.

I disagree with Mr. Ploss, however, on his interpretation of the nature and import of the several key issues, and on his assessment of the utility of Khrushchev's memoirs in interpreting these issues. For example, Mr. Ploss considers the issue of Party versus state bureaucracies to be a "fundamental problem of Soviet communism" that is becoming "more acute in the Communist world." He cites Khrushchev to demonstrate the existence and acuteness of the problem, but avoids discussion of "Khrushchev's characteristically erratic approach to its solution." Yet this is the source of some of the most intriguing passages in the memoirs! *Whether or not* conflict between these bureaucracies or between their elite representatives exists, is to me an unexciting question. I take it for granted that conflicts abound.

But what are the *nature* and *implications* of this conflict? Some of it is probably nonpolitical and less acute, as in the universal tension between generalists and specialists, technocrats and politicians, and organizations with overlapping jurisdictions and competing goals. Some of the conflict, on the other hand, is certainly political and acute, a product of the distinctively Soviet pattern of Party activism and the values that often accompany it: collectivism, urgency, pressure, mobilization, and suspicion of technocracy. Whether or not the assumptions built into Party and State organizations are becoming more incompatible, and the conflict more acute, cannot be learned from reading Khrushchev's memoirs, since Nikita Sergeyevich himself was baffled by the problem during his years in office.

What these memoirs (*KR* II, in particular) do provide, however, is fascinating insight into Khrushchev's personality and the bases of his "characteristically erratic approach" to the problem. We read of his annoyance with military commanders who do not understand the difference between influence and power (*KR* II, 25–27); his satisfaction with the technical expert who knows his field but exercises proper humility and respect in the presence of politicians (*KR* II, 42); his hesitation about intervening in the solution of technical problems, but his impatience with inertia (*KR* II, 49). We are also treated to Khrushchev's distrust of official statistics and his search for out-of-channels sources of information (*KR* II, 130–131; 139). And we are witness to a fascinating series of "confessions" on Khrushchev's part – last rites before his impending death? – as he apologizes to Kapitsa (*KR* II, 68), Pasternak (II, 77), Neizvestny (II, 80), and unnamed Soviet architects (II, 98) for having imposed his will upon them.

All of this presents the image of a Soviet leader who was *profoundly ambivalent* about the boundaries between political and technical questions, and about the propriety of using certain measures for enforcing compliance. The quotation offered by Mr. Ploss, as he himself has indicated, is not representative of Khrushchev's behavior while in power. The picture of Khrushchev's ambivalence that is painted by the memoirs suggests that he never fully explored and understood the interrelationships among his policies.

Mr. Ploss also cites the issue of material versus moral incentives as a "basic dilemma of government . . . nowadays unique to the Soviet polity." I am not persuaded. All regimes have to deal with the problem of spurring productivity and civic contribution. Few systems would flourish for long if they elicited purely instrumental, cash-based involvement on the part of their citizenry. Whether we call it civic pride, public duty, patriotism, or some other source of inspiration, the problem of finding a mix of moral and material incentives is hardly *unique* to the Soviet polity. What *is* unique is the way in which the Soviet authorities define and handle the problem. Clearly, Soviet values embody inner conflicts between austerity and consumerism. But they also reflect political forces and bureaucratic interests that clash over the issues of individual versus collective rewards, and economic efficiency versus social equality. Regime policy has changed frequently over time to deal with the tradeoffs that are inherent in reconciling achievement and equality, differentiation and social integration, and Khrushchev played a role in some of these changes.

Once again, the memoirs present a picture of Khrushchev struggling with these tradeoffs. We see the strong meritocratic strand in his appeal for peasants to receive more money for higher productivity (*KR* II, 114), and in his criticism of guaranteed salaries for administrators and government researchers (*KR* II, 114, 117). At the same time, we see him trying to recreate the drama of a social movement, by appealing to Soviet youth to go out to the Virgin Lands, in the spirit of "the days of hardship and sacrifice during the war" (*KR* II, 122). And we see his egalitarian streak in strained competition with the meritocratic: "I've never been in favor of reducing everybody and everything to the same level, but at the same time I'm against discrimination. . . . I believe in the rational distribution of wealth, so that there will be neither too much nor too little difference in the incomes of various categories of workers" (*KR* II, 144).

There is no simple resolution of this conflict, so Khrushchev's ambivalence should be of little surprise. Complex bonus schemes for individual or collective reward, degrees of differentiation between social strata, transfer payments – all are combined in a mix of various moral and material incentives to contribute. Soviet political discourse on this issue, and Khrushchev's position, were far more complex than is suggested by Mr. Ploss's treatment.

I am in substantial agreement with Mr. Ploss on his overall evaluation of Khrushchev as an "almost statesmanlike" historical figure. Khrushchev brought the Soviet Union out of the rigidification of late Stalinism, and offered political and ideological innovations that were to break from Stalin's methods of government. At the same time, he was not a constitutionalist or a liberal democrat, and he supported the basic institutional framework developed under Lenin and Stalin. I could hardly agree more with the assertions that "Khrushchev's movement towards a looser variety of dictatorship was in part a response to the inner need of Soviet society for trust and cooperation between the governing political elite and other classes of the population," and that Khrushchev sought new "methods for enlisting the cooperation of social strata which Stalin had driven to excessive passivity." Indeed, I have argued precisely this at length in my own work.[1]

The issue of trust is, to me, a crucial one. The Stalinist ethos viewed all unregulated initiative as bordering on anarchy, and this created an atmosphere of profound distrust of the masses. That Khrushchev, who was raised in this system, was willing to take risks and to argue for greater "trust" in mass initiative in order to break through the stultifying atmosphere of late Stalinism, is, I believe, one of his great contributions to recent Soviet history. (The call for Soviet officials to exercise greater "trust" runs through the speeches Khrushchev made while in power, and it is also in evidence in his memoirs – see, for example, *KR* I, 76, 112, 295, 307; and *KR* II, 60.) And Khrushchev went even farther. He recognized that effective stimulation of genuine initiative required reducing the social distance between officials and masses by decreasing inequalities of wealth, abrogating certain privileges, changing recruitment policies, and urging citizens to criticize their hierarchical superiors. Khrushchev may not have been a constitutionalist, but he did try to offer the Soviet masses greater material and physical security than they had ever enjoyed, and he developed some interesting ideas about methods of mobilization that I

[1] George Breslauer, "Leadership and Adaptation in the Soviet Union Since Stalin" (paper presented to the AAASS Conference on CPSU Adaptation, New Hampshire, June 1974). Since this exchange with Mr. Ploss was written, I have published an interpretation of Khrushchev's regime: "Khrushchev Reconsidered," *Problems of Communism,* 25 (September–October 1976), 18–33.

consider impressive in light of his Stalinist background.

Yet, whether or not we are impressed by Khrushchev depends on our perspective: do we stress how far he came in the evolution of his thinking, or do we emphasize the limits of his evolution? Should we be impressed by his "statesmanlike" behavior as a "Stalinist turned reformer"?[2] Or should we concentrate upon his limitations as a "visionary imprisoned by his past"?[3] The answer will largely hinge upon what one expects from a leader and how one defines "statesmanlike" leadership. This requires, in turn, that we examine the opportunities and constraints facing leaders in different realms of policy, and the ways in which they have dealt with them. And because of the complexity of such an exercise, I do not find very useful Ploss's global characterization of Khrushchev as a "pragmatic conservative," although Khrushchev was certainly pragmatic in some respects and conservative in others. But, except for rigid dogmatists and radical reformists, who isn't? How would we characterize the current Soviet regime, using these labels?

Mr. Ploss also underestimates the utility of Khrushchev's memoirs for gaining some preliminary insight into the sources of Khrushchev's novel ideas. Was Khrushchev's frustrating experience with the central bureaucracy while in the Ukraine a formative experience that shaped his subsequent calls for regionalization and limited decentralization (cf. *KR* II, 115–116)? Was his role in supervising the reconstruction of Moscow in the 1930s a crucial experience that defined his subsequent image of the ideal relationship between leaders and led (cf. *KR* I, 57)? Did his involvement on the local fronts in the Ukraine, in Moscow, and in World War II combine to inspire in him a basic faith in "the people," and a greater trust in their initiative than that held by central bureaucrats (cf. *KR* I, 76; 112)? Were these the cognitive bases for his later attacks on Party exclusiveness, bureaucratic layering, abstract ideological discussions, and administrative methods? Did all these influence his subsequent advocacy, at the 22nd Party Congress, of an increased, and more authentic, role for the Party *aktiv* as a mobilizing agent? These strike me as fascinating questions, terribly difficult to answer, but capable of being partially addressed through careful use of the testimony in the memoirs, and with full awareness of the limitations of these memoirs as a source.

Let me lock horns with Mr. Ploss on one final issue – Soviet defense expenditures. Kremlinologists may be excited by the prospect of another Soviet "public debate" about the possibility of victory in a nuclear war, especially if it ended "on a decidedly promilitary note." But what are we to make of this assertion? It is worth bearing in mind that discussion in Soviet journals has been quite heterogeneous over the past fifteen years, making it difficult to know just how much a given statement reflects Politburo opinion. Surely there are military officers who publish articles, especially around appropriations time, implying that victory in nuclear war is possible. But this opinion has hardly defined the regime concensus during the past fifteen years, and I would be very surprised if this new "debate" ended with an editorial in a major Party journal affirming the militarist position. If it did, I would be the first to acknowledge that this is news.

In sum, there is much with which I agree and much with which I disagree in Mr. Ploss's review. Overall, I have argued in this "Comment" that Khrushchev's memoirs can be used, despite their inaccuracies, to help us overcome simplistic images and labels about the issues, dynamics, and personalities of Soviet politics.

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[2] Carl A. Linden, "Stalinist Turned Reformer," *Problems of Communism,* 20 (September–October 1971), 48–51.

[3] Edward Crankshaw, *Khrushchev: A Career* (New York: The Viking Press, 1966).

Rejoinder

SIDNEY I. PLOSS
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George Breslauer's comments are important because they serve to caution against the indiscriminate use of *KR* as an historical source. The comments illuminate, too, some of the major differences of analytical perspective among students of Soviet politics.

Unlike Mr. Breslauer, I take nothing for granted about Soviet bureaucratic relationships and assume essential harmony unless there is evidence to the contrary. Since *KR* does provide such evidence about Party-State relations, I felt that it merited citation for the benefit of those who value research as a basis for theory. Viewed from that angle, the question of this conflict's existence is not "unexciting." I would add that it is futile to try to disentangle the "nonpolitical" from "political" in the context of intrigue at the top of a totalitarian society.

Dubious of psychologizing from afar and assertions that Khrushchev was a full-fledged dictator, I attach less weight to Khrushchev's "ambivalence." In the main, I surmise that when Khrushchev had the upper hand in Moscow's inner councils his latest idea became law; and when his fortunes declined, the experts gained greater control. Certainly, the feelings and attitudes of men do count for something in Soviet politics, but the balance of forces in the ruling clique strikes me as more significant than *ad hoc* ruminations about problems of governance. In other words, Khrushchev as Chairman was probably not the mindless activist that Mr. Breslauer suggests he was, but someone who had to operate and compromise occasionally in a framework of shifting majorities within the Party command. As for the "cognitive bases" of Khrushchev's relative pragmatism, Brezhnev assuredly experienced many of the same frustrations at the grass roots as did Khrushchev, but Brezhnev later proved to be much less innovative in the sphere of domestic policy. What may be most "fascinating" is not necessarily a key determinant of factional behavior on the Soviet scene.

The boundless comparativism which afflicts our political studies is exemplified by Mr. Breslauer's statements that "all regimes have to deal with the problem of spurring productivity and civic contribution" and that "the problem of finding a mix of moral and material incentives is hardly *unique* to the Soviet polity." Quantitative differences have a way of becoming qualitative ones. The General Secretary of the CPSU Central Committee has doubtless met with Politburo colleagues many more times than the U.S. President has with Cabinet members in order to reconcile divergent attitudes about how best to combine the people's selfish and altruistic impulses. A single minting of WIN buttons and dash of White House exhortation should not be confused with decades of Socialist Competitions backed up by the full force of Party and Cheka. Notwithstanding the "complexity" and hidden "tradeoffs," it seems clear that an overprivileged elite in the USSR has since Stalin continued to deny its subjects the fruits of their labor for the sake of realizing great-power ambitions. An upper-class appreciation of the resentment this harsh policy course evoked in the Soviet population – and not the "Stalinist ethos" – would seem to have accounted for that "fear of the masses" Khrushchev claimed to deplore in the bureaucracy during his reign. Once more, the pitfalls of stylish comparativism are rather manifest.

While abhorring "simplistic images and labels," Mr. Breslauer does not hesitate to frown upon "Kremlinologists" who, it seems, are the only ones keenly interested in Soviet controversies over defense policy. No, I cannot produce an editorial from a CPSU journal endorsing the militarist view – as if those editorials were virtual stone tablets cast down from the Soviet Union's ruling heights! But I can point to an expression of toughness by Army General V. G. Kulikov, then chief of the USSR Armed Forces' General Staff, in *Pravda* (November 13, 1974) and suggest that it be examined in the light of a less bellicose pronouncement by, say, G. A. Arbatov, director of the USSR Academy's U.S.A. Institute, in *Problemy Mira i Sotsializma* (No. 2, 1974). Given the sensitivity of the resource allocations issue and laws of political patronage in Moscow, these conflicting opinions on "theory" which is related to defense outlays were likely to have caused a stir at the Politburo. And eventually that sort of thing does become news, if only in the naming of a civilian Defense Minister, as happened not too long after the death of Marshal Grechko in April 1976 and Kulikov's demotion in January 1977.

BOOK REVIEWS

The Crimes of Politics: Political Dimensions of Criminal Justice. By Francis Allen. (Cambridge: Harvard University Press, 1974. Pp. 105. $6.95.)

". . . [T]he twentieth century will be recalled as, perhaps preeminently, an age of politics" (p. 1). With this observation Francis Allen begins his study of political crimes. If Professor Allen is correct, if this is an age in which political events have captured the imagination of the public, in which the mass media have transformed politics into a spectator sport with a national audience, and in which the language and metaphors of politics are used to explain a broad range of human experience, then perhaps it is not surprising that lawyers interested in the problem of crime should begin to perceive its political dimensions. Allen's book is itself evidence of the popularity of political explanations and political perspectives.

Allen identifies the decades of the 1960s as the period in which political explanations first found their way into the study of crime. He suggests that at the start of the 'sixties what we now take as a given – namely, that criminal law is an important area in which government exercises its power and in which competing political forces seek to legitimize their values – was neither a given nor particularly well understood. Allen accepts, although not without some hesitation, the marriage of political science and criminology. The source of his discomfort lies in the ideological nature of this marriage. He argues that studying criminal law and the administration of criminal justice as tools of the powerful has an impact on the way people respond to both. Such a view justifies social protest and further politicization of the criminal process. It is this development which Allen fears. He is, for all his analysis and sympathy, still a believer in the law, a believer in the value of law itself. To recognize the instrumental relationship of law and politics is to remove from law its primary claim to legitimacy and thus to weaken it.

Turning from the way in which crime is studied to the substance of the criminal law, Allen argues that American law, unlike its European counterparts, recognizes no concept of political crime. In this use Allen means crimes motivated by a desire to protest some action or policy of government. Instead of a formalized category of political offenses, Americans have the first amendment. Instead of defining certain political acts as criminal and moderating the applicable penalties, our law insures their fullest protection. Allen also argues that, in spite of a widespread suspicion of government, it is not natural for Americans to view government as an enemy. This distinction among cultural attitudes is neither particularly clear nor particularly useful as an explanation for the absence of a doctrinally developed conception of political crimes. Allen himself acknowledges that the very concept "political crime" is imprecise. It is used to characterize the conduct and motives of individual criminals, to identify certain acts, e.g., treason, and to describe the motivation for particular prosecutions. Allen shifts among these meanings in an unpredictable manner and in so doing detracts from the clarity of his argument.

No matter how political crime is perceived or defined, its impact on the administration of criminal justice is deleterious. In spite of certain compelling short-range uses, for example as a technique for disrupting and harassing political opponents, politically motivated prosecutions, according to Allen, are generally more trouble than they are worth: Political prosecutions occur most frequently in periods of social upheaval. They serve to divert attention from the real causes of the upheaval and in so doing weaken society's capacity to deal realistically with its problems. Furthermore, political prosecutions often result in an abusive use of law enforcement powers, powers which once extended in the service of such prosecutions are difficult to restrict in other areas. What is most troubling to Allen is not the prosecutions themselves but the way in which the practices employed in handling political crimes infect practices employed in other aspects of the criminal law and thus threaten the integrity of the criminal justice system. Here Allen is true to his belief in law. His perspective and position is that of defender of the institutions of law; he is not a friend to the accused. Allen ignores the possibility that politically motivated prosecutions are simply wrong, no matter what their impact on the criminal justice system. So long as the criminal justice system is not weakened or embarrassed, as so often occurs in dealing with political crimes, Allen is prepared to accept if not endorse the political uses of the criminal law. In fact, the real danger is that politically motivated prosecutions undermine the values which make a democratic legal system worthwhile in the first place.

In spite of, or perhaps because of, its brevity, *The Crimes of Politics* is worth reading. It represents an interesting, if not wholly satisfying, attempt to deal with developments which have revolutionized the study of crime and the criminal justice system. It stands as testimony to the importance of politics and to the importance of political science perspectives on law.

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The Essential Comte. Edited and with an Introduction by Stanislav Andreski. Translated and annotated by Margaret Clarke. (London: Croom Helm and New York: Barnes and Noble Books, 1974. Pp. 252. $18.75, cloth; $5.25, paper.)

The hopes raised by this volume, which presents itself as offering "The Essential Comte" are destined to be somewhat disappointed: what is offered here is simply a number of selections from the *Cours de Philosophie Positive*, available since 1853 in Harriet Martineau's translation, and whose first two chapters (reproduced here) are readily accessible in Frederick Férre's translation in paperback. For a number of reasons I find myself out of sympathy with the principle of selection adopted here, and the quality of the translation also leaves much to be desired.

Any translator of Comte has a strong claim on our sympathy and indulgence. An execrable stylist, incapable of concision let alone elegance, Comte commanded a prose elephantine in its cumbersomeness, his paragraphs tended to run to several pages, and hardly a sentence was not freighted to breaking-point with asides and subclauses. He loaded his nouns with adjectives and was apparently unwilling to let a verb issue from his pen without attaching an adverb. The translator has decided to dispense with a large proportion of such adjectives, adverbs, and even asides and subclauses.

Now, one might be prepared to sacrifice literalness to readability. But what we get here is neither very readable nor accurate. Comte's tiresome iteration on every page of *essentiel, fondamental, approfondi, élevé, actif, réel, rationnel, logique, principal, spontané* (whatever that means) and their corresponding adverbs, adds nothing to Comte's sense, as the translator rightly remarks; these are indeed mere pleonasms. But such stylistic vices mirror the vices of Comte's mind (on which subject Professor Andreski's introduction is eloquent), and they should all have been reproduced, in my view. And even if license were granted in this matter, there is no excuse for the omission of the two most overworked adjectives in Comte's vocabulary, *nécessaire* and *inévitable*, which he meant quite seriously.

In general, the translation is unsatisfactory as regards both accuracy and readability. The English text is not easy to follow (I cannot speak with confidence, having read the translation with the original open in front of me). There are many gauche expressions and some quite unintelligible ones. There is evidence of a lack of care in revision, and even some schoolboy howlers – like *ancien* before a noun being translated as "ancient." There are gratuitous embellishments, such as the repeated translation of *enfance* by "infantilism," and not infrequently the translation misses the point altogether.

The selections are prefaced by an all too brief introduction from Professor Andreski. His style (remarkable for a sociologist by reason of its crispness and its entire freedom from jargon) is the only agreeable reading in the whole book, and his points are lucidly made.

I could have wished, however, that this introduction had devoted less space to the curious circumstances of Comte's life, and to somewhat gratuitous (though welcome) lambasting of some of Professor Andreski's bugbears, and rather more space to the elucidation of the principles of selection employed. I do not find in this introduction a convincing substantiation of the claim made by the title of the book, that what is presented here is the *essential* Comte. For my part, as a student of political and social thought, for example, I find the essential Comte in the Comte of the *Politique Positive* and the *Discours sur l'Ensemble du Positivisme*, who puts in an appearance here only insofar as the theme of the propagation of the positive spirit is common to all his works.

Professor Andreski's selection is evidently guided by his judgment, supported in the introduction, that what Comte has contributed to sociology is the idea that society should be studied in a positive, that is scientific, manner. I regret this habit, often evinced by sociologists, of treating the founding fathers of sociology as "contributors" to a current enterprise. More might be gained by using the opportunity afforded by historical reflection to gain a critical distance from that enterprise by viewing it in the process of its coming to be, rather than in retrospect. No doubt Comte's methodological thought is essential to his work, but as the introduction itself says, the idea of applying the scientific method to the study of society was an eighteenth-century commonplace on which Comte simply embroidered, and with the thread borrowed from Saint-Simon at that. Better service would have been done to the reader, I think, by a greater concentration on Comte's substantive writings on social statics and social dynamics, which are in any case less indigestible than the merciless repetitiveness of his "methodological" and "philosophical" musings.

A protest must also be entered about the failure of this book to tell us what proportion of the original is here represented. On p. 116, for example, there is sequence of unconnected paragraphs, the lack of connection becoming intelligible when it is realized that some 130 pages of the French original have been omitted without any sort of acknowledgment. Nor is it possible to tell from Comte's outline of the *Cours*, appended to this translation, how much of the original has been reproduced, how much omitted, and how much condensed.

In summary, then, a defensible, but not I think entirely prudent or satisfactory selection of texts, and a questionable translation.

H. M. HÖPFL

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Max Weber and the Theory of Modern Politics. By David Beetham. (Atlantic Highlands, N.J.: The Humanities Press, 1974. Pp. 287. $14.75.)

The reader will be disappointed if he picks up this book expecting from the title to find an assessment of Weber's contribution to an authentic, integrated theory of modern politics, or an evaluation of his place in the contemporary state of political theory and science. Both aims lie far beyond the scope of the study. In a fascinatingly negative way, the work is a mine of data and ideas for one who is looking for reasons: (a) to explain the state of the field as at least partly due to Weber's influence, (b) to minimize Weber's standing as a political theorist, notwithstanding the author's explicit recognition of his subject's "undoubted brilliance as a scholar and thinker" (p. 276).

Mr. Beetham achieves this reductive effect, intended or not, by centering his attention upon, and *identifying as political*, the so-called "practical" writings. These consist of Weber's public policy studies, newspaper and journal articles, speeches and papers frankly prepared to influence public attitudes and group or official thinking. According to the author, these writings are to be understood and evaluated on a different basis from the historical, comparative, systematic writings on religion, capitalism, bureaucratic and social systems, and scientific methodology. The latter are to be evaluated by the canons and perspectives of the scientific community – in particular of "academic sociology" (pp. 30–31). Thus cutting through the academic *methodenstreit* in the burgeoning Weber literature of the last twenty years, with which he is obviously familiar (chapter 1), he proceeds to write a beautifully succinct second chapter, uncluttered by any self-examination or discussion of alternative theories of political evaluation. This sums up Weber, the political writer and actor, as "protagonist of bourgeois values – nationalist, liberal, elitist" (p. 55).

If locating Weber on the political spectrum in ideological terms is all the reader is interested in, he can stop right there. But Beetham knows very well that the relation between Weber's scientific-academic work and his policy writings is far more complex than can be explained on the basis of a simple division between two types of communication and their correlative judging audiences. He devotes the main body of his study (chapters 3–8) to showing how Weber applied *analytic-empirical concepts* and methods to identify and explain controversial problems of his day. These chapters constitute a welcome addition to the critical literature available in English on Weber's application of his analytical ideas to practical political problems.

Among the problems thus selected and elaborated are (italics mine):

(1) the *limits* of the ideal-types of bureaucratic and capitalistic rationality,

(2) the *weaknesses* and *variations* of the assumed linkages between parliamentarism and democracy,

(3) the *differential consequences* of conflicting conceptions of nation, race and class for achieving organized group and party objectives in Germany and Russia,

(4) the *institutional effects* of the struggle and balance between different theories of political leadership – plebiscitary, autocratic, bureaucratic – again primarily in their German and Russian settings,

(5) the *moral-intellectual ambiguities* confronting the political writer who attributes historical or scientific correctness to particular mass-elite doctrines and action-strategies in the midst of the political struggle (not treated as a separate category).

Only one general conclusion is drawn from the "empirical" chapters, and the author labels that "not very original or startling." He finds that Weber on the whole maintained a different analytical stance, asked different kinds of questions, and applied different modes of analysis in his practical, as compared with his systematic-theoretical writings, and that "the full implications of these differences have never been adequately explored" (p. 261). Beyond specifying what he means by "different," Beetham gives no empirical tests for this finding, which many social scientists, in my opinion, would find both interesting and controversial. It is exactly for this reason, one suspects, Beetham is inhibited from recognizing how much the political and social sciences of today are indebted to Weber for the degree to which since 1890–1920 they have become more self-critical, more reflective, more analytical than merely positivist, more insistent upon the distinction between theoretical-scientific and practical-policy knowledge.

The conclusion of which the author seems most proud is that, as an historical and scientific sociologist, Weber committed "ideological errors." At specifiable points, particularly in Weber's treatment of democracy and capitalism (the generalization does not extend to the whole of the systematic-scientific work), Beetham tells us that Weber's conscious-unconscious need to support the value-syndrome expressed in the political writings led him to make assertions of sometimes universal, sometimes causal necessity that are conceptually distorted, one-sided, incomplete, or just not necessarily so. Further, the author argues that some of Weber's propositions cannot be defended on objectively scientific grounds, while others can be shown in specific cases to be logically refutable. The author makes these methodological debating points skillfully (pp. 264–275), but textual analysts who do not share his persuasion must decide the substantive weight to be given an appraisal of *The Protestant Ethic* and *Economy and Society* on such grounds, particularly when by his own criterion of selection they should have been excluded from his appraisal.

The unsettled questions left by the book are whether modern political theory does or does not, should or should not, judge Weber's work on the basis of either the practical or the academic writings. The author might have said these are *its* dilemmas, but for his own good reasons he refrains from doing so. His dialectical position is that the "practical" criteria of the citizen-politician's world, or an ideological segment thereof, must be controlling. If I may employ a strained analogy, would it be appropriate to appraise Plato on the basis of *The Statesman* and *The Laws*, and then find that portions of *The Republic* are ideologically incorrect? Alternatively, would not an adequate modern evaluation have to start from the position that Weber acted politically not as one personality, but in at least three, perhaps four, roles: as theoretical system-builder, social scientist, policy analyst, and public action-mover?

AVERY LEISERSON

Vanderbilt University

Greedy Institutions: Patterns of Undivided Commitment. By Lewis A. Coser. (New York: The Free Press, 1974. Pp. 166. $7.95.)

Greedy institutions "make total claims on their members and . . . attempt to encompass within their circle the whole personality . . . they seek exclusive and undivided loyalty" (p. 4). They are not, however, "total institutions" (in Erving Goffman's sense) because, instead of relying on physical force, they usually prefer "non-physical mechanisms to separate the insider from the outsider" (p. 6). As against both of them, Coser's own commitment is squarely to political pluralism. His concept of "greedy institution" is deliberately evaluative. There is no value-neutrality here on the part of a distinguished liberal member of the profession, who has been fighting the old and new extreme left for many years. There are echoes of this struggle in this collection of ten essays, half of which have been published previously and one of which goes back two decades to an attack on Leninist and Trotskyist sectarians in *Dissent* magazine.

Coser wants his work to be understood as "largely exploratory" (p. 17). A polished essayist in Simmel's tradition, he must not be expected to present a systematic contribution to organization theory. Instead, he enjoys roaming far afield through the centuries. He obviously had great fun drawing nine vignettes on eunuchs in the old empires of the Far and Near East, eighteenth-century Court Jews and French royal mistresses, sixteenth-century Janissaries and Jesuits, nineteenth-century domestic servants and American utopians from Oneida to the Shakers, and twentieth-century Trotskyists, Leninists, and housewives. What do they all seem to have in common? All are said to be objects of greedy institutions. Faithful to his intellectual career as reconciler of a functionalist and a conflict approach, Coser has a sharp eye for the benefits and costs of social arrangements, and skillfully practices the sociologist's trade of revealing ironies, paradoxes, and unintended consequences by identifying celibacy, eunuchism, and promiscuity as functional equivalents that fulfill essentially similar social functions – maximizing allegiance to, and dependence on, the ruler.

From the outset Coser is generous in acknowledging his intellectual debts to Simmel, Max Weber, Caillois and his colleagues. One must ask, however, whether the theoretical legacies acknowledged are successfully integrated under the rather extended umbrella of the "general concept of the greedy institution" (p. 17). The greedy institution is an aspect of all organizational efforts at mobilizing the intellectual and emotional resources of actors in the face of competing claims. As such this "general concept" has no historical identity, in contrast to the series of ideal types (or sociohistorical models) that Coser subsumes under his general concept and that he assembles properly "from very different historical periods and very different cultural traditions" (p. 136).

Coser divides his general concept and his types into three substantive parts. Part I deals with dominant political systems and the ways in which eunuchs, aliens, Court Jews, Christian renegades, and royal mistresses served the ruler. Part II treats domestic servants as a largely extinct species and present-day housewives as the great, and more or less silently suffering, majority of women. Part III concerns itself with political and religions collectives, either countercultural groups of a revolutionary or pacifist nature, from nineteenth-century American utopians to Leninists, or celibate priests and militant Jesuits as servants of the Church in her struggle with state, family, and religious opposition.

Parts I and III offer pertinent illustrations to Weber's political sociology of patrimonial and charismatic rulership, especially of the ways in which the patrimonial ruler utilizes persons outside or at the lower rungs of the status hierarchy, and of the manner in which sects and other charismatic communities control their members. Yet in spite of his frank acknowledgment, Coser asserts that Weber did "not specifically [deal] with aliens as servants of rule" (p. 10), although he in fact did deal explicitly with this practice (cf. Weber's *Economy and Society,* 1968, pp. 1015, 1018, 1043). This may be a mere slip, but it may also be indicative of the general tendency of sociologists to see Weber mainly as the student of bureaucracy and of the Protestant ethic. Significantly, Coser speaks of "Weber's Calvinist" (p. 45), when in his own context he could just as well have spoken of "Weber's patrimonial ruler," but even the term "patrimonial" does not seem to appear in the text. In my judgment, by failing to fully take advantage of Weber's comparative constitutional analysis and complex typology, Coser pays a price in reduced theoretical explicitness.

Coser encourages the reader to think of his own contemporary illustrations for greedy institutions, and these might range from the White House staff as a patrimonial apparatus to countercultural charismatic organizations such as the Weather Underground or the SLA. One question in my mind is whether Coser's own inclusion of the present-day nuclear family is really an appropriate case. Certainly, the extended family was once a patriarchal rulership, as is demonstrated by the chapter on the growing obsolescence of domestic servants. But today the housewife appears as the victim of what in an obvious sense is the most greedy institution of all, the nuclear family. Yet its greediness is different from the ruler's political greediness or the greedy hostility of collectives against dyadic relationships. The nuclear family is, after all, also a most important basis of resistance to the other two manifestations of greediness and can mobilize its members against eunuchs and celibates, Jesuits and Leninists. I found Coser's chapter (co-authored with his wife) on the housewife and her greedy family the weakest part of the collection. In contrast to the other essays, almost no paradoxes, ironies or unintended consequences are revealed. Everything said in the chapter is true enough and standard Women Liberation's fare. But just because the contemporary nuclear family is no longer a form of domination of women and children in the patriarchal mode, everything depends on the psychological balance of power between husband and wife (and children). There usually is some psychological inequality, but the point is exactly that this can go both ways. Strikingly, the authors seem to me to misinterpret E. A. Ross's Law of Personal Exploitation by thinking only of the abuse of the wife, when this "law" can clearly apply both ways: "In any sentimental relation the one who cares less can exploit the one who cares more" (p. 91). But that is no longer a matter of institutional greediness, and it is not even clear to me who is the greedy one in such a relationship.

GUENTHER ROTH

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The March of Socialism. By J. Alvarez del Vayo. (New York: Hill and Wang, 1974. Pp. 426. $12.50.)

In Senor del Vayo's lengthy *confessio fidei*, socialism marches from Spartacus to Amilcar Cabral. The author sets the tone at the beginning of his introduction: "This is not a history book. Its author is not a historian – neither by temperament nor by training. It is, rather, a political action, undertaken by a Socialist who believes in socialism. As you will see from the following pages, there have been far too many Socialists to whom socialism was nothing but a label or a mere party card." After a brief glance at Spartacus and the Middle Ages, Senor del Vayo has chapters on 1789, 1848, and 1871 before moving solidly into the twentieth century with discussions of the Russian revolution of 1917, the operation of successive Internationals, the Spanish Civil War, the Chinese revolution, and contemporary guerrilla movements.

Senior del Vayo's qualifications for writing such a work are formidable. The statement in the book's dustjacket that he was active in the first as well as the second International is indeed inaccurate, but the spirit is quite correct: the author is a veteran campaigner. He was the foreign minister of Republican Spain in its civil war and his country's chief spokesman at the League of Nations. He was foreign correspondent for *The Nation* for twelve years and continues to be an active leader of the anti-Franco movement. And he makes the grandiose claim "to have personally known virtually every important Socialist and Communist leader in the world" (p. 4).

All this is quite promising, but the author himself indicates the weaknesses as well as the strengths of his book in his rather curious statement that "the events are here either studied in depth or personally lived through." Why not both? The events that are more or less part of the author's own experience have the immediate sort of interest characteristic of autobiography: his impressions of Socialist leaders, his memories of the Spanish Civil War and his long section on China. At the same time, Senor del Vayo is constantly pouring scorn on people he refers to as "academics" and "experts." And we can see why. For when he strays from the strictly personal, he is unsteady, both in the detail and in the interpretation. As for detail, if the author's romanticized inaccuracies about Marx are representative of the whole, then the reader had better beware. As for the interpretation, there is an uncritical "orthodox" Communist approach to everything: an adulation of Lenin and even of Stalin; the grisly Stalinist repressions of the 1930s dismissed airily in a line; not a word on the Molotov-Ribbentrop pact; no attempt to give even the briefest presentation of Trotsky's ideas. Senor del Vayo is by present profession a journalist, and some sections of his book remind one of the anti-Communist Cold War journalism of the 1950s – but from the opposite side. Nevertheless, he can write very readably and this comes through all the more vividly in the interesting passages that represent what was "personally lived through" rather than the "studies in depth." And it is for these personal reminiscences that the book is to be recommended.

DAVID MCLELLAN

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The Unbounded Frame: Freedom and Community in Nineteenth Century American Utopianism. By Michael Fellman. (Westport, Conn.: Greenwood Press, 1973. Pp. xx, 203. $10.00.)

Michael Fellman builds his sketches of various American utopian thinkers around the problematic relationship between freedom and community and he provides us with at least two valuable insights into his subject. First, this account points up what is, at least on the surface, an incredible variety of American thinking about utopia, encompassing the simple individualism of Margaret Fuller, the authoritarianism of John Humphrey Noyes and the moral elitism of George W. Curtis. Second, Professor Fellman invites us to understand utopianism through reference to the historical context in which it developed, that is, as a product of those early nineteenth-century feelings of open and unlimited prospects for social invention that marked the American experience. By virtue of the wide range of their imaginations and their resistance to the fetters of their own culture, there may be a temptation to examine utopian thinkers outside the bounds of intellectual history, but the author is effective in showing the importance of historical setting to the development of this type of social thought.

Beyond this, however, the author chooses to engage in a close analysis of each thinker and to forego a penetrating interpretation of the whole movement. This is a legitimate choice but probably a difficult and unfortunate one, given how much there is here to explain and understand. For example, it should have been worth exploring how this American tradition of social thinking was so decidedly antipolitical, one characteristic which seems to bind together an otherwise diverse group of individuals. Though the search for the right mix of freedom and community operated on a moral, religious, and psychological level, and even touched the level of institutional reform (as with George W. Curtis and Ignatius Donnelly), there was lacking any explicitly political expression of utopian imagination which might have taken the form of active public appeal and political mobilization. The desire to extend one's personally discovered vision was greater than any drive to translate utopian insights into some popularly comprehensible political terms. Some may wish to see in this an explanation for the consistent personal frustration that runs through these accounts and is so vividly portrayed in the description of Noyes spending his last years, after his expulsion from the Oneida community which he had founded, rocking in his chair as he looks out at Niagara Falls.

There is also the matter of how we are to make sense of these individuals in the context of our larger social and political experience. There is good reason to want to think of them as extraordinary figures giving voice to very special kinds of historical alternatives. Any touch of the heroic, however, is cancelled out by an altogether opposite impression which Fellman's analysis supports, perhaps unconsciously. In reading this book, one begins to feel that these are very typical Americans, or that they represent what being "typically American" has become. Even the seemingly exotic Isaac Hecker was, like Orestes Brownson, one of those moral and theological itinerants that represent a now conventional pattern of religious seeking in a society of melting pot religiosity and belief derived from choice. As de Tocqueville appreciated, the notion of indefinite human perfectibility and the independent, footloose exploration of beliefs have become characteristically American. Thus, in a strange and ironic way, if we have fulfilled the dreams of the utopians, it may be by having absorbed into the mainstream of American culture some of their continual seeking and resisting settlement.

Fellman does not formally engage these issues, and the preconceived theme of "freedom and community" heavily dominates as the sole theoretical anchor. We have, then, a readable and informative review of a number of utopian figures in which the historical approach is successfully appealed to but left unfulfilled. The author sees this era of social imagination coming to a close toward the end of the century and turning into progressivism, yet the reader is really left to ponder how such thinking has lived on and touched the broader dimension of our historical experience.

WILLIAM J. MEYER

The University of Michigan-Flint

Representatives, Roll Calls, and Constituencies. By Morris P. Fiorina. (Lexington, Mass.: Lexington Books, 1974. Pp. 143. $13.00.)

Professor Fiorina's book is primarily an effort at formal or analytical political theory that addresses a number of long-standing questions about the impact of constituency on legislative voting. The book will be of interest to a number of different audiences. For the student of legislative behavior most comfortable with empirical analysis, the first chapter of the book is must reading. Here the author discusses the often confusing body of literature on party vs. constituency influence on legislative voting and manages to bring some semblance of order to the welter of empirical findings. Particularly insightful is the author's analysis of the impact of electoral marginality-safety on the policy positions (extreme vs. moderate) taken by the legislator. Fiorina convincingly argues that existing studies which seem to indicate that representatives from marginal districts take moderate policy positions are misleading because they confuse the policy arena of the legislature with the policy arena of the constituency.

Thus, the author develops a formal theoretical structure in order to investigate the relationship between district preferences and the representative's voting. He distinguishes between two types of representatives – maximizers, whose primary goal is to maximize their probability of re-election (i.e., maximize the number of votes they receive) and maintainers, who chiefly seek to insure their re-election at some specified probability level but can be influenced directly by other concerns once that level is reached. Fiorina constructs a fairly straightforward formal apparatus and usually buttresses this structure with arguments suggesting that the model is not as unrealistic or restrictive as some might believe. For example, in response to numerous survey findings that the citizenry is not very aware of legislative actors nor highly concerned about many issues, Fiorina argues that citizenry awareness is less crucial than legislators' perceptions that constituents might potentially care about an issue. In general, the author exhibits a great sensitivity to the ultimate confrontation between the world of theory and the world of data. Hence he refuses to generalize his model at a number of points because such generalizations, while theoretically elegant, may be totally irrelevant to the empirical world. This may get the author in some trouble with those formal theorists whose chief concern is the development of the broadest possible theory, no matter how inapplicable it is to any real-world phenomena.

The author derives conclusions from his model for maintainers and maximizers under various conditions including conflictual vs. consensual constituencies; he later extends the analysis to allow for voting abstentions. One of his important results concerns the empirical relationship between district homogeneity and electoral safety on the one hand and district heterogeneity and electoral marginality on the other; he concludes that the homogeneity/heterogeneity dimension is the fundamental one which in large part accounts for the safety/marginality dimension. Another important conclusion deals with the so-called moderate marginal representative; here the author provides some theoretical foundation for empirical results that indicate that representatives from marginal districts do not in fact take moderate or centrist policy positions.

Fiorina's formal analysis has two strong points. One is that it is readable and understandable: It is not encumbered by excessively imposing notation or by highly abbreviated proofs that assume too much of the reader, partly because of the relative simplicity of the author's model, and partly because of the author's obvious concern to communicate with an audience wider than a few formal theorists. The model itself is self-contained and the proofs sufficiently detailed so that the layman can get a first-hand feel for how formal theorizing proceeds. Thus, the book will be of interest to those who want to try to get through a piece of formal theory for the fun of it.

The other strong point of the formal analysis is that, unlike many such efforts, here the author explicitly attempts to bring data to bear upon the theory. While most of his tests and indicators are crude and makeshift (as the author readily admits), the testing is done in such a way as to encourage further efforts along these lines. The data problems the author encounters make good reading for anyone proposing to conduct empirical research in this area.

My criticisms of the book are generally minor and most of them are due to the book's being a revision of the author's dissertation. The prose at times seems forced, the critique of the literature too flippant, and the discussions too truncated. Especially annoying is the large number of throwaway lines that the author scarcely develops, e.g., "What may be needed is a richer theory of coalition dynamics" (p. 81). Such lines may reflect the need in a dissertation to protect all flanks against the onslaught of a dissertation committee, but they leave the reader wanting more.

In summary, the book will be of interest to legislative scholars (particularly chapter 1), to formal theorists, and to those wishing to tackle a piece of formal theory. It is a readable piece of formal analysis that addresses some classical questions of legislative behavior.

HERBERT B. ASHER

The Ohio State University

The Death of Contract. By Grant Gilmore. (Columbus: Ohio State University Press, 1974. Pp. x, 151. $8.00.)

This is a splendid little book. In a hundred-odd pages of text (plus another forty of notes) Professor Gilmore sketches with the deftest of touches the rise and fall of contract theory. To him, the idea of any general theory of contract law (as opposed to the law of specific contracts) does not really start until Langdell published his Casebook on Contracts in 1871. Subsequently Holmes enunciated the "bargain" theory of contract which, together with various other Holmes heresies, was sanctified by Williston and enshrined in the *Restatement.* But from the first, the *Restatement* contained inconsistencies, largely owing to Corbin, whose views differed radically from those of Williston. In due course Corbin's ideas prevailed, and the bargain theory of contract began to come apart at the seams. On the one hand, the concept of injurious reliance, canonized somewhat uncertainly at first, in the *Restatement,* §90, threatened to swallow a large part of contract doctrine proper; and on the other hand, the growth of restitutionary ideas has encouraged the notion that a benefit is worthy of being recompensed, no matter that it was not bar-gained for, so long as it was not clearly intended as a gift. Hence, contract law is falling apart, and is likely to be soon swallowed by the expansive development of tort; or (and surely rather more accurately) in a completely new restatement of civil obligations.

It is a brilliant thesis, superbly presented, and abounding with verbal gems. But is it right? That is likely to be a controversial question. I am prepared to go with Gilmore about three-quarters of the way. I go along with him, first, in placing consideration right back into the very heart and center of contract law, instead of treating it as some odd and anachronistic piece of irrational doctrine. At the fundamental level of ideas, consideration makes a good deal of sense. Prima facie, benefits should be recompensed, and injurious reliance (or detriments) compensated. Indeed, in any society based on private property, it is hard to see how something like these ideas can be dispensed with. If my property is protected by the law, and somehow some of it finds its way into your hands so that you now have received a benefit at my expense, why naturally there is at least a prima facie case for saying that you must pay for it. And equally, since every human society depends on some degree of cooperation between its members, a property-based society can hardly avoid doing something to protect the person who has been impoverished by relying on his fellows who have let him down. Naturally, ideas as wide as this provide little guide to decision until they have been hammered out in experience. But, like Gilmore, I have no doubt that the twin ideas of benefit and detriment underlie a great part of the law of contract, tort, and unjust enrichment.

I go along with Gilmore, secondly, in thinking that the time has arrived when the traditional conceptual structure of contract doctrine is likely to begin to disintegrate. Indeed, I believe that the process has already been going on quite a while (I speak mainly of England but I do not think things are very different in America) but that we do not see what is happening under our very eyes because most of us wear conceptual spectacles. The growth of restitutionary ideas on the one side, and of tort ideas on the other are unquestionably splitting contract down the middle. This development would not, I think, be nearly so unexpected if the history of English contract law around 1770–1850 were better known. My own researches in this area suggest to me that at this time precisely the reverse process was operating. Between 1770 and 1800 or thereabouts there is a good deal of evidence that benefit was a more important concept to English judges than consent. This changed during the course of the nineteenth century, and I have no doubt that this change was greatly influenced by the development of economic theory in England in the first half of the century. And here I must vigorously dissent from Gilmore's view that the lawyers and the economists were not influenced by or even conversant with each other's work. Political economy in England between 1815 and 1860 was not an esoteric subject, discussion of which was confined to experts. On the contrary, it was all the rage. Every educated person was expected to know something about it. The *Reviews* of the period were filled with articles on political economy for the general reader. It was taught in the schools (savagely parodied by Dickens in *Hard Times* [1854]); it was taught by governesses to young ladies; it was made the subject of hugely popular tales and parables by Harriet Martineau and others. And many of the leading political economists were themselves lawyers or were closely associated with lawyers. It is inconceivable that a young man reaching intellectual maturity in England at any time between 1820 and 1850 would not have imbibed a great deal of political economy. And young lawyers growing up between 1820 and 1850 were the judges of the second half of the nineteenth century. This makes me think that what is taking place with contract ideas today is not the wholly new development which Gilmore thinks it is, but (at least in part) a return to the ideas of 1800. And that is not nearly as odd as it seems, for against the background of nearly two centuries it is much easier to see the period of classical political economy and classical contract law as an interlude in the development of moral ideas, when consent, free will and free choice in all things was erected into the ultimate value. In 1800 and again in 1975, consent, free choice, and all that just do not seem to have the same moral force that they had in 1850.

P. S. ATIYAH

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Plato's Republic. Translated by G. M. A. Grube. (Indianapolis, Ind.: Hackett Publishing Co., 1974. Pp. vii, 263. $10.00, cloth; $1.50, paper.)

This new translation recommends itself to teachers and students because of its price: the paperback version will be the cheapest available, and the publishers are to be commended for their economy. Moreover, the translation is quite literal. It almost goes without saying that it is superior to the Cornford translation.

intent on understanding Plato's political philosophy.

THOMAS L. PANGLE

Yale University

Critique of Instrumental Reason: Lectures and Essays since the End of World War II. By Max Horkheimer. Translation by Mathew J. O'Connell and others. (New York: The Seabury Press, 1974. Pp. x, 163. $7.95.)

Max Horkheimer, like other famous members of the "Frankfurt School" was a radical thinker, but his radicalism was embedded in a philosophical approach which he called "Critical Theory," not in any form of political activism. Characteristics of that approach were the search for historical roots of social phenomena, rejection of ideological loyalties, and a preference for analytic rather than political categories. Marxism was the starting point of almost all of Horkheimer's work, and this has tempted many politically minded scholars to seek counsel and support in Horkheimer's work, only to be repelled by his irreverence for Marxist dogma and the pessimism which negates the "political messianism" to which both the old and the new left are committed. Nevertheless his name and his work continue to appeal, not least because he is the most explicit exponent of the dialectical dilemma posed by the simultaneous quest for equality and freedom.

He is also the most astute analyst of the rationalistic elimination of traditional values and perhaps the only modern philosopher to have succeeded in rescuing religion from the contemptuous and simplistic Marxian critique and restoring it to a position of significance as a social category worthy of further study.

In 1947 Horkheimer published a series of public lectures which he presented in the Spring of 1944 at Columbia University, under the title *Eclipse of Reason.* Alfred Schmidt, Horkheimer's successor at the University of Frankfurt and the indefatigable editor of the re-issues of Horkheimer's work, translated these lectures into German and entitled them "Zur Kritik der Instrumentellen Vernunft." In 1967, the lectures together with a collection of essays and speeches under the heading "From the Lectures and Essays in Germany" were published under the single title "Zur Kritik der Instrumentellen Vernunft." The volume under review, *Critique of Instrumental Reason: Lectures and Essays Since the End of World War II,* contains nine of the thirteen items published in the second part of the German version and therefore carries a title which does not belong to it at all. It has a brief foreword by Horkheimer, written in 1967, in which he acknowledges the work of Alfred Schmidt and credits him with selecting and revising the texts. This too has no place in the book under review, in its present form. The "blurb" of the book informs us that Horkheimer's essays deal with "a single theme – the triumph in the twentieth century of the state-bureaucratic apparatus," a description which is not only inaccurate, but also sadly typical of the continuing attempts to reduce philosophical analysis to the dimensions of a poster carried in a protest march.

With so much carelessness already displayed in the title and foreword of the book, it does not really come as a surprise to find that there is no introduction to explain either the considerable difficulties posed by a translation of Horkheimer, a subtle, complex, and frequently laborious and contorted stylist, or the rationale underlying the choices made from the original German selection. Thus an essay on "The Future of Marriage" is included, while an equally important essay on "Authority and the Family of our Time" is left out. The interesting essay on "Theism and Atheism" is included, but another essay on "Religion and Philosophy," which is particularly relevant as an analysis of the religion-Marxism continuum, is left out. An essay on "Kant's Philosophy and the Enlightenment" has been excluded, while a brief and somewhat dated note on Eichmann's arrest is included. In short, one cannot avoid concluding that selection was based more on what was felt to be the kind of material that would appeal to "radicals" than on a serious desire to present a dimension of Horkheimer's work.

Regrettably, the translation itself must also be faulted. Apart from too many simple printing errors, there are serious shortcomings. Sometimes whole chunks are left out of sentences (e.g., p. 49), or the translation is done so literally that it makes for very poor English. Horkheimer does make things difficult for a translator by frequently choosing words which carry several meanings. Thus, when he speaks of the *"leidende Rolle"* of women in bourgeois society, this certainly includes the concept of a "passive role" which the translator offers (p. 16), but it means much more than that, for included in Horkheimer's expression are ideas of long-suffering and oppression. Horkheimer frequently used a concept of "the other" or "the totally other" *(Das Andere, das ganz Andere).* To translate this as "another posintent on understanding Plato's political philosophy.

THOMAS L. PANGLE

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Critique of Instrumental Reason: Lectures and Essays since the End of World War II. By Max Horkheimer. Translation by Mathew J. O'Connell and others. (New York: The Seabury Press, 1974. Pp. x, 163. $7.95.)

Max Horkheimer, like other famous members of the "Frankfurt School" was a radical thinker, but his radicalism was embedded in a philosophical approach which he called "Critical Theory," not in any form of political activism. Characteristics of that approach were the search for historical roots of social phenomena, rejection of ideological loyalties, and a preference for analytic rather than political categories. Marxism was the starting point of almost all of Horkheimer's work, and this has tempted many politically minded scholars to seek counsel and support in Horkheimer's work, only to be repelled by his irreverence for Marxist dogma and the pessimism which negates the "political messianism" to which both the old and the new left are committed. Nevertheless his name and his work continue to appeal, not least because he is the most explicit exponent of the dialectical dilemma posed by the simultaneous quest for equality and freedom.

He is also the most astute analyst of the rationalistic elimination of traditional values and perhaps the only modern philosopher to have succeeded in rescuing religion from the contemptuous and simplistic Marxian critique and restoring it to a position of significance as a social category worthy of further study.

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It is not really possible to recommend this collection of Horkheimer's essays either to the beginner in search of contemporary ideas or to the specialist intent on extending his knowledge of Horkheimer's work. Although there are some lively passages and some of the essays at least catch the spirit in which they were written, the collection as a whole is too haphazard and too carelessly put together to do justice to a thinker of Horkheimer's stature.

JULIUS CARLEBACH

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Organized Citizen Participation in Urban Areas. By John D. Hutcheson, Jr. and Frank X. Steggert. (Atlanta, Ga.: Center for Research in Social Change, Emory University, 1974. Pp. xii, 198. $7.50, cloth; $3.50, paper.)

The authors (John D. Hutcheson, Jr., Assistant Director, Center for Research in Social Change, Emory University, and Frank X. Steggert, Director, Atlanta Urban Observatory, Georgia State University) seek to clarify the process and impact of organized citizen participation in urban areas. To do this they set out to survey and analyze the literature on organized citizen participation, although no attempt is made to analyze individual studies. They attack the lack of a clear conceptual framework in the literature for empirical analysis of organized citizen groups. Their contention is that the typologies are inadequate for purposes of explanation and prediction of citizen participation activities. They correctly but not surprisingly argue that many of the studies involving public participation in democracy fail to use or even consider similar characteristics or variables. They criticize the inconsistency in identifying important variables in the literature and the lack of case study methodology applicable to organized citizen groups. In a rather short but well-organized presentation a beginning attempt is made to remedy these deficiencies. Unfortunately the tools or variables offered do not go far enough to handle the many elements involved in the processes of organized citizen participation. But the book does constitute a welcome contribution to the literature on the methodology of studying participatory democracy.

This study involved the cooperative effort between universities and municipal governments in a national network of urban observatories which formulated what they considered three categories of citizen groups. These included groups sponsored by federal programs such as the Community Action Program, city-sponsored advisory groups, and self-initiated voluntary groups which often attached government polices. These groups were looked at in the context of two case studies involving Atlanta, Georgia. The cases are followed by the six variables or characteristics presented in a general typology and applied to the cases. The variables include: "(1) the goals or purposes of the organization, (2) characteristics of the membership, (3) structural characteristics of the organization, (4) tactics and strategies employed by the organization, (5) degrees of influence attained by the organization, and (6) the relationship of the organization to government institutions" (p. 126). Up to this point the authors used comparable data in their citizen group categories; their only conceptual framework is David Easton's systems model used because of its value in placing citizen participation in a contextual perspective. In some respects the book presents nothing new with the possible exception of the common variables or characteristics.

They suggest that these six variables "must be considered in any explanation of the total process of organized citizen participation and general typology" (p. 126). Greater elaboration, however, is needed about the application of these variables, particularly to the case studies. While the framework of analysis lends itself to the cases provided, an additional case dealing with opposition groups would have been very useful. The authors suggest, however, that the variables can be applied to other case studies to facilitate comparison but do not discuss any methods or problems of doing this for the various case study approaches.

Good questions are raised in the beginning of the book and there are conclusions at the end along with an extensive bibliography. The volume's most important contribution is offering a brief overview of the literature and a conceptual framework using common variables in a general typology which can serve as a useful point of departure for analyzing existing literature and doing further research in citizen participation.

ALLEN K. SETTLE

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The Roots of American Order. By Russell Kirk. (La Salle, Ill.: Open Court, 1974. Pp. xvi, 534. $15.00.)

Russell Kirk's latest book is, in important respects, unsatisfying and unsatisfactory. Kirk intends to chart the "roots" of America's "order." Yet, there is never an adequate joining of the roots and the order. The roots are described with great care and detail. The order is treated as if self-evident, because immutable. The relationship between the two is, presumably, intuitively obvious. Kirk's message is a charade: a guessing game which instructs us via tableaux and indirection. Thus, to appropriate the book in a fuller sense, the reader must construct a suitable syntax to animate the image units provided. The grammatical key to this work is one man's reading of the conservative tradition in America, two hundred years after independence.

According to Kirk, the American order was not founded on any particular philosophy, philosopher, or set of unique experiences in national life. Rather, it is the repository of an extensive historical process – the "Great Tradition." America accumulated this tradition in a two-step course. We took the best from London after London had assimilated the best from Jerusalem, Athens, and Rome. The Judaic experience taught London that all true order and justice comes through a covenant with God rather than by contracts among humans. Greek blunders provided London with helpful warning signals, but it was the "virtually incorruptible" Romans who became the new protectors and heralds of the transcendent order, exemplified by their devotion to *labor, pietas,* and *fatum.* Christianity intensified London's understanding of personal and moral order through its teachings on faith, hope, charity, and salvation. The light of medieval custom, learning, and valor aggregated London's inheritance and readied it for a practical translation into personal and communal life.

While the British reformation, particularly Richard Hooker, counteracted the renaissance threat to the "Great Tradition," the tradition was receiving its great practical embodiment in British national life. Parliamentary authority, organic society, religious toleration, and aristocratic rule attest to this accomplishment. According to Kirk, four men represent an incarnation of this British tradition as it spoke to America. Montesquieu, who had a conservative impact in the Anglo-American world, illustrates the devotion to representative institutions, while Hume, Blackstone, and Burke denigrate reason, exalt historical precedence, and embrace medieval Christianity, respectively.

Colonial America, says Kirk, is fathomed largely by explaining Britain. It was Britain transplanted to a new, freer, more austere, and isolated environment. Our personal order remained nearly identical to that of good Englishmen, but our public order introduced several important, but distortionless, differences. We were governed by gentlemen of merit rather than by aristocrats; we had been granted a pattern of political autonomy (salutary neglect); and we had greater religious pluralism. The Declaration of Independence, the American Revolution, and the Constitution embody, express, and conserve this tradition for America against both past and future adversaries. Through them we merely claim and preserve "the chartered rights of Englishmen."

Of course, to have a "Great Tradition" is to have a "Lousy Tradition" as well. Kirk does not disappoint us. There is a continuous Manichaean struggle throughout this work. It is not systematically or, apparently, consciously developed, but it is the only reminder in these pages of what has been omitted, negated, or distorted. Listing "Great Tradition" items first, here is an abbreviated roster of Kirk's warring pairs: American Revolution/French Revolution, Great Tradition/Enlightenment, Christianity/Humanism, Reformation/Renaissance, Eastern America/Western America, Rome/Greece, tradition/ideology, faith/reason, covenant/contract, righteousness/*hubris,* individualism/egoism, common law/positive law, Hooker/Hobbes, Burke/Locke, Bunyan/Milton, Hume/Rousseau, and Washington/Jackson. Investigations of these conflicts provide focused and critical perspectives by which Kirk's project can be understood and evaluated.

Kirk's heavy rhetoric against reason and ideology is especially ironic since he only offers us a rationalistic tract in the history of ideas. There is a neglect, salutary or not I wouldn't guess, of identification for most primary source quotations. More substantively, Kirk is unwilling to ask hard questions and to come to terms with the implications of his own assertions: e.g., consider the relationships between personal order and social order, "order" and social class, and liberty and equality. Most troubling of all, Kirk's vision is consistently flat, unidirectional, and exclusionist. Does scripture provide such easy chronologies? Does Hooker summarize the Reformation for America? Can Locke be displaced as well as dethroned? What confidence can we have in the summaries of Hume when his position vis-a-vis American independence is so totally misstated? Are British and American common-law perspectives undifferentiated? How tight must we squint our eyes to see the Declaration of Independence as a conservative document, the Constitution as an organic covenant, and to have the *Federalist* and the Articles of Confederation drop from sight altogether? What is the relationship between classical liberalism and Kirk's conservatism with regard to representative government, checks and balances, separations of power, liberty, private property, individualism, free enterprise, stability, toleration, and continuity?

In important respects, Kirk offers a confession of faith rather than a rigorous historical or philosophical analysis. If you come to it as a believer, you'll be overwhelmed. But how can you respond as an infidel? Yet, respond you must. The heuristic stimulation of this book is enormous, as is the danger that it may be swallowed whole. Solace may come inadvertently. The seeds of this harvest may succumb to the sterility of their own purity of breed.

ROBERT W. HOFFERT

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Integrative Mechanisms in Literature Growth. By Manfred Kochen. (Westport, Conn.: Greenwood Press, 1974. Pp. xv, 275. $14.95.)

This book was not written for political scientists, and, unfortunately, there is little in the book that most would find useful. I say "unfortunately" because Professor Kochen, a distinguished information scientist who stands at the forefront of his field, addresses a question which should be of central interest to all scholars: "If a society possesses, at a certain time, a body of knowledge, what laws, if any, govern the way this body of knowledge can grow?" (p. 3). If Kochen has answers to this question, our entire discipline should take notice. That this is not the case renders the book interesting only to those political scientists who are especially concerned with "research on research," to use Kochen's phrase.

The book was published as Number 9 in the series, "Contributions in Librarianship and Information Science." It consists of eleven chapters, seven of which are reprints of lectures or articles dating from 1968 to 1972. The "library science" vs. "information science" distinction is roughly analogous to the "traditional" vs. "behavioral" approach in political science, and this book is an excellent example of the empirical and quantitative orientation of modern information scientists. Rigorous use of mathematics is prominent in the book, especially in chapter 5, which formulates alternative models for evaluating "the extent of literature integration in any year by the number of review papers published in that year," with the key variable being "the fraction of the literature published in year *t* which is ever cited by one or more review papers" (p. 91). The models, with stringent assumptions, were evaluated for their mathematical properties rather than tested against the literature in any field. Kochen realizes the limitations of his analyses, but he nevertheless contends that they "open up an area of exploration which sharpens our understanding of literature growth, suggests the kind of logic and mathematics which applies, and points to a variety of experimental and empirical studies to be done" (p. 106).

Some parts of the book are decidedly less relevant for political scientists than others. chapter 5, just described, would fall in the marginal category. Six other chapters, all of which are reprints of previous work, appear to be even less valuable. The assessment of "Information Processing with Contemporary Computers" (chapter 4) is based on 1969 information, and, despite the brief updated appendix, neither advances the knowledge of one already familiar with computer applications nor serves as a useful tutorial. Chapter 6, "Information Science and Computer Aids in Education," would be read by many who have already had firsthand experience with systems like those he outlines. His results of "An Experiment to Change Attitudes Toward New College Mathematics" (chapter 7) are not conclusive, and the experiment is only obliquely related to the subject of the book. The title of chapter 8, "Newer Techniques for Processing Bibliographic Information," seems pertinent enough, but the specific system pertains largely to library administration; the information is also dated, having been published previously in 1968. His "Cost-Effectiveness Analysis of *See*-Reference Structure in Directories" (chapter 9) could have had considerable practical significance for political scientists engaged in indexing books, document or data collections, and so on, but he found no optimal number for *See*-references in directories. Finally, his study of "Information-Seeking Behavior of Catalog Users" (chapter 10) is clearly aimed at librarians.

Political scientists who are especially interested in the history of science are most likely to find stimulation in Kochen's first three chapters. In chapter 1, he stakes out his quest for laws which govern the growth of knowledge: "basic laws of principles, analogous to those sought by biochemists and physiologists, *can* be discovered" (p. 8). The phenomenon of "fragmentation" of knowledge is a central concern in his pursuit of such laws. Chapter 2 employs mathematical models of epidemics to examine the "spread" of scientific ideas. Kochen concludes that works of evaluation and synthesis are necessary to produce stability and that theory is necessary to avoid fragmentation. He pursues this line in chapter 3, which argues that "forces leading to the accumulation of isolated specialized facts act independently of forces leading to the formation of structures that cross-link facts" (p. 44). Institutions engaged in the production of knowledge must, therefore, stimulate imagination and creativity in order to promote intellectual bridge building.

Kochen discusses "Integration and Disintegration in the Knowledge Industry" in his final chapter. How well does he think he has achieved his prime objective? "Inasmuch as we have not yet established any new and really basic laws governing the growth of knowledge, we have not yet answered the first question to our complete satisfaction" (p. 248). While I agree, I would also urge Kochen to continue his inquiry, for the question is important to us all, and few scholars demonstrate the combination of experience, imagination, and rigor that seem necessary for the task.

KENNETH JANDA

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Kant. By Alexander Kojève. (Paris: Éditions Gallimard, 1973. Pp. 219. Price not listed.)

In Alexandre Kojève's *Kant,* one is offered a Kant refracted through an Hegelian lens ground with Marxian equipment; given this limitation, or this advantage (depending on one's point of view), one finally has a Kojevian account – found among his posthumous papers – of the sole philosopher considered by Kojève himself to be on a level with Plato, Aristotle, and Hegel.

In his brilliant and influential *Introduction to the Reading of Hegel,* which Allan Bloom has rightly called the most "subtle" effort to find foundations for a more adequate Marxism in Hegel, Kojève makes his treatment of the *Phenomenology* revolve around Hegel's great set piece, "Master and Servant." And in this treatment Kojève argues that for Hegel human society and human "discourse" began when men were first willing to risk their "animal" and biological existence in a "fight to the death" for "pure prestige," for "recognition" by the "other": the man who became the Master was he who was "willing to go all the way" in this fight, while the Slave was the one who saw and feared his own "nothingness" should he die in the struggle; the Slave "recognized" the Master rather than die. The Master, for his part, finds that he is not "satisfied" with mastery, since he has risked his life for recognition by a mere Slave whom he uses as a "thing"; the Master has the "pleasure" of not having to *work,* but this pleasure is not a true satisfaction. The Slave, on the other hand, who works in the service of the Master, can ultimately find satisfaction in his work (by which he transforms the natural world of "given-being" and himself as well): through work, which "negates" "given-being," the Slave overcomes the world.

Since this overcoming is not historically complete until men choose their own work in Hegel's "civil society" and become citizens of the Hegelian state, history is *inter alia,* a history of "slave ideologies" by which Slaves conceal their slavery from themselves. For Stoicism, Epictetus in his chains and Marcus Aurelius on his throne are "equal" as "wise men"; hence for the Stoic "ideology" the chains do not "matter." In Christian "ideology" equality is of a different sort: all men are "equal" before God, whatever their earthly stations; but this is simply another escape to a "beyond" (beyond the historical world of work and struggle) in which, though there are no "masters," there is one "universal" Master (God) to whom everyone is enslaved. A modern bourgeois may, by contrast, appear to be "his own master"; but he is simply enslaved to property and to capital. And so long as there is slavery, whether to a Master, to God, or to Capital, man will never be truly "satisfied" or truly free, since true satisfaction and freedom come from being "recognized" as an equal by an equal, which is possible only given Hegelian "civil society" and the Hegelian state.

No one has doubted the ingenuity of this reading of the *Phenomenology:* if it is not an irresistible or a "natural" reading – since, to use George Kelly's language, it treats the Master and Servant "tableau" as "the synoptic clue to a whole philosophy" – it is one which "works" for substantial and important sections of Hegel's greatest work. The notions of "work," "struggle," "recognition," "satisfaction" and "freedom" are really *there;* and if, strictly speaking, the notion of "ideology" is a Marxian importation, it is arguably prefigured in Hegel's remark about philosophy (as the "thought of the world") arriving after reality is "cut and dried."

However far work, struggle, recognition, satisfaction, freedom and even ideology may go in illuminating Hegel's *Phenomenology,* they are arguably *not* the notions which cast the strongest or the fairest light on philosophers who are not Hegelians. And this emerges clearly in Kojève's treatment of Kant: it is no accident that he argues that if one suppresses Kant's idea of the "thing-in-itself" ("beyond" the empirical world, in a realm of "transcendence") and treats freedom and purposiveness as *real* rather than as "necessary hypotheses" (as Kant treated them), one can "transform" Kantianism "quasi-automatically" into the Hegelian "system of knowledge" – as if such a "transformation" would necessarily be advantageous.

Now Kant did treat freedom as a "necessary hypothesis" whose reality could neither be demonstrated nor denied; in his *Groundwork of the Metaphysic of Morals,* indeed, he argues that "freedom is only an idea of reason, and its objective reality in itself is doubtful." Nonetheless Kant urges that if the idea of a free will as a "noumenal" or intelligent "causality" is given up, all morality will become merely "empirical," and concludes that while freedom cannot be proved to be "actually a property of ourselves or of human nature," it must be presupposed "if we would conceive a being as rational and conscious of its causality in respect of its actions, that is, as endowed with a will." And this "hypothetical" freedom has a very immediate bearing on Kant's politics, since he argues in *Eternal Peace* that if there exists "no freedom and no moral law based upon it, and if everything which happens . . . is simply part of the mechanism of nature," then it is appropriate to manipulate men as natural objects in order to govern them; but that if "right" is to be the "limiting condition" of politics, then politics must be capable of treating men as they ought to be treated – as "ends in themselves."

What distresses Kojève is that the Kantian "categorical imperative" of treating men as "ends," as free beings who ought to be "respected," is based on a freedom which is only *hypothetical:* one builds categorical moral requirements, which also serve as the "limiting condition" of politics, on "as if" foundations. Or, as Kojève himself puts it, "one cannot effectively speak of 'morality' without speaking of 'freedom,' and good sense would be scandalized by a 'commandment' addressed to someone . . . incapable of following it." Kant, he goes on, succeeds only in talking about "what one ought to do, IF the will is free"; but this reduces the categorical to the hypothetical, with a devastating effect on the "limiting condition" of politics. And since (according to Kojève), Kant failed to treat freedom as something real, he "never wanted to admit the *efficacy* of free or conscientious human activity in the world and as a result refused to identify (discursive) truth with history" – a history characterized by work and struggle.

Since one cannot "settle" the question of Kantian freedom at this juncture, it will have to suffice to say that (1) even if freedom could be shown to be something more than a necessary "idea of reason," Kant would have had no reason to "identify truth with history," since he "needed" freedom to account for the *conceivability* of common moral concepts (duty, responsibility, etc.), not to explain historical "work and struggle"; and (2) it is by no means clear that it is legitimate to reduce the Kantian notions of "necessary hypothesis" and "idea of reason" to a mere "as if": Kant does not say that it is "as if" men were free (but "really" not), but that men must *necessarily* conceive themselves as free if their ordinary moral ideas are to be intelligible.

The strengths and weaknesses of Kojève's insistence in the 'as-ifness" (so to speak) of Kant's philosophy emerge with particular clarity in his handling of the *Critique of Judgment,* which is Kant's greatest treatment of teleology or "purposiveness." Kojève begins by observing, correctly enough, that Kant undertook *Judgment* "with a view to looking for a connecting-point between the *given* natural world and man as possesser of a *free will";* that connecting-point was to be "purposiveness," since the natural world has a "purposive" structure and man *qua* free agent has "purposes." But Kant failed, according to Kojève, to talk about "struggle" and "work," which are the most "striking" instances of a "purposive" freedom operating on a purposively-organized nature; and this failure can be traced to the fact that in *Judgment* Kant treated purposiveness "in the 'as-if' mode."

Now what this characterization misses is the fact that in *Judgment* Kant actually *did* find a "connecting-point" between nature and human freedom (and art as well) through the notion of purposiveness: plants have "purposes" which men impute to them, men both *have* purposes and *are* the "final purpose" or "final end" of creation, and art is "purposiveness without purpose." But Kojève mis-reads Kant's effort to achieve this tripartite linking-up when he says that Kant limits "his teleology to natural and artistic beauty and to organic life," that he excludes purposiveness from "the domain of human action, of struggle and work." This not only contradicts what Kojève himself earlier called Kant's effort to find a "connecting point" between nature and human freedom, but leaves wholly out or account the very section of *Judgment* which is most important for Kant's moral and political philosophy. And in that section (84), Kant argues that while the "purposes" of things in the natural world are imputed by us to those things, man is the ultimate purpose of creation on earth because "he is the only being upon it who can form a concept of purposes, and who can by his reason make out of an aggregate or purposively formed things a system of purposes"; that one cannot ask "for what end" man exists, since "his existence inherently involves the highest end." Since the argument that only man can *conceive* purposes – let alone conceive of *himself* as the "ultimate" purpose – constitutes Kant's most effective defense of the notion that men are "ends-in-themselves" who ought never to be used merely as means to arbitrary purposes, and since that "purposive" argument is the "limiting condition" of rightful politics, it is difficult to understand why Kojève argues that Kant "excludes" purposiveness from "the domain of human action." Even though Kant treats purposiveness, like freedom, as a "necessary hypothesis," this involves no "exclusion"; indeed Kant argues that teleology is the *one* concept which draws together nature, human freedom, and art.

If Kojève's (Marxianized) Hegelian emphasis on work and struggle leads him to exclude purposiveness from (Kantian) "human action," his insistence on "satisfaction" leads to a difficulty with which one can reasonably conclude. Now Kant is famous for the asceticism of his moral philosophy, and is rightly celebrated as the enemy of eudaemonism and utilitarianism (hence of Aristotle and Bentham); "the majesty of duty," he once observed, "has nothing to do with the enjoyment of life." But Kojève makes Kantian morality turn on "satisfaction" in a "beyond." To be sure, Kant sometimes argued that the "supreme good" – happiness conjoined with *worthiness* to be happy – does not exist on earth, that there ought to be immortality in which God provides this conjunction (and then, *selon* Kojève, those who have been good are "satisfied"). This interpretation leaves out of account the fact that Kant increasingly doubted this "moral proof" of God and immortality (as evidenced in his *Opus Posthumous*), and it seems to make "satisfaction" the pre-condition of morality. But if there was ever a philosopher who separated moral philosophy from psychology (with a vengeance), it was Kant; if, for him, one's motive in "acting well" involves a hope for happiness ("satisfaction"), one doesn't have a "good will" at all. Kant's idea of the "supreme good" is problematical enough without trying, *à la Kojève*, to make "satisfaction" the pivot of Kantian morality – which is not, after all, just a Benthamism in which "satisfaction" is maximized in a "beyond" rather than on earth.

PATRICK RILEY

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Averroes on Plato's Republic. Translated, with an introduction and notes, by Ralph Lerner. (Ithaca and London: Cornell University Press, 1974. Pp. xxxix, 176. $15.00, cloth; $2.95, paper.)

Averroes, the great philosopher of medieval western Islam, planned a summary of Aristotle's philosophy. Being unable to obtain a copy of the *Politics,* he summarized Plato's *Republic* instead as the second part of political science, to come after the account of the *Ethics.* The original Arabic version is lost, but a fourteenth century Hebrew translation has survived; it is this Hebrew version which is translated into English in the above book. Averroes excludes Book One, the first half of Book Two. and Book Ten of the *Republic* because their arguments are "dialectical" rather than "demonstrative." The rest of the *Republic* is covered in three sections: first, the structure of the ideal state and the early education of the guardians; second, epistemology and the higher education of the guardians; third, the nature of the imperfect constitutions and their corresponding individual types. In both plan and substance, the work follows the *Republic* fairly closely, though there are frequent references to contemporary problems and Islamic views and the epistemology and moral philosophy owe more to Aristotle than to Plato. The present translation uses a slightly different text from that of Rosenthal (Cambridge University Press, 1956) and those who know no Hebrew or Arabic will find it easier to use. Footnotes to the translation deal with matters of content only while textual and linguistic notes are collected in a separate appendix and glossary. Page references to the appropriate sections of the *Republic* are given in the margin so that the reader may easily compare the commentary with the original and identify the places where Averroes departs substantially from Plato. A short introduction, which the completely uninitiated may find hard to follow, concentrates on the relation between Averroes's beliefs and those of the *Republic.*

The modern reader acquainted with recent Platonic scholarship will probably find that

Averroes's work does not shed any new light on the *Republic* itself. It may, however, serve to remind us of certain important features of the *Republic* which we might otherwise forget: for example, that the differences between Plato's and Aristotle's ethics and epistemology, at least as far as they affect political theory, are much less significant than the similarities; that, though the *Republic* is explicitly utopian, the principles which it incorporates are relevant to ordinary, everyday politics; that much of the impact and indeed the meaning of the *Republic* is lost if we try to detach its doctrines from the literary expression Plato gives them.

Averroes's work is worth reading today less as a commentary on Plato than as a work of political theory in its own right. Of particular interest is the author's mediation between Platonism and the tenets of Islam. He is able to adopt many of the major Platonic doctrines without qualm, such as the superiority of the intellect and the noncorporeal, the need for censorship and propaganda, and the comparison of the ruler with the doctor and all that this comparison implies. Perhaps more surprisingly, he accepts communism of property and children and the equality of women. (If he had been able to obtain the *Politics,* he might have thought differently!) He finds many contemporary examples for Plato's descriptions of imperfect states, although, significantly, in the case of democracy, he emphasizes the licentious character of the citizens rather than their control of government through a popular assembly. There are aspects of the *Republic* with which he has difficulty: for example, Plato's ethnocentricity and belief in the superiority of the Greeks, his restriction of the types of justified warfare and the small size of his ideal state. In dealing with such questions, Averroes reveals himself as neither slavishly Platonic nor slavishly Islamic but as a thinker of independent and critical judgment. With this new edition and commentary, Ralph Lerner has given students of medieval political thought access to a work of permanent philosophical and historical interest.

R. G. MULGAN

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The Third Duma, Election and Profile. By Alfred Levin. (Hamden, Conn.: Archon Books, 1973. Pp. 210. $8.50.)

The Imperial Duma, the Tsarist version of Russian parliamentarism, has become in recent years the subject of growing interest and extensive research in Western historiography. The reasons for that development are rather obvious: after many years of intensive preoccupation with the history of Russia's revolutionary movement the time has come for the study of alternative roads of development that were available to the Empire before its demise. A more balanced picture and better understanding of Tsarist Russia during the last decade of its existence is bound to emerge out of the increasing number of studies devoted to movements, institutions, and personalities not connected with the revolutionary movement. The studies dealing with the Dumas are of special value in this context: whether explicitly or by implication they try to evaluate the strength and weaknesses of Russia's short-lived attempt at a constitutional monarchy and the reasons behind its failure.

Professor Levin's book is a most valuable addition to the literature on the Duma period. While it carries the rather modest name of *The Third Duma, Election and Profile,* the book is much more than a formal description of the elections to the Third Duma or a statistical analysis of its composition, as its title implies. The author provides us with a detailed and well-researched analysis and description of the entire political spectrum of prerevolutionary Russia: from the extreme reactionary right that hoped to destroy the newly established representative institution, because it contradicted the "true Russian spirit," to the extreme left, because the Duma constituted a possible obstacle to its revolutionary hopes. The result is a study of great value not only because it describes the political currents in Russia in 1905–6, but also because it provides a perceptive evaluation of the political mentality and political culture of prerevolutionary Russia.

The subject proper of Mr. Levin's book are the elections to the Third Duma following the "governmental revolution" of June 3, 1907, which radically changed the Electoral Law in an attempt to create a "trustworthy" and "workable" Duma. The first chapter of the book is devoted to a detailed analysis of the new election law and the governmental reasons behind the changes. In order to achieve a "trustworthy" Duma, the representation of the peasantry, workers, and national minorities was drastically curtailed, while that of the more wealthy city and in particular land owning gentry were given a preponderant place. In describing the attitudes and composition of the various competing parties, Mr. Levin provides us with a detailed and balanced evaluation of the political forces operating in Russia in the decade preceding the revolution. The analysis of the election process, of governmental manipulation of the voting, and of the composition of the Third Duma is based on a broad variety of archival and newspaper reports used for the first time in Western historiography. The author succeeds in providing us not only with reliable data but also with some penetrating insights into the political culture and mentality of imperial Russia. The book, designed as one of a series of studies dealing with the Duma period, certainly constitutes a most valuable link in the series.

BEN-CION PINCHUK

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Lenin on the Jewish Question. Edited by Hyman Lumer. (New York: International Publishers, 1974. Pp. 155. $7.50, cloth; $2.75, paper.)

Roy Medvedev, in his perceptive, though regrettably neglected, *samizdat* essay on the Soviet Jewish problem (*Blizhnevostochnyi konflikt i yevreiskii vopros v SSSR*, May, 1970), concludes that the way to solve the burning "Jewish question" in the USSR is "simply . . . to carry out consistently the most important instructions of V. I. Lenin. . . ." Thus a compilation of Lenin's writings on the "Jewish question" is to be welcomed especially at a time when the question has assumed a central role on the agenda of detente discussions with the West.

Careful analysis of Lenin's comments on the "Jewish question" – basically, but an element in his broader ideological concept of the nationality question – indicates two principal themes. First, Lenin considered that the Jews, lacking a territorial base, were not a "nation"; in consequence, in his view, their assimilation into Russian society is a "historically progressive" phenomenon. Proposals advanced by the "Bund," the Jewish Workers' Union, in the early part of the twentieth century, to maintain and promote the cultural integrity of Russian Jewry, were rejected as "untenable scientifically" and "reactionary politically." They were merely "the slogan of the rabbis and the [Jewish] bourgeoisie. . . ."

Yet, even as assimilation was encouraged, Lenin opposed what later would be referred to as "forcible assimilation." Always the pragmatist, who was profoundly conscious of national sensitivities, the Bolshevik leader, once in power, actually approved the creation of separate Jewish sections ["Yevsektsiia"] within the Bolshevik Party. His opponent on this vital issue was none other than Yakov Sverdlov, a Jew who became the first president of the Soviet regime. This episode together with a considerable body of evidence about Lenin's toleration of the creation of a vast network of Jewish institutions are examined in *The Jewish Nationality and Soviet Policy*, the meticulously documented work on the "Yevsektsiia" by Professor Zvi Gitelman. Ultimately, the Jewish people were expected to wither away; but, for the time being, its attachment to Bolshevism could be secured only through specifically Jewish institutions – Communist-controlled, of course.

Lenin's second major theme, intimately related to the first, was that anti-Semitism and all forms of discrimination against Jews must be vigorously combatted. In his view, Jewish separatism and nationalism are nurtured by anti-Semitism. With the elimination of anti-Jewish discriminatory practices and pogroms, the process of assimilation would be hastened. As early as March 1914, Lenin drafted a bill on nationality rights for the Bolshevik fraction in the Duma which provided for "the repeal of all restrictions upon the rights of Jews. . . ." And he was the moving force behind the historic legal ban on anti-Semitism adopted on July 27, 1918 by the Council of Peoples' Commissars. Nine months later, he delivered an impassioned address which denounced "those who foment hatred towards the Jews. . . ."

Both themes have been subverted by Lenin's successors in the Kremlin. "Forcible assimilation" became the policy of Stalin in 1948 and has persisted ever since. The outstanding researcher on Jewish sociology, Yakov Kantor, in an article (in Yiddish) published in Warsaw in 1963 called attention to the demolition of Soviet Jewish "schools of all kinds, museums, theatres, libraries, even sections of academies and so on" as the means for forcing the process of assimilation. Lumer makes no reference either to this essay or to Medvedev's analysis.

Ironically, however, alongside of an apparent assimilation, a heightened national self-consciousness among Jews has also emerged. As Lenin might have foreseen, the development is a product of a growing official discrimination against Jews, most recently in higher education and in career opportunities, as well as a vitriolic anti-Semitic propaganda campaign, only thinly masked as anti-Zionism. An expression of the heightened national consciousness is the emigration drive among Jews that has become intimately linked with current East-West detente negotiations. Medvedev warned that "if we want the number of 'repatriates' [i.e. Jewish emigrants] not to be too sizeable, we must change decidedly the position of all Jews in the

USSR, abolishing and prohibiting all the discriminatory measures regarding this national group."

The warning is not very likely to be heeded for the reason that a neo-Stalinist ideology on domestic matters, rather than a Leninist one, is dominant today in the Kremlin. Its current leaders persist in refusing to recognize publicly the existence of a Soviet Jewish question. That view is echoed in the Introduction to *Lenin on the Jewish Question* written by Hyman Lumer. His comments, rather than providing either profound analysis or illuminating additional information, offer a simplistic apologia for current Soviet policy on the "Jewish question." He draws upon no independent critical analysis of Soviet nationality policy. Still, Lenin's writings, as assembled by Lumer and taken from the English edition of his *Collected Works*, Published in Moscow between 1960 and 1970, are nonetheless there for students to examine.

WILLIAM KOPEY

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The Meaning of Czech History. By Tomáš G. Masaryk. Edited and with an introduction by René Wellek. Translated by Peter Kussi. (Chapel Hill: The University of North Carolina Press, 1974. Pp. xxxii, 169. $9.95.)

Tomáš G. Masaryk, social philosopher, Protestant humanist, and Slavic awakener, was the first President of the Czechoslovak Republic (1918–1935), which was the sole oasis of democracy in Middle Europe between the two world wars. He succeeded in protecting it against threats from the extreme Right as well as from the extreme Left, and the adherents of both radical trends have never forgiven him for their political and moral defeat.

It is true that the Czechoslovakian Communist party, when it was still concerned with proper election results, did not refrain from appealing to Masaryk's social ideas. However, after the power take-over in 1945, when the Communist Party no longer considered it necessary to respect public opinion, it staged a campaign against Masaryk, in no way less bitter than that which the Nazi occupants and their internal accomplices had carried out against Masaryk a decade before. Both campaigns also shared the same goal: to discredit the value of democratic liberties and institutions which Masaryk personified and which the Communists, as well as the Fascists, had done away with.

In the 'fifties, the compilation of anti-Masaryk pamphlets in Czechoslovakia represented one of the means by which young Czechoslovakian arrivistes worked their way into political careers, and only in the second half of the 'sixties, with the growing skepticism toward the achievements of the Communist party, did some Communist intellectuals again take a more objective approach to Masaryk's ideas. Yet many prominent Communists never – not even during the Dubček period – gave up their anti-Masaryk attitudes, and after the Soviet military intervention in Czechoslovakia in 1968 propaganda against Masaryk reached its apogee once again.

This fact itself brings out the importance of the ideas of Tomás Masaryk, the selection of whose work is presented by René Wellek to American readers in a careful English translation by Peter Kussi. This selection, devoted to the theme of the meaning of Czech history, comprises the following chapters: "Jan Hus and the Czech Reformation" (from a speech delivered in 1910 and published in Jan Hus: *Our Rebirth and Reformation*, 1923), "The First Stirrings of National Revival," "Czechs and Slavs: The Time of Kollár and Jungmann," "Completion of the Czech National Renascence," (all from *The Czech Question*, 1896), "The Mind of František Palacký" (from *Palacký's Idea of the Czech Nation*, 1898), "The Essence of Political Realism" (from *Karel Havlíček: Endeavors and Desires of the Political Awakening*, 1896), and "The Social Question" (also from *The Czech Question*, as published in Czechoslovakia during the short period of interregnum in 1969).

René Wellek prefaces the book by an excellent and instructive analysis of Masaryk's personality and supplements it with an extensive bibliographical index that keeps the reader from getting lost in the labyrinth of names and events alluded to in Masaryk's work.

The organization of chapters using a historical sequencing of their content, binds the chosen passages into a logical unity. By the same token, however, the thematically most distant selections are advanced to the beginning of the book, while the matters which are presently more relevant are deferred to the end. It is unfortunate that the collection does not begin at the point where this book finishes, that is, with the social issue; and this should have been extracted from Masaryk's *The Social Question* (1898), rather than from his *The Czech Question.*

Masaryk's social philosophy, as elaborated in his *The Social Question*, is still vital and could be of considerable interest even for an American reader. In contrast, Masaryk's presentation of the Czechoslovak national question, although one of the most important documents of the cultural tradition of Middle and Eastern Europe, retains only historical value.

A renowned Czechoslovakian journalist, who in his youth belonged to Masaryk's intimate circle, has recently written: "... where Masaryk describes the circumstances of his time it is like looking at an old map which no one has had in hand for a long time already." This volume brings to life the memory of this map, but it cannot bring to life the map itself.

ZDENEK KRYSTUFEK

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The African Image. By Ezekiel Mphahlele. Revised Edition. (New York: Praeger Publishers, 1974. Pp. 316. $8.50.)

Ezekiel Mphahlele, a native South African, now professor of English at the University of Pennsylvania, has updated and expanded his previously published (1962) examination of Africa's political and literary images. In his own words, this is largely a personal book. He chooses to explore images which can cut across the barrier that lies between literary activity and social study. His tools are observation, imagination and intuition.

Mphahlele makes no pretension to objectivity. He admits he "cannot portray the character of a white man in the round" (p. 14). His attempts to look at the white stereotype from another angle fail because of his anger. In this lack of objectivity lies the book's real value to the nonblack student of African affairs, who can now read, feel, and perhaps understand how a black African intellectual sees his own world.

And a brutal world it is; there is little, if any, accommodation with whites of any genre. He sees capitalists and expatriate civil servants thus:

> We are poor; we do not manufacture; we do not process our raw materials. For the prices of our products we depend on the big bosses who control world markets. We sack expatriate civil servants and compensate them. They leave and come back into higher jobs in the same civil service, as technical aid from our one-time colonial master. "You're *still* my burden," I can hear them chuckle (p. 19).

The political left does not fare better:

> If our leaders used the small segment of the radical left among the whites in our political life, it was literally to *use* them. Naive minds interpreted this as a subordination of African interests to the whims of white marxists. American political scientists saw this as the thin end of the wedge for Moscow to come in. Nothing could have been more ridiculous than to see white and black anti-communists provide America with just the kind of lies that would justify its material support of Boer fascism. To use whites politically where one finds a genuine radicalism is not to seek cultural integration or assimilation in a country where Blacks are such an overwhelming majority as in South Africa. You could count with the fingers of your hand Africans who were ideologically Marxist. Nationalism has always been the dominant motive in liberation politics in South Africa whatever the rhetoric (p. 28).

The first part of the book, "Political Images," suffers from strict black-and-white portrayal, in both senses. Mphahlele accepts no shade of gray. He sees the recent victories in national struggles as obtained at the expense of identity. In his simplistic exposé of factors obscuring the noble heritage, religion comes off badly:

> The missionaries were busy dragging and pushing and kicking us "up" to the Christian god. They were busy collecting souls – Black souls – in order to pickle them, preserve them for the good lord to collect in his own good time. If they taught rebellion, it would also come to rebellion against their god for sure. It would amount to a tearing of the veil before the altar and finding a void (p. 26).

Mphahlele's view of the political world is harsh and uncompromising. He preaches total opposition to white rule in any form. Even "a *black* liberal is a kind of chimera" (p. 50). White liberals are pathetic – cases in point to him are Margaret Ballinger and Helen Suzman, leading South African Progressives. Africans who search for a *modus vivendi* with the white presence are reviled as "Political crackpots, small-time dictators like Hastings Banda of Malawi" (p. 65).

The objective reader of *The African Image* may find it possible to accommodate Mphahlele's militancy. No compromise is possible, however, with distorted history: "When the whites came to Africa, they were emerging from a war-ravaged Europe. The veneer of respect for law and order on them did not take long to peel off once they were faced with organized African communities" (p. 59). What whites? The Dutch settlers who arrived with Jan van Riebeeck in 1652? The British "Albany Bay" settlers of 1820? The anti-Nazi refugees? What organized African communities: the Haphazard tribal Bantus, the Sotho, Ngoni and others, nomads from north of the Limpopo region? The Bushmen? The Hottentots? Mphahlele's blanket condemnation of Bantustans – "Fences of barbed wire across the country, across allegiances, across the landscape of African nationalism. . ." (p. 52) – does not accord with the Organization of African Unity decision to accept *de facto* boundaries, since an adjustment along ethnic or other homogenizing lines is practically impossible in the present-day African context.

Equally hard to accept are Mphahlele's views on ecology and conservation:

> It is a silly western idea to conserve wild life for the entertainment of foreign tourists. Yes, it brings in revenue, but left to ourselves, we would rather use the land to provide food for us. Personally, I have no sympathy with white people who weep over the disappearance of a species of lion or leopard or elephant. Because this western attachment to animals, wild and domestic, that goes beyond basic utility, disgusts me. It does most other Africans (p. 71).

One can only devoutly hope that his views are not shared by many.

The last part of the "Political Images" section analyzes *négritude* and Afro-American attitudes vis-à-vis African nationalism. The ample literary references and Mphahlele's criticism are the forte of the book.

The second part of the book, "Literary Images," examines the writings of whites on blacks and blacks on blacks. Mphahlele ably presents selective synopses about Africa and Africans, juxtaposes these for comparative purposes, and offers the reader his evaluation, without being overbearing. Writers include Joseph Conrad, William Faulkner, Alan Paton, Doris Lessing, Nadine Gordimer among the whites; Daniel Kunene, Thomas Nofolo, Wole Soyinka, Chinua Achebe among the blacks. The list is long and impressive. Since this kind of literary effort is very rare indeed, the second part of the book makes for genuinely enlightening reading. The exhaustive "Select Bibliography," listing fiction, poetry, drama, criticism, autobiography, essays, anthologies and periodicals (16 pages) is updated to 1971.

Any scholar interested in Africa should read *The African Image.* He may bristle at some of the author's brutally stated views; he may find himself in agreement with others. He will certainly find that the literary section contributes substantially to his knowledge and enjoyment.

EDGAR S. EFRAT

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Uses of the Future. By Herbert J. Muller. (Bloomington: Indiana University Press, 1974. Pp. 264. $10.00.)

Herbert J. Muller, Distinguished Service Professor Emeritus of English and Government at Indiana University, is not a professional "futurist." Nor does he write as a social scientist of any description. His new book, like his earlier works, does not consist of definitions, propositions, demonstrations, and marshalings of evidence. He is by temperament and style a humanist, while in his politics he is a traditional liberal who admires John Dewey and Adlai Stevenson. The essays collected in this book are discursive – and sometimes rambling – discussions of such subjects as the contemporary state of ideology in America, the theories of B. F. Skinner, and the future of America and the world.

At first glance, therefore, Muller would not seem to have much of value to say to the contemporary political scientist. Yet to jump to this conclusion would be to beg the major questions to which he addresses himself: the extent to which the future can be predicted, the role of values in education and scholarship, and the nature of the intellectual enterprise generally. Both implicitly and explicitly Muller attacks most of the assumptions of both social scientists and the general public about the nature of the contemporary social world, and in so doing illustrates the extent to which traditional humanism is today the logical ally of antiestablishment politics. His measured conversational tone belies the radical nature of most of his positions, and a careful reading of his essays suggests that we are already deep in a period of ideological flux in which to be truly conservative is to be a revolutionary, since the present ideological and political establishment is wedded to policies and positions which can only result in the destruction of the fabric of our society, culture, and institutions.

Muller's views can best be illustrated by sampling rather than by explication through a series of propositions, because he is not interested in building a system but simply in presenting us with the fruits of his own wide-ranging scholarship and reflection. His essays on "futurology" argue for the need to stress moral values and to view the future as something to be created rather than simply predicted (even were that possible); and he emphasizes the special difficulty of predicting the future of cultural values. He also pointedly notes that when the Commission on the Future, chaired by Daniel Bell, was formed in the late 'sixties it contained no one professionally concerned with ecology, a prime example of failure to confront major issues which would surface before the ink was dry on the reports of its deliberations. He holds that, both for political and environmental reasons, "America is a major threat to hopes of human survival, to my mind even the most dangerous threat today" (p. 15), that economic growth throughout the world as a whole must be brought to an eventual halt, and that technology alone cannot extricate us from the problems we currently face.

While much of his life's work has been devoted to history, Muller argues that history cannot give us the answers we need. "We cannot learn from the past how to live with H-bombs, or how to deal with the ecological crisis, simply because it is only in our generation that men have faced such hazards" (p. 26); he warns that we must especially beware of the misuse of historical analogy: "One example is the common appeal to Munich to justify our aggressive foreign policy, and then specifically our war in Vietnam; whereas there are obviously important differences between prewar Czechoslovakia and the dictatorial, corrupt, unpopular Siagon regimes we maintained in power" (p. 37).

Muller rejects the fact/value distinction so dear to most contemporary social scientists and argues: "In effect, however, the neutrality of specialists in these sciences comes out as a basic conservatism" (pp. 29–30). He assumes that "there is such a thing as human nature" (p. 226) and contends that the American way of life "is in a real sense unnatural" (p. 234). "America today," he declares, "is a really sick society, sick enough to menace hopes of a desirable future" (p. 142). When he attacks the Nixon-Agnew regime (in power as he wrote) as "shallow ... because they fail to see that permissiveness or self-indulgence is at the heart of the American way of life, systematically promoted by free, private, enterprising admen" (p. 155), he is indicting their immediate successors as well.

Muller's final conclusions are not optimistic. Maybe the future can be better, he writes, and there is some reason for hope in the late 'sixties revolt of the anti-establishment young. But his personal postscript and the tone of his whole book is more an elegy to American civilization as he has known it through a long lifetime than a call to arms, despite his stated contention that our problems stem from not caring enough about the future. Readers will react to this volume in terms of their own personal attitudes toward how social issues can be most usefully discussed and their own personal ideological leanings. From my point of view at least, Muller's essentially conservative radicalism is most congenial, and while the reasons on which his conclusions are based are inadequately set forth, the net result comes close to what political theorists in earlier ages would have unashamedly called wisdom.

VICTOR FERKISS

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Bertrand Russell's Philosophy. Edited by George Nakhnikian. (New York: Barnes & Noble Books, 1974. Pp. 278. $19.50.)

This volume consists of fourteen essays written for a symposium held at Indiana University in 1974 to commemorate Bertrand Russell's centenary. Most of the contributions to this collection are arranged in pairs with the second member of the pair written in reply to the first. Of the fourteen contributors, twelve are philosophers, one a lawyer, and one the former private secretary to Lord Russell during the 1960s. As one would expect, the technical philosophical themes within Russell's voluminous writings dominate this book, starting with logical analyses of Russell's (and Whitehead's) *Principia Mathematica* and including discussions of Russell's ontology, philosophy of mathematics, philosophy of mind, epistemology, theories of memory, and his theory of scientific inference.

These chapters, with few exceptions, are of such a technical philosophical nature that I think they have little immediate political relevance and thus will not have much direct interest to most political scientists. It is not until the final two pairs of essays that Russell's political views are discussed, and these concern Russell's liberalism and his involvement with the Peace Movement. Richard Wollheim's essay ("Bertrand Russell and the Liberal Tradition," pp. 209–220) and the brief response to it by George Nakhnikian (pp. 221–226) on Russell's intellectual relation to the liberalism of his godfather, John Stuart Mill, provide a good introduction to Russell's political ideas: his emphasis upon human impulse and desire as determinants of human political behavior; his advocacy of a wide range of reforms regarding the equality of wealth and property, the freedom of women, the establishment of a world government and economic security through the basic wage (see pp. 211–217). However, it is Ralph Schoenman's personal account of Russell's political activism and leadership in the Peace Movement ("Bertrand Russell and the Peace Movement," pp. 227–252) and the response to it by Edward P. Sherman (pp. 253–263) that I think most readers of this journal will find particularly interesting and valuable.

Currently, the American people and our political leaders are attempting to overcome the trauma of American involvement in Southeast Asia (part of which may have required a show of American military force such as that in the Mayaguez Incident to improve the American national image damaged by defeats in South Vietnam and Cambodia). In this political context, Bertrand Russell, the political activist of the Peace Movement, the organizer of The Tribunal on American War Crimes in Vietnam, and the harsh critic of American foreign policy seems especially important.

One issue raised by commentator Sherman consists of whether the shift from Russell's anti-Stalinism and anti-Communism during the cold war to his anti-Americanism of the 1960s was consistent with his liberalism. Did it constitute a distinct radicalization of Russell's political views and the rejection of his earlier liberal position?

This question seems to turn less on Russell's liberalism than on one's notion of liberalism in general and its limits; that is, when does change in the liberal's views become so great as to constitute a transformation of his political theory? This point about the nature of liberalism requires a thorough discussion of the history of liberal ideas, which Sherman omits. Indeed, the required discussion of liberal values and textual support from Russell's own philosophical and political writings for Sherman's claim that Russell's philosophical beliefs were similar to his political views are also absent from Sherman's essay.

Furthermore, the issue that Sherman raises about Russell's political activism being consistent with his liberalism does not address the most significant aspect of Shoenman's contribution. Rather, what is most valuable in Schoenman's account of Russell's later political views and activities is how they related, if at all, to Russell's general philosophy.

The volume has a disjointed quality because its contributors address various wide-ranging subjects within Russell's overall philosophy. Essays in his technical logico-mathematical philosophy are combined with essays in his political thought, reflecting Russell's own sharp division between his philosophical theories and his political views. After describing his philosophic development in another book, Russell writes: "I come now to what I have attempted to do in connection with social questions. I grew up in an atmosphere of politics, and was expected by my elders to take up a political career. Philosophy however, interested me more than politics, and when it appeared that I had some aptitude in it, I decided to make it my main work" ("My Mental Development," in *The Basic Writings of Bertrand Russell,* ed., Robert E. Egner and Lester E. Denonn [New York: Simon and Schuster, 1961], p. 47). Later in the same work, Russell tells his readers that what satisfied him in philosophy was "the extension of the sphere of reason to new provinces" (*Ibid.,* p. 50) previously only vaguely understood. When this vagueness is removed, such matters become "amenable to an exact technique which makes possible the kind of progress that is customary in science" (*Ibid.,* p. 49). But Russell could not completely satisfy himself with technical philosophy alone. Throughout his life he was deeply concerned with the welfare of humanity. Thus he says, "my political interests, though secondary, nevertheless remained strong" (*Ibid.,* p. 47).

The First World War crystallized Russell's political beliefs and formed his great horror of war, prompting his lifelong interest in the political problem of the prevention of future wars. Herein lies the uniqueness of Bertrand Russell. No other thinker in the twentieth century can match his philosophical achievements, which include major contributions in several areas of philosophy, simultaneous with an interest in, writings about, and activism over, political questions. It is the concern for this distinguished characteristic of Bertrand Russell's life and thought that makes Schoenman's essay most important to students of politics.

Russell's appearance as "impulsive, frequently inconsistent, and prone to invoke the most inflamed rhetoric for incompatible positions within the space of a few years and sometimes months" (Schoenman, p. 228) is attributed by Schoenman to Russell's failure to consider his political actions and beliefs from a basic systematic theoretical viewpoint. Russell's preoccupation with an immediate political response to human injustice and suffering seems to have resulted in tolerance for vagueness and inconsistency in his political beliefs, and disregard for the value of careful systematic general political theory – all in contrast with his technical work in philosophy.

What caused this great philosopher so noted for his contributions in the rigorous fields of philosophy to have treated political questions in such an unsystematic way? I can merely suggest some answers by way of additional questions. Was it the scar of Russell's unhappy childhood as an orphan and the resulting empathy he had for others in unfortunate circumstances that made Russell a non-theoretical political activist? Was it, as Schoenman suggests, the conflict of Russell's deep commitment to corrective political action – so deep that he could accept the use of radical methods including mass civil disobedience – with his rather middle-class liberal goals that explains the sharp dichotomy between Russell's philosophy and his politics? Or, is this dichotomy the result of a conflict within liberalism itself? In an essay on the relation between philosophy and politics, Russell, who realized the limitations of philosophy, writes: "The British are distinguished . . . by the excellence of their philosophers, and . . . by their contempt for philosophy. In both respects they show their wisdom" ("Philosophy and Politics" in Egner and Denonn, p. 454). Many political activists would apply this remark to contemporary political theorists who seem to hold politics in contempt. Certainly Russell did not. But he may provide the explanation for this dichotomy between philosophy and politics when he states: ". . . the essence of the Liberal outlook lies not in *what* opinions are held, but in *how* they are held. . ." (*Ibid.*, p. 463).

Does Russell's notion of liberalism necessarily require the rejection of systematic substantive political theory? For illumination of such issues as the nature of liberalism, the relation between liberal goals and the political means to achieve them, and the relation between technical philosophy and political beliefs and action, I strongly recommend the examination of the life and thought of Bertrand Russell. Only a few selections in Nakhnikian's volume, however, are useful for this purpose.

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Edmund Burke: His Political Philosophy. By Frank O'Gorman. (Bloomington and London: Indiana University Press, 1973. Pp. 153. $6.95.)

This second volume in the Indiana University Press series, *Political Thinkers* (the first was Raymond Plant's *Hegel,* 1973) is a slim, critical, carefully documented historical study of Burke as a figure in late eighteenth-century British politics. The author, an historian at the University of Manchester, seeks to reconstruct the environment out of which Burke's thought emerged and thus to balance the many interpretations which distort it, he thinks, through a lack of proper historical orientation.

Mr. O'Gorman appears to have made use of the whole corpus of Burke's writings, and he quotes liberally from the *Works,* the *Correspondence,* and the *Speeches.* There are few secondary references, although the volume closes with a brief critical bibliography. The helpfulness of the footnotes is enhanced by references to precise pamphlets, letters, or speeches as cited. The book is organized into an introduction dealing with the early writings (to 1765), six chapters which cover in roughly chronological order Burke's thoughts on party, constitution, empire (America, Ireland, India), and revolution (France). There is a brief recapitulation in the concluding chapter.

The thrust of Mr. O'Gorman's criticism is aimed at authors (on this side of the Atlantic) who have placed Burke within the natural law tradition, paying insufficient attention to the "ambiguity" and infrequency of Burke's explicit references to natural law (p. 13). The author regards some of these references as politically motivated maneuvers (e.g. in connection with the Popery laws and the trial of Hastings).

In the second place, Mr. O'Gorman has in mind a variety of interpreters who have seen Burke as a systematic political theorist, as the father of the two-party system, or as a prophet of responsible government and a commonwealth of nations. None of these, he argues, can be substantiated from the historical record. In fact, Burke's "method was characterized by a fund of common-sense, practicality and a distaste for abstract theorizing" (p. 67). He "was always ready to expand his vision and to incorporate into his thought new circumstances and new situations" (p. 93). "His political philosophy is the disputation of a public figure constantly engaged in controversy and conflict. It was evolved in the press and on the floor of the House of Commons, in public and in party" (p. 143). Consequently Mr. O'Gorman sets aside all interpretations of Burke's thought which do not derive from the record of The Man and His Career (a phrase which would have made a more appropriate title for this book). The author's intent is to catch the richness and complexity of the historical Burke's personality, his interests, and his predicaments. He is more interested in *who* Burke was than in *what* his significance might be in the history of political philosophy. Mr. O'Gorman wants us to think of Burke as a contentious figure embedded in a series of escalating political struggles by which he was himself shaped.

All of this may make for good history, but is it good political philosophy? A reader attracted to the book by its title and expecting an interpretation by which to test his own assessment of Burke's "contribution to the development of political philosophy" (p. 147) is likely to be disappointed. In the first place, Mr.

O'Gorman reduces political philosophy to an instrumental role, missing its dimension as a timeless and disciplined inquiry into and conversation about political things. For him political philosophy is but a set of theories (of party, of constitution, of toleration, of empire, of revolution and counter-revolution, in Burke's case) which keeps political action from degenerating into mere expediency (pp. 142, 144). Such a narrow construction turns Mr. O'Gorman's true-to-life portrait into something of a caricature of Burke's place in the history of the discipline of political philosophy. By confining himself to the question, "Who was the historical Edmund Burke?" the author neglects and risks distorting what Edmund Burke was and has been in political philosophy. Hardly anyone would quarrel with Mr. O'Gorman's fair and scholarly summary of Burke's career. But the question remains: Do the results of this study warrant such a general debunking of efforts by political philosophers to assign significance to Burke's premises and conclusions? To regard them as comparable in "arbitrariness" with those of the Jacobins, as Mr. O'Gorman does (p. 107), seems hardly justifiable. Moreover, to find "ambiguity" in Burke's pronouncements on natural law is to overlook that steady, if unexpressed, commitment which qualifies him to be a major representative figure in the critique of modernity.

A famous man of the eighteenth century opposed to the Enlightenment? That is news! Is it so historically irresponsible for some twentieth century men opposed to secular humanism to find Burke's example heartening in circumstances so parallel to his? The inspirational and exemplary contribution which Burke makes to the critique of modernity does not so much oppose the historical record as transcend it. Is Mr. O'Gorman entitled to call this contribution into question on the grounds of historicity? The political scientist certainly stands to benefit from good history. But for him the more interesting questions about a man like Burke concern his significance rather than his achievements and failures. Mr. O'Gorman hints at this significance in passages such as these: Burke regarded certain principles "as fixed and unchanging, beyond the power of man to alter or amend" (p. 51); he "did not wish to concern himself with abstract rights divorced from political, social and historical realities" (p. 68); he saw "the dangers inherent in most forms of political activism" (p. 142). But the bulk of his biographical analysis drives toward conclusions such as these: Burke "completely failed to read the direction in which history was moving" (p. 66); he "lacked the detachment" to be "a critic of the revolution in the Europe of the 1790s" (p. 141); his "most basic assumptions amounted to little more than an unthinking acceptance and reiteration of some of the traditional themes of political and intellectual life which had become embedded in the European consciousness" (p. 144); he "was so anxious to preserve the fruits of the past in the present that he was unable to look to the future" (p. 146). The reader can determine for himself whether it is Burke's representative significance or his actual biography that makes him an important figure in the history of political philosophy. Then he may judge Mr. O'Gorman's success in giving a balanced account of Burke's impact.

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Jeremy Bentham: Ten Critical Essays. Edited by Bhikhu Parekh. (London: Frank Cass, and Portland Oregon: International Scholarly Book Services, 1974. Pp. xxvii, 204. $15.00.)

Mr. Parekh provides an introduction, some notes, and one of the essays. Apart from that essay and the contribution from Professor J. H. Burns, which is a revised version of a paper presented to the 1964 Conference of the Political Studies Association of the United Kingdom, all the other essays have been published before, although some have been revised for this volume, and some are not very readily available.

The editor's introduction does not discuss the other essays, but gives a general account of varous aspects of Bentham's thought which is likely to be of more interest to readers who already have some acquaintance with Bentham's works than to those who are approaching them for the first time. Newcomers, indeed, may not understand the references to the Bowring edition in the notes at the end of the introduction. They may also wonder what to make of the fact that Mr. Parekh cites the *Deontology,* although in the very first essay, John Stuart Mill's "Bentham" from *Dissertations and Discussions* (1859), they will find that Mill mentions "almost the only passage of the 'Deontology,' which from its style, and from it having before appeared in print, may be known to be Bentham's" (p. 13). They will further see that in footnote 15 to Mill's essay Mr. Parekh himself points out that when it first appeared in *London and Westminster Review,* Mill had written instead of "the 'Deontology,' " "Bowring's 'Deontology.' " (Moreover, if they turn to the Essay in *Dissertations and Discussions* entitled "Dr. Whewell on Moral Philosophy" they will find Mill saying there – "the 'Deontology' was not, and does not profess to be, written by Bentham" (Vol. II, p. 463).)

Mill's Bentham is too well known to require comment here, and the next two essays, extracts from William Whewell's *Lectures on the History of Moral Philosophy* and chapter VII of John Watson's *Hedonistic Theories from Aristippus to Spencer,* will be of interest mainly to readers who are concerned with nineteenth-century philosophical debates; although it is understandable that they should have been included when the editor's aim was presumably to publish a representative selection of essays from Bentham's time to the present. The essay that follows, "Bentham" by Professor H. L. A. Hart, usefully draws attention to the need for reconsidering Bentham's psychological position in the light of recent analyses of pleasure and pain. It also points out how much work remains to be done on other aspects of Bentham's thought, on, for instance, language and logic and the analysis of rights. Mr. Parekh's own essay considers Bentham's justification of the Principle of Utility. The approach is again that of the moral philosopher rather than of the political theorist (which is perhaps why Bentham is quoted (p. 104) as stating that the "fabric of felicity" is built "by the hands of reason," and the words that immediately follow in the first paragraph of the *Principles,* "and of law," are omitted). Mr. Parekh also claims, rather darkly (p. 116), that Bentham gave no reason for holding that "personal and political life should both be regulated by the same principle."

Next, in "Bentham's Ideal Republic," Thomas P. Peardon provides a clear summary of the *Constitutional Code;* and in "Bentham on Sovereignty," a further essay by Professor Hart, the differences between Bentham and Austin are explored. There follows an entertaining essay by Professor J. H. Burns on "Bentham's Critique of Political Fallacies," in which the changes in Bentham's political attitudes are well set out and attention is drawn to Bentham's reactions to those changes, which, as Professor Burns says, were never more engagingly expressed than when he scribbled on an old sheet of manuscript the bewildered query, 'What can this be? Surely this was never my opinion!' " (p. 155). Professor Hart had earlier indicated that he shares (as I do) Professor Burns's appreciation and admiration of Bentham's exposure of fallacies; but neither points out that on occasion Bentham himself, for instance when writing on indirect legislation, was not above commending a device that he condemned in the *Handbook of Political Fallacies* (ed. Harold A. Larrabee [Baltimore: Johns Hopkins University Press, 1952, pp. 145ff]): sophistical "Imposter Terms," such as the substitution of "zeal" for "persecution," appear to be approved of in the *Theory of Legislation,* where the convenience of using "public prosperity" instead of "luxury" is indicated when one engages in the apparently respectable "art of leading public opinion without the public suspecting how it is led" (R. Hildreth translation of E. Dumont's French version of Bentham's ms. [London: Kegan Paul, Trench, Trubner, 1900, p. 433]).

The two last essays, by Wesley C. Mitchell on "Bentham's Felicific Calculus" and David Roberts on "Jeremy Bentham and the Victorian Administrative State," help to put Bentham in perspective. Mitchell gives good reasons for holding that Bentham "came nearer to being the Linnaeus than the Newton of the moral world" (p. 182), and Roberts sums up well the evidence against the exaggerated and over-simplified picture of Bentham's influence presented by A. V. Dicey and others: "when each particular reform is closely scrutinized, Bentham's ideas seem less and less to have been the decisive factor" (p. 198). What is to be admired is rather "the foresight, the clarity and the logic with which he expressed the truths which other forces, far stronger than his own ideas, would bring to pass" (p. 199).

Political theorists who are interested in Bentham and who have not already collected copies of the previously printed material will find it useful to have this book on their shelves, even though it is presumably not the critical study in progress that was mentioned by the publishers on the dustcover of Mr. Parekh's *Bentham and Political Thought* [New York: Barnes & Noble, 1973], reviewed in the *APSR* in September 1976.

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Psychological Needs and Political Behavior: A Theory of Personality and Political Efficacy. By Stanley Allen Renshon. (New York: The Free Press, 1974. Pp. 300. $10.95.)

This book expands the literature of political psychology by providing an eclectic and reasonably persuasive argument that a psychological need for personal control has important consequences for political participation. The author draws widely on such notions as the "effectance" motive observed in children striving for autonomy, Rotter's social learning theory, the Survey Research Center's venerable political efficacy construct, and a sketchy reading of behaviorist psychology. He accomplishes this theoretical task in only thirty pages including a review of some literature on participation and political culture; in an additional fifty or so pages he browses in developmental psychology, animal studies, Freudian theory, and humanistic psychology. Sweeping, yes. But it is more a review of relevant sources than a serious attempt at integration or theory development. Researchers and theory builders seeking to follow up the outline provided in this book should be prepared to fill in much of the detail themselves. The sketchiness of Professor Renshon's approach seems reasonable, though, if we accept the fairly modest and tentative goal he sets for the book; namely that "the most important thrust of this research lies in the relationships uncovered" and in pointing out "indicators of important future research possibilities" (p. 4). Readers keeping that statement in mind will find the book useful, though for reasons to be discussed below they should be wary of uncritically accepting certain of the "relationships uncovered."

The data for the empirical analysis consist of two separate samples of university students taken during (1) an "unstructured period" during which the university mildly encouraged political participation (1970), and (2) the march on Washington in 1971. Using a framework strongly derivative of Julian Rotter's social learning theory, the author argues that personal control and participation are linked when the individual perceives that the political system is instrumental to his/her preferred outcomes (e.g., needs, values) and that action is needed to influence those outcomes. This appraisal establishes the "control-relevance" or salience of the system. Resulting behavior then is mediated by the expectancy that influence attempts may be successful. Thus, Renshon combines a quasi-phenomenological approach with an "expectancy-value" or instrumentality theory. The approach leads him to a re-analysis of the political efficacy scale and the eventual adoption of what he claims to be a modified version of Rotter's internal vs. external locus of control (I-E) scale as the key dependent variable. Oddly, though, little time is spent on the empirical validation of the modified scale and no item statistics or estimates of reliability are provided. This creates some serious difficulties in the specifically *political* portion of his analysis and leaves considerable doubt whether the dependent variable, on which virtually the entire analysis hangs, is adequate for the uses to which it is put. I will suggest that it is not, but first we should note some of his conclusions that seem fruitful new research areas.

Using his scale of Personal Control (PC), Professor Renshon finds that it is associated with autonomy while growing up, trust in parents, parental empathy, and birth order. On the personality level, PC is related to interpersonal trust, life satisfaction, and political trust. With the exception of political trust, his conclusions with respect to socialization practices and family nurturance patterns do suggest an important link between childhood, personality, and political behavior. He has – I think rightfully – argued that personal control may be an important mediating variable between one's general social experiences and orientations toward political participation. His data from both students and their parents seem quite appropriate for this kind of analysis. Perhaps my only quibble with this part of his data work is that some of the tables using Multiple Classification Analysis (MCA) have some statistical anomalies that are confusing. For instance (e.g., pp. 150, 203) the beta coefficients often balloon drastically compared to their comparable eta coefficients. Since it is explained that beta is roughly equivalent to partial correlation and eta to zero-correlation in parametric statistics, and since partial *r*'s generally are smaller than zero-order *r*'s, some explanation seems necessary. This is particularly desirable since the mysteriously growing partials (betas) figure significantly in his conclusions. This seems quite a trifling matter, however, compared to difficulties encountered in the analysis of political variables.

The main difficulty with the political analysis is that Renshon's modification of the Rotter I-E scale carries much of its rationale, some of its items, and a sometimes unfortunate multidimensionality of Rotter's original scale. A growing number of studies shows that the I-E scale contains separable subscales which may not covary precisely. Phares, in his monograph "Locus of Control: A Personality Determinant of Behavior" (General Learning Press, Morristown, N.J., 1973) cites studies by Mirels (1970), Gurin and her associates (1969), and others who, writing before Renshon, found reason to question the I-E scale. With variations, the various researchers seem to find components of achievement ideology, success in interpersonal relationships, and perceived impact on sociopolitical authority structures (government, politics, war powers, corruption). The last component is conceptually separable from the others and, if incorporated in a personal control scale, will lead to spuriously high correlations with other scales purportedly measuring political attitudes. In other words, a certain amount of association between the I-E scale and political affect scales is virtually built in. That the sociopolitical component can subvert many analyses is suggested by Phares's statement that changes in college students' I-E scores over time "can be accounted for by the several items on the I-E scale which refer to the political or governmental sphere" (p. 18). Now, if multidimensionality is a problem in the I-E scale for political science purposes, what implication does that have for Renshon's analogous Personal Control scale? Unfortunately, a lot.

Inspection of the PC scale items (p. 273) shows that at least four deal with that dimension (world affairs, Vietnam, government) and could therefore be expected to correlate with similar items in the political trust, alienation, and political affairs scales that Renshon employs in his analysis. Therefore any correlation obtained can only be accepted with great skepticism or great faith. I favor the former, though a re-analysis of the data with better dependent measures could convince me that the political attitude conclusions may hold. Ironically, Renshon has the makings of a coherent personal efficacy scale in a set of semantic-differential-type items (p. 269) relating to the individual's self-orientation. Those items would have made a better dependent measure than the one employed. If such a scale were developed and did *not* correlate with the Renshon's PC scale, the whole political affect section should be suspect; if there were a substantial correlation, my criticisms would be allayed. But the empirical answer to these comments seems not to be found in the book. If it were, I would be spared having to criticize a research work with which I am in basic sympathy and which I recommend for political scientists who are prepared to accept its hypotheses as challenges; however, they should treat many of its conclusions gingerly, if not with outright skepticism.

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The Seventh Hero: Thomas Carlyle and the Theory of Radical Activism. By Philip Rosenberg. (Cambridge, Mass.: Harvard University Press, 1974. Pp. 235. $10.00.)

Rosenberg's *The Seventh Hero* has a twofold purpose. First, it attempts a radical interpretation of the important writings of Thomas Carlyle. Second, on the basis of a radical Carlyle, Rosenberg outlines a new theory of radical activism (p. xi).

The dominant view challenged in this study sees Carlyle as a mystical conservative. In order to make his case, Professor Rosenberg must account for three opposing positions. First, he must show that Carlyle's praise of at least some features of Chartism, aristocracy, the "Captains of Industry," and even slavery is incidental to the deepest stratum of Carlyle's thought. Second, Rosenberg needs to rescue Carlyle from a total, mystical abandonment of politics; Carlyle is famous for his comparison of politics to patent medicine, a fake cure for the truly incurable. Lastly, Rosenberg is forced to argue that Carlyle's political "solution" of "A Whole World of Heroes" (pp. 176–203) is not ironic or absurd and further that it is adaptable to a political radicalism based on human equality – a position uncongenial to Carlyle.

The radical theory which emerges on the basis of the author's skillful interpretation of Carlyle is as follows. First is the rebel band, ready "to renounce all normal ties to society and to turn their backs on what they have been taught to see as legitimate order" (p. 197). Second is the charismatic hero who can maintain the loyalty of the rebel band through the quality and integrity of his leadership. Apparently the *sansculotte* revolution in France died because it lacked such leadership. The cultural revolution in China, on the other hand, illustrates the case not only of a charismatic revolution that was successful but also of a charismatic leader who was able to avoid the bureaucratization of his rule (p. 198).

As the last example shows, Rosenberg is aware of the major charge to which such a radical theory is open. It appears, if nothing else, patently tyrannical or totalitarian (p. 198). The defense from this charge is prepared by Rosenberg and not Carlyle (p. 123). Reversing common scholarly opinion, Rosenberg argues that regimes founded on constitutional "contracts" and natural rights have a greater tendency to absolutism than those maintained by charismatic rebellion. Constitutional governments are founded on a transhistorical basis which makes them over-legitimate (p. 115); charismatic rebellion, on the other hand, is responsive to historical flux and therefore able to adapt to circumstances. "And when these traits [committed rebel band, charismatic leader] are combined with a conscious, theoretical commitment to charismatic rebellion itself, the dangers of a lapse into totalitarianism are, at the very most, no greater than they inevitably must be so long as the means of massive coercion exist" (pp. 198, 122–123).

The major problem of the book is not whether this claim is radical. Plato made as radical a claim when he said that until philosophers become kings and kings philosophers, the evils shall not vanish from the cities. But in developing his utopia ("nowhere"), Plato presented a model of political analysis of such thoroughness that the problems inherent in the *Republic*'s perfectly just city became clearer. Rosenberg, on the contrary, raises a fundamental question of political philosophy, and then fails to explore its implications. An adequate discussion of "charismatic rebellion," it seems, should include an analysis of political succession, parties, administration of public policy, education and voting or consent, to name but a few important aspects of political life.

Rosenberg is not unaware of the distinction between theoretical rigor and theoretical superficiality, and indeed, he criticizes Carlyle for superficiality or incompleteness (p. 100). Nevertheless, he makes the same mistake when he fails to challenge the positive reconstruction of Carlyle's thought in his concluding chapter, "A Whole World of Heroes."

Rosenberg does not prove, as he wished to do, that the older liberal tradition is inadequate and antiquated. A student of totalitarianism can still learn from such works as the *Federalist Papers* about the desirability of a rule of law based upon decentralization, separation of powers, a judiciary holding office during good behavior, and the other substantive and "auxiliary" precautions there set forth. The presidential resignation we have recently witnessed may be seen as an example of the moderation of the present constitutional system.

On balance, Rosenberg's account of Carlyle's radical activism is original and well done. Still, one wishes that Rosenberg had included *Cromwell* and *Frederick the Great* in his analysis of Carlyle, and that the overall execution of *The Seventh Hero* were more consistent. While the description of the French Revolutionary theorists is excellent, the summaries of modern natural-right and economic theory are too eclectic (pp. 108–123). There are even a very few instances in which these summaries are misleading, e.g., the absence of Locke from the discussion of the relation of property and contract (p. 117). But the major problem of *The Seventh Hero* remains the unrealized goal of the book – a new (and adequate) theory of radical activism.

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Testing Theories of Economic Imperialism. Edited by Steven J. Rosen and James R. Kurth. (Lexington, Mass.: Lexington Books, 1974. Pp. 284. $16.50.)

This is an unusually interesting collection of papers, some of which were given at the International Studies Association convention in 1973, some at the International Political Science Association congress in Montreal, and some at the Peace Science Society of North America convention in Boston. The average quality of the papers seems to be substantially higher than is usual in a symposium of this kind. This is perhaps no doubt the work of the editors and also of the interest which this subject is arousing among concerned scholars. The subject is one with high ideological components, especially on the left, so the problem of testing becomes particularly acute. None of the authors, I think, are particularly optimistic that ideological conflict can be resolved by testing. Any question that can be resolved by testing, like the composition of the periodic table, does not become a subject of ideological conflict. It is precisely the untestables that are the components of ideology. Nevertheless, as we move towards testing procedures which are generally agreed upon, one hopes at least that the ideological components of conflict will shrink, even though they may never completely fade away.

The first question, of course, is what are we testing? Imperialism is a concept which though not many-splendored is at least hydra-headed. About the only thing that we all agree on is that nobody likes it, at least in the modern world. It is on all sides a term of abuse. What it is that we are accusing each other of, however, is by no means clear. This volume does help to clear up the field of controversy in considerable degree. The first part, entitled "The Theory of Economic Imperialism," indeed is mainly concerned with this. After an introductory essay by James Kurth, one of the editors, Karl Deutsch gives a masterly exposition of four folk theories and six "serious" theories: the Conservative, the Liberal, the Marxist, the Schumpetrian, and two "Dependencia" theories – one by Johan Galtung and one by Samir Amin. In a final flight of fancy at the end of the essay, Deutsch identifies what he calls the Spanish-Muslim culture complex as the principal obstacle to development, and therefore by implication, if imperialism is differential development (i.e., that some people or groups get richer faster than others), a prime cause of what looks like imperialism. While there are many attempts in the rest of the book to test some of the other theories, this one is never tested.

Andrew Mack looks at somewhat the same field from a slightly more left-wing view, and Thomas Weisskopf looks at it from an extremely left-wing view, giving a picture of virtuous, socialist, nonimperialist America which the editor himself describes as "utopian" and which I found, I must confess, unbelievably naive, so much so that I was almost tempted to regard the whole thing as a parody, especially as it comes out of what used to be the hardheaded University of Michigan. The final essay in this section, by James Caporaso, is an extremely useful exercise in the problems of measurement of inequality, dependence and exploitation which the author regards as the principal hallmarks of imperialism, each being a necessary element in the concept.

Part two of the volume is a series of six essays on the empirical testing of theories of capitalist imperialism. There is an excellent piece by the editor, Steven Rosen, presenting an historical study of what happened before and after political changes in Brazil, Chile, Indonesia, Greece, and Peru. The evidence as usual is a bit conflicting, and the conclusions are not quite as clear from the charts as is asserted in the text, but still suggest that political changes do affect the course of trade, aid, and investment in ways which are not wholly surprising. Oddly enough, there is no study of Cuba anywhere in the volume, which one would have thought was a very interesting, extreme case. John Odell then relates U.S. military assistance and military intervention to U.S. economic interests, the first being somewhat related and the second not related at all, which gives a little comfort and aid to both sides of the argument.

Theodore Moran explores the relationship of the large corporations and the Chilean middle class in the bargaining position of Chile and develops an interesting theory of rising bargaining power on the part of the material producers, somewhat at variance with the older view on this matter. Stephen D. Krasner has a more general essay on the raw material producers, arguing that on the whole they can only organize themselves for economic purposes in alliance with the large corporations. Donald E. Milsten has a rather sketchy essay on exploitation in ocean resource development, and Walter Goldstein has a somewhat debunking piece on U.S. economic penetration of western Europe. The book concludes with two essays on Soviet imperialism. The one by Paul Marer on the political economy of Soviet relations with eastern Europe shows very convincingly the nature of the "reverse Marshall Plan" by which eastern Europe paid to the Soviet Union a slightly larger amount than the United States granted to western Europe in the Marshall Plan. It shows also that since the middle 'fifties, exploitation in eastern Europe by the Soviet Union has been small and that there may even have been some reverse movement. Roy F. Grow has a similar article on Soviet economic penetration of China, showing also that this was very small. Outside of military aid, the Soviet contribution to China is about one cent per Chinese per annum – an amount that has been paid back with interest; the Soviet Union made no grant to China at all. The essay ends in 1960, and since there is no discussion of the break in 1960, the whole argument is rather incomplete.

The overall picture presented by the essays is quite encouraging in the sense that the information processes of the world social system are now getting to the point where some kind of testing of ideological propositions is possible, although we are still a very long way from Daniel Bell's "end of ideology." One of the major difficulties is inadequacy of information on the multinational corporations, especially in regard to reinvestment of profits abroad. Another inadequacy is the dearth of information on the "grants economy" of the socialist countries. I would particularly like to have some information on the "internal imperialism" of the Soviet Union; to what extent do the Soviet "colonies" like Lithuania, Estonia, Latvia, Usbekistan, and so on exploit the Russians, or do the Russians exploit them? There are similar problems, as a matter of fact, in the internal economic life of all countries, and the larger the country the more significant this phenomenon may become. This whole problem of the world grants economy is important from the point of view of the discussion of exploitation, which Caporaso despairs of measuring. If exploitation is defined as illegitimate grants, at least the beginning of wisdom is to find out where the grants go and from whom, to whom.

A careful study of this phenomenon perhaps is the only possibility of testing the major ideological split in this area, which is a split between those who see inequality developing primarily as a result of differential development and those who see it primarily as a result of exploitation. In a crude sense, the exploitation theory seems to be quite untenable. Rich countries are not rich because the poor produce a great deal and the rich take it away from them. Rich countries are rich because they have been getting richer faster and longer than the poor countries. In this process, however, exploitation may play a significant minor role and it is important to find out how much. Cultural exploitation may turn out to be more important than economic exploitation. Cultural imperialism itself is a very tricky problem which is not treated very much in this volume, though there are occasional references to the "*mission civilisatrice.*" Cultural imperialism, indeed, may have as one of its more benign components an equalizing reaction to differential development attempting to eliminate those cultural differences which create different rates of economic growth. On the other hand, from a cultural point of view it can also be regarded as interfering with what is really other people's business. Certainly the *mission civilisatrice* of the Communists is one of the major factors creating pathological conditions among those who do not want this particular kind of civilization. One has the uneasy thought that other similar missions might have equally undesirable results.

The editors are much to be congratulated on a volume which will undoubtedly stimulate a great deal of thought and discussion. My one complaint is that there is no index, which is a particularly unfortunate defect in a volume of this kind.

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Thomas Hobbes in His Time. Edited by Ralph Ross, Herbert W. Schneider, and Theodore Waldman. (Minneapolis: University of Minnesota Press, 1975. Pp. 150. $7.50.)

Five essays have been written for this volume and an old one reprinted. The common theme is "Thomas Hobbes in his time" as opposed to recent interpretation which is said to place him in our time, as if his questions were the same as ours. It is of course characteristic of our time to make this criticism, and the five living authors do not, for the most part, specify their differences with the recent Hobbes literature (a useful bibliography is furnished) or insist on their own superiority. The pace is easy, the tone civil, and the argumentation relaxed. But we are not informed by them whether Hobbes "in his own revolutionary century" (p. 3) made his own revolution or merely got caught up in the general enthusiasm.

If the book had no other virtue, it would be enough to have reprinted John Dewey's essay "The Motivation of Hobbes's Political Philosophy," first published in 1918. According to Dewey, the center of Hobbes's thought is his theory of sovereignty. It must be seen as a solution to the conflict of church and state in Hobbes's time, not in terms of the conflict between individual freedom and public control in our time. Dewey points out (with the aid of Figgis) that the popular side in the English Civil War, including the Levellers, held to divine right as much as the royal side; and Hobbes's theory of sovereignty was distinguished by its "deliberately anti-theological" intent (p. 11) against both sides. His individualism, Dewey says, not the popular sovereignty of his opponents, was the source of Locke's individualism. One need only add (what Dewey implies) that "the people" as they were in the seventeenth century had to be secularized before we today would have approved of their sovereignty – and a clear warning is sounded to historians and political theorists who with impatience and partisan favor salute every manifestation of democracy, even the theocratic.

As priests (no matter how many they are) must be subordinated to the sovereign, so must lawyers. Dewey finds the "specific empirical sources" (p. 20) of Hobbes's political philosophy especially in *Behemoth* and *A Dialogue between a Philosopher and a Student of the Common Law.* Then he sees Hobbes's science as "the sole alternative to the continuation of the troubled regime of opinion" (p. 23) in which ambitious priests and lawyers presume on their private, professional doctrines to claim privilege, power, and immunity. The universities have been centers and missions of corrupt opinion, but they can be induced to teach true moral and civil science so that the cure makes its way through the channels of infection. Hobbes's science becomes the safeguard of his sovereign while redeeming its arbitrariness, for the same teaching that forbids the subjects to raise seditious claims forcibly counsels the sovereign not to provoke them. Since Hobbes places the common good unquestionably above such claims, Dewey calls his position "a thoroughgoing utilitarianism."

The other authors are perhaps closer to each other than to Dewey, as they either do not focus on the religious issue or take a contrary view of it. Craig Walton's "The *Philosophia Prima* of Thomas Hobbes" considers the relative status of natural and civil philosophy in Hobbes's thought, especially in regard to his concept of endeavor. "Endeavor" is a bodily reduction of the soul's motion which substitutes for teleology in his *philosophia prima* and thus helps him to ground civil philosophy in natural. Ralph Ross, with "Some Puzzles in Hobbes," says that Hobbes's doctrine "posits a coherent world"; and instead of beginning from its "empirical sources" as did Dewey, he claims that in studying Hobbes "we too easily forget the enchantment of a vision of total explanation. . ." (p. 59). Yet he finds "some puzzles," not to say "gross contradictions" (p. 55). One of them, he says, is that it makes no difference whether in the state of nature all men have a right to all things or no one to anything. But in the latter case all right would be conventional, and the exchange that Ross imagines would occur for the immediate advantage of the stronger. Moreover, if right is conventional, Hobbes would have no basis in nature for defending men against the claims of divine right. It is only today, when theocracy no longer threatens, that people think it is possible to stand on nothing and repel divine right.

In "Hobbes on the Generation of a Public Person" Theodore Waldman argues that Hobbes "recognizes the truth of the Greek view of man as a political animal although he explores it in his own way" (p. 70). He qualifies the usual understanding of Hobbes as an individualist (on which Dewey had laid emphasis) by bringing up the fact that for Hobbes, commonwealth is a great boon, not a necessary evil, to mankind. But this is not the issue, since it is clear that without commonwealth Hobbes's individuals are in a state of war, not of self-sufficiency or self-absorption. The issue is whether men are *by nature* political, which Aristotle (not any casual Greek) affirms and Hobbes denies (*Leviathan*, ch. 17), for this decides whether men are capable of living together in virtue as opposed to commodiously. On this score, Waldman's attempt to find self-sacrifice in the individual's authorization of a public person does not seem successful. To curtail one's natural desires (p. 75) out of worldly fear or interest is not sacrifice.

The last two essays put greater trust in Hobbes's professions of piety than did Dewey, who apparently believed in some genuine sense that Hobbes "believed in some genuine sense that God was reason" (p. 27). In "The Piety of Hobbes" Herbert W. Schneider admits that Hobbes's morals and politics are clearly stated in the secular parts of his writings; but, he asserts, "the fact remains that none of his writings are merely secular" (p. 85). To dismiss the extensive nonsecular parts "as mere protective coloration or social security seems preposterous. . ." (p. 99). Someone might want to know why, but he is not likely to disagree that Hobbes was as religious as his old employer Bacon.

Paul J. Johnson's "Hobbes's Anglican Doctrine of Salvation" presents the more specific thesis that Hobbes floated in the mainstream Anglicanism of his time. His narrowing of Christian doctrine to the one thing necessary to believe for salvation (that Jesus is the Messiah) is likened to the Broad Church distinction of Hales and Chillingworth between the fundamentals and accidentals of faith, and it is even traced to Hooker. To do this Johnson found it necessary to argue away the sarcastic tone of Hobbes's remarks on religion, and with more space he might have considered his polemic with Bishop Bramhall. He asserts there is nothing skeptical in Hobbes's definition of true religion (invisible power truly such as we imagine) even though he quotes Hobbes's statement that God is inconceivable. If God is inconceivable, how can He be truly imagined? Johnson agrees that for Hobbes "the idea of God played no functional role" in metaphysics, psychology and politics (p. 124), but he does not say what is left for God to do. He rests his case on an opinion characteristic of our day, and not of Hobbes's, that God can be ignored without being denied. In judging the thesis of an interesting book, this reviewer was confirmed in his belief, here practiced by Dewey, that knowing Hobbes is the way to knowing his historical climate, rather than the reverse.

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Planning and Organizing for Social Change: Action Principles From Social Science Research. By Jack Rothman. (New York: Columbia University Press, 1974. Pp. 628. $20.00, cloth; $7.50, paper.)

This book is the first major product of the University of Michigan School of Social Work's NIMH-sponsored Community Intervention Project. *Planning and Organizing for Social Change* is both an important contribution to the applied science of planned social change and an innovative methodological landmark in the retrieval and application of social science research. Seeking to bridge the gap between the concerns of the social sciences and the problems that challenge social planners and community workers in the human-service fields, Rothman and his research team retrieved and processed 921 research reports published between 1964 and 1970 (of which 578 make up the data base for this book). Most of the retrieval effort involved a detailed search of thirty major disciplinary and professional journals. This core effort was augmented by a search for "fugitive reports," dissertations in social work, and papers delivered at sociology and social work conferences. The result is a propositional inventory leading to testable short-run "modalities of intervention" or action guidelines that suggest strategies for practitioners designing incremental social change programs.

Planning and Organizing for Social Change is organized into seven parts, eleven chapters, and two short methodological appendices. After a brief Foreword by Richard A. Cloward, Rothman introduces his study with an intriguing explication of his personal values and perspective on social change. In chapter one he discusses the purposes and methodologies involved in research utilization and the uniqueness of his approach from other propositional inventories generated in the social sciences. Parts Two through Six (chapters two through ten) contain the major portion of the book and are organized to cover three "social aggregate levels" – practitioner roles, organizational factors and political processes – citizen participation, and two change processes that affect community practice; the diffusion and adoption of innovations, an artificial or induced process, and the movement and assimilation of populations, a more "natural" process. In chapter eleven, Rothman presents a six-stage, five-step model of the research utilization process which guided the project's overall retrieval and dissemination effort and serves to organize a lucid discussion of the utilization gap between social science research and practitioner behavior.

The most innovative, difficult, and ambitious contribution is the development and presentation of action guidelines "which are intended to translate generalizations into their logical applied formulations" (p. 28). In developing these guidelines, Rothman and his team grouped findings into middle-range generalizations and, based on their judgment of the "content and configuration of findings" (p. 28) in the literature, assigned support ratings to each generalization. In order to keep the cognitive leap from declarative statements to imperative prescriptions small, the team "stayed close to the findings" while employing "inference, conjecture, and imagination" in developing the action guidelines as "emergent intervention hypotheses for future research" (pp. 31–32).

In discussing the gap in the research utilization process, Rothman argues that the fundamental difference between social scientists and practitioners is based in their different functions, i.e., the "social scientist has the primary function of *comprehending the world*" while the practitioner has the key function of *changing the world* (or more specifically, parts thereof)" (pp. 545–546). To enhance research utilization, Rothman proposes the development and encouragement of individual linking agents and formal linking institutions having a mix of social science and practitioner competencies. Given the activities and plans of the Community Intervention Project (some of which are discussed in this book) to field test and disseminate findings in manual form, one is persuaded that Rothman has created a prototype of such a linking institution that will serve to test both his action guidelines and his research utilization model in an institution-building experiment.

Despite my enthusiasm, I find this book is not without its flaws or weaknesses. For example, the nine substantive chapters are of uneven depth and coverage. But this represents the state of the literature rather than any failing on the part of Rothman and his team. Also, because the generalizations are inductively generated, their presentation in the substantive chapters often reveals the lack of an overall integrating and organizing framework. In addition, the data base is more than four years old and, therefore, cannot represent some emerging research trends and issues. Finally, some of the generalizations are commonsensical; some of the action guidelines are vague and equivocating; and the scheme for weighing the support for generalizations is imprecise.

Nevertheless, one cannot help but admire and be impressed by the scope and the effort involved in this project. Rothman and his associates have created a most useful encyclopedic handbook and a new research direction for students of applied social change.

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The Political Criminal: The Problem of Morality and Crime. By Stephen Schafer. (New York: The Free Press, 1974. Pp. 179. $7.95.)

Among the more encouraging post-Watergate revelations is the realization that crime is too serious and pervasive a problem to be left only to politicians, lawyers, judges, and police – especially when it is they who too often make as well as break the law. Perhaps our lawmakers, law interpreters, and law enforcers should reflect upon the questions raised by Professor Schafer. What is political criminality?

How is morality related to the formal codes comprising the criminal law? Is the traditional definition of political crime broad enough to cover the multitude of more recent and less visible acts of violence against society?

At a time when concern with values, norms, ethics and (gasp!) morals is branded as unbehavioral, the author squarely addresses these questions as well as the one that everyone is avoiding: can the actions of a political criminal be legally wrong and at the same time morally right?

Using impressive references covering over 300 years of social, criminological, legal, and political writings on his subject, Schafer argues that political crime is the oldest yet least understood form of criminal behavior. Recognizing the complexity of his task, the author asks the reader to forgive the uneven, and fragmentary coverage of the subject (Preface, ix). Despite the self-admitted limitations and the lack of empirically verified referents, this compact book provides a valuable historical introduction to the literature on crime and is a useful addition to an area lacking in-depth treatment.

Professor Schafer acknowledges the difficulties inherent in specifying what the term political crime means. In Schafer's judgment, contemporary definers of the concept have been too eager to avoid the problem of morality, which he feels is "the most crucial issue of political criminality" (p. 133). Arguing that an understanding of political crime is at the crux of the larger problem of understanding all crime, Schafer relates morality and political values to the criminal law. To him, criminal law is the codification of interests, norms, and values that society wishes to protect, and it is used by the state to declare certain acts to be crimes and to "exercise control over accentuated ideological issues" (p. 20).

The book contains five chapters; the first two deal principally with the history of political criminality and its relation to ordinary crime. Chapter 3 confronts the problem of relating morality to the criminal law, chapter 4 summarizes previous theoretical conceptualizations of political criminality, and the last section attempts to "reconcile and correct" past conceptions by proposing a typology of political crime-types: the convictional criminal, the conventional criminal, and the pseudo-convictional criminal.

In contrast to the conventional criminal (who commits a crime primarily for his own personal gain), the convictional criminal is "convinced" of the truth of his beliefs and justifies illegal behavior through "an altruistic-communal motivation rather than an egoistic drive" (p. 147). Although "inadequately socialized" himself, the political criminal's crime "often mirrors the hopes of all those common people who are likewise inadequately socialized to the moral command, but who somehow do not reach the degree of inner conflict that can lead to the action-prompting emotional dilemma" (p. 150). Finally, pseudo-convictional criminals, whose "skills at hiding their opportunistic criminal identity poses the greatest problem for identification of the true political criminal" (p. 155), are considered the most dangerous of all criminal types, "not only because [they] may be able to make innocent people criminals, but because [they] victimize the collectivity" (p. 157).

In a statement as relevant today as it would have been centuries ago, Schafer observes that "many of those originally hailed as heroes have become political offenders and many of those regarded as political criminals have been elevated to the glorified pedestal of heroes" (p. 5). In a statement that is all too familiar in this post-Watergate era Schafer writes: "The political criminal needs the support of the masses but is usually isolated from his followers, who try to achieve something that they think he is advocating" (p. 15).

As well written as it is, the book is not without flaws, including the continuous references to a uniquitous "political-social power" from whom all laws, morals, and values originate. Schafer does not treat the problems of elite misbehavior in detail and ignores the effects of overlapping jurisdictions within a federal system which allow many social-political powers simultaneously to make, interpret and enforce the criminal laws. Occasionally, the prose style is ragged and difficult to comprehend without re-reading. Also, Schafer uses psychoanalytic jargon to explain why the political criminal resorts to "excited emotional confrontations with the ruling power." According to the author, "his emotions are not really caused by a Freudian sublimation of primitive or infantile instinctual drives, but by a misunderstanding ... of moral issues" (p. 112). Finally, the book may have limited classroom usefulness in courses other than legal philosophy and political theory. And, regrettably, it may receive least attention in criminal justice programs and law schools, where future police, lawyers, prosecutors, judges, and politicians would benefit most from a thorough reading of its contents.

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The Judicial Mind Revisited: Psychometric Analysis of Supreme Court Ideology. By Glendon Schubert. (New York: Oxford University Press, 1974. Pp. 183. $9.00, cloth; $4.95, paper.)

The Judicial Mind Revisited represents the culmination of a methodological development in the study of judicial behavior for which Schubert laid the foundation in an article in *The American Behavioral Scientist in 1961.* The noteworthy title of the article was "A Psychometric Model of the Supreme Court." An extended model in this form was introduced by Schubert in his article, "The 1960 Term: A Psychological Analysis," in the March, 1962 issue of *The American Political Science Review* and in his article, "Judicial Attitudes and Voting Behavior: The 1961 Term of the United States Supreme Court," in the Winter, 1963 issue of *Law and Contemporary Problems.* Each of the preceding articles was concerned with one term of the Supreme Court. On a much more extensive scale, Schubert's book *The Judicial Mind* – published in 1965 – covered all the terms of the Court from 1946 to 1963. In methodological exposition and amplification, the book revealed aspects that the articles – within the limitations of space – could not present. It is in terms of this development of Schubert's work that *The Judicial Mind Revisited* must be understood.

Basically, the underlying model in the encompassing conceptual framework of *The Judicial Mind Revisited* is characterized by regarding a case before the Supreme Court as a stimulus (*j*-point) and the position of a justice with respect to one or several dominant principles pertaining to the case as an ideal-point (*i*-point). In terms of this model, the position of a judge with respect to the decision is a response to the stimulus, depending on the location of the *i*-point relative to the *j*-point. If the positions of the justices in the case can be properly explained in terms of one principle, both *j*-points and *i*-points can be located in one-dimensional space, i.e., all points – *i* and *j* – can be placed on one line. The votes of the justices in the case then can be understood in terms of the location of the *i*-points relative to the *j*-points on the line. This unidimensional scale also can be represented in the form of scalogram analysis. Neither conceptually nor with regard to applications, however, is the psychometric model limited to the unidimensional case. The *i*-points and the *j*-points can be located by coordinates in two-dimensional space, and their respective positions can be compared on the basis of projections on scale axes within this coordinate system. This approach provides the basis for multidimensional scale analysis. More than two principles – each represented by a dimension – can be introduced in the analysis, and the psychometric model then is conceived in terms of *n*-dimensional space, although – beyond three dimensions – it cannot be represented geometrically, but only by logical and algebraic extension. This approach provides the basis for multidimensional scale analysis in *The Judicial Mind Revisited.*

The extended psychometric model in this book requires a rigorous method for the location of the *i*-points, namely, factor analysis, as well as additional methodological features in scale analysis for relating the *j*-points to the *i*-points. On the basis of the respective positions of the nine justices in the cases before the Supreme Court, it can be determined by factor analysis to what extent the positions of the judges can be explained in terms of salient principles. The "principles" in this sense should be regarded in terms of a tentative orientation, however, for Schubert indicates that the factors serve merely as reference axes, and that more substantive meaning is attributed to the scale axes, which will be mentioned in a moment. Schubert finds that three-dimensional space adequately accommodates his multi-dimensional model. Theoretically, of course, the model can be extended beyond three dimensions. The location of the *j*-points, which represent the cases, in relation to the *i*-points, requires the use of scale analysis in modified form. Scale axes, which can be considered analogues of the cumulative scales represented by scalograms, have to be placed in the multidimensional space defined by the factorial axes in such a way that the projections of the *i*-points on the scale axes correspond – in a statistically acceptable sense – to the rankings of the justices in the respective scalograms. Schubert found an empirical solution that meets the requirements of statistical significance. On the basis of the projections of the *i*-points on the scale axes, relative to the projections of the *j*-points, the analysis offers an unprecedented explanation of the positions of judges in cases and provides an understanding of judicial decision making that otherwise cannot be obtained.

The methodological aspects of *The Judicial Mind Revisited* are of particular interest also in another respect. Originally, Schubert used for factor analysis Thurstone's Centroid Method. In *The Judicial Mind Revisited,* Schubert uses Hotelling's Method of Principal Components or Principal Axes, which is a mathematically more rigorous method. It is interesting to note that Thurstone himself, who was a highly sophisticated advocate of mathematical and statistical methods in psychology, gave one of the best expositions of the Method of Principal Components in his book *Multiple Factor Analysis* and showed its analytical power. He developed his own Centroid Method at a time at which the use of digital computers in the social sciences was not yet available, and at which the application of the Method of Principal Components was prohibitive with respect to effort and time. The factors that are obtained by the Method of Principal Components are represented by orthogonal axes. The meaning of this mathematical attribute is that the factors are regarded as being independent of each other. Since, however, the factors represent – at least tentatively – dominant principles, in relation to which the ideal-points of the justices are located, the factors actually may be related to each other. Such a relationship is mathematically represented by oblique instead of orthogonal axes. For this reason, Schubert also employs oblique factor analysis in *The Judicial Mind Revisited.* Furthermore, Schubert uses "smallest space analysis." This method is similar to factor analysis, in the sense that it locates observed phenomena as points in coordinate space, but it is different inasmuch as it does not make the assumptions about the statistical properties of the original variables and their mutual associations that factor analysis must make. Consequently, Schubert finds this method particularly suitable for examining the positions of the justices in *The Judicial Mind Revisited.* Finally, it was possible to include in this book the later terms of the Warren Court – an empirical consideration that is important for a study that is concerned with a systematic verification of hypotheses. The greatest importance of this book is found, however, in the far-reaching implications for a general theory of judicial behavior.

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The Paris Commune of 1871: The View from the Left. Edited by Eugene Schulkind. (New York: Grove Press, Inc., 1974. Pp. 308. $4.95, paper.)

On the Paris Commune's centenary, in 1971, a conference was held at the University of Sussex on its significance for the Left. This valuable book of documents, edited by Dr. Schulkind, who teaches at Sussex, emerged in response to the heated debates.

The Paris Commune represented a transitory revolution, occurring in a city which industrialization was only beginning to change, at a time when socialist ideologies were only beginning to emerge. Whether the Commune was actually or potentially socialist, in whole or in part, affects its historical analysis and the legend and lessons subsequently drawn from it by the Left. As Dr. Schulkind's introduction makes clear, his selection of documents concerns both, and presents two views from the Left.

The first is from within the Commune. Dr. Schulkind argues that this view, rarely that of subsequent Left interpretations, and too often ignored by historians, is real, and relevant to the Left today. He characterizes it by the conviction that the civil war transcended the Jacobin tradition and represented a proletarian government inaugurating a classless society. He provides valuable documentation, a good deal of it drawn from the rich local level of political clubs, craft unions, and sections of the International. Much of it is presented in English for the first time. Here, and in the Commune's plans for cooperative production, or for a comprehensive nonmeritocratic education system, a distinctive socialism is evident. Nevertheless, as Jacques Rougerie has argued, the Communard socialist was a socialist "of his own time and in his own way" (*Paris Libre,* (Paris, Seuil, 1971), p. 235). Dr. Schulkind's documents illustrate the point well. Like later socialists, the Communard socialist was concerned with a redistributive social justice and workers' control. Unlike them, he was unclear if this meant collectivization of the means of production, and he ignored economic planning. He used little social analysis of the evolution of capitalism and less political strategy. Reflecting the limitations of his historical context, he possessed only the most rudimentary tools of later socialism, political parties, and trade unions.

Communard socialism, drawn from Blanquist "authoritarian" socialism and the "anti-authoritarian" socialism of the French branch of the First International, formed only one part of Communard ideas. Moreover, Dr. Schulkind's documents show that it remained closely related to the Jacobin tradition which was the central influence on Communard thought. This tradition itself recognized a social dimension in its concern for political equality. The social dimension was enlarged when the Commune, responding to national defeat, rejected the existing state and therefore, potentially, the existing social structure. Communard socialism thus formed a distinctive, but not a conflicting view of the events of 1871. It was perhaps the brevity of the Commune, however, which prevented its socialist implications from conflicting with this older political radicalism. As Dr. Schulkind argues, the Commune's sheer existence as the first significantly working class attempt to replace the current state by its own freely elected government, commanded solidarity against the menace from Versailles.

The subsequent significance of this fact provides the second view from the Left. Extracts from the Left between 1871 and 1921 reveal not only the retrospective self-justification of participants, and a mixture of shrewd analysis and polemic from committed observers, but also the Commune's importance for several aspects of the vital debate on the state and revolution. Anarchists cited it against "authoritarian" socialists in the quarrel accompanying the decline of the First International. Bakunin applauded the Commune for rejecting the state, and its socialist minority for attempting to resolve, through the "free association and federation of workers" (p. 221), the ultimate conflict of individual freedom with society. Kropotkin condemned the Commune for preventing such an achievement. Marx, however, considered it a largely nonsocialist and historically limited precursor of the dictatorship of the proletariat. With Engels and Lenin, he saw its revolutionary significance in the implementation of two principles. The first was that the proletariat should break with the bourgeois state and bourgeois democracy, and the second that it should directly and democratically control the new political structure while dismantling the old society. The first was taken up by both Juarès and Bernstein. The second arose in the related dispute of Kautsky with Lenin and Trotsky, over the interpretation of the dictatorship of the proletariat, during the Russian Revolution. The Commune was invoked here to condemn, and defend, the Soviet Republic.

Dr. Schulkind rightly protests against more recent simplifications, especially of the Marxist interpretations. He concludes that if the second view from the Left reveals as much about the Left from 1871 to 1921 as about the Commune, its concepts and variety are nonetheless important in showing the spirit in which the contemporary Left might reinterpret the Commune. His excellent selection of documents allows the reader to judge for himself both this, and the importance of the reconsidered Communard Left for the current Left. Equally, it illuminates the too long neglected historical complexity of both of these views of the Commune from the Left.

JOHN HORNE

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De Republica Anglorum: A Discourse on the Commonwealth of England. By Thomas Smith. Edited by L. Alston. (New York: Barnes and Noble Books, 1974. Pp. 210. $15.00.)

The present volume is a photolithographic facsimile of the 1906 edition of Sir Thomas Smith's minor classic, first published in 1583, some six years after the author's death. Smith himself tells us that the book was originally composed in 1565, at which time he was serving as Elizabeth's ambassador to France. The text appears to have been revised in the years that followed, but since the manuscript of the first draft has not come down to us, it is impossible to determine the extent of the revisions. Numerous early editions of the work attest to its immediate popularity. Excerpts from it are quoted in the second edition of Holinshed's *Chronicle* (1587). The book opens with a series of considerations on the nature, origin, and division of political regimes, as well as on the different classes of English society. This is followed by a general discussion of the role of the king and Parliament. The remaining two-thirds of the book are taken up with a detailed account of the English judicial system.

The treatise as a whole may be described as a study in comparative jurisprudence. Its sole purpose, we are informed (p. 142), is to show how English common law differs from the Roman or civil laws in use on the continent. The author exhibits none of the philosophical gifts of either Fortescue or Hooker. One does not find in his essay any of the customary allusions to natural law or any attempt to engage in a theoretical discussion of the principles of just rule. The thesis laid down at the outset is that each regime is to be judged on the basis of its agreement with the nature and particular needs of the people governed by it (p. 28). Although no universal claim is made for the superiority of the English laws, there does not seem to be much doubt in the author's mind that the English regime is "the righter, truer, and more commodious way to govern the people as well in war as in peace" (p. 143). One suspects that the description of the English commonwealth is at times deliberately flattering. Smith speaks as though torture were unheard of in England (pp. 104–6); yet we know from one of his letters that as a member of the Privy Council, he himself was once persuaded to resort to it. The remark that serfs or bondmen had by then become so few in England that "it is not almost worth the speaking" (p. 131) may likewise be taken as a euphemism.

The book is justly famous for its chapters on the authority of Parliament (II, 1–2, pp. 48–58), in which Sir Frederick Pollock and other late-nineteenth-century scholars saw an anticipation of the Hobbesian doctrine of absolute power. It is by no means certain, however, that Smith was acquainted with the new theories of sovereignty which had begun to spring up in France and England, or, if he was, that he shared them in any way. His remarks on this subject remain by and large traditional and amount to little more than the assertion that "the prince in parliament" (p. 63) constitutes the highest authority in the land. Civil society is said to be natural and is defined as "a multitude of free men collected together and united by a common accord and covenants among themselves, for the conservation of themselves as well in peace as in war" (p. 20). As for the English regime, it is neither simply regal nor simply political but a combination of the two, inasmuch as the legislative power is shared jointly by the king and the people, represented by Parliament. The possibility of a rift between the Crown and Parliament, of the kind that developed in the seventeenth century, is never envisaged. The right of rebellion is acknowledged in principle, but the author cautions that its exercise is "always a doubtful and hazardous matter" (p. 13).

The single most distinctive feature of the book appears to be its general disregard of the biblical tradition. Unlike all of his predecessors and most of his successors, Smith never quotes Scripture and makes no effort to provide a theological basis or justification for his views. He subscribes wholeheartedly to the Elizabethan Church system and is obviously eager to bring the political and religious powers under a single head. Accordingly, the ecclesiastical authority is not treated as a separate entity but rather as an integral part of the political establishment. Apart from its strong anti-papalism, the book betrays no awareness of the incipient conflict between the established Church and nonconformist religious groups. Christianity is credited with having instilled a measure of gentleness in the innately bellicose English temperament, but the reader is left with the impression that its demands are to be subordinated to the necessities of the political life. Smith's own political ideal would seem to be a thoughtful blend of humanity and policy, which he regards as indispensable both to the strength of the state and the well-being of its citizens.

One hopes that the reprinting of Smith's long-neglected work will stimulate new interest in it and incite scholars to undertake a more diligent study of its contents.

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Classical Economics Reconsidered. By Thomas Sowell. (Princeton, New Jersey: Princeton University Press, 1974. Pp. 152. $9.00.)

This is the fifth book on classical economics in the last three years: the first by Maurice Dobb, *Theories of Value and Distribution Since Adam Smith* (1973), in fact ranges beyond classical economics, and in any case is too tendentious to constitute more than a point of departure for an interpretative debate; the second by Marian Bowley, *Studies in the History of Economic Theory Before 1870* (1973), is stronger on the 100 years before Adam Smith than on the 100 years after, and indeed does not reach the classical school until the half-way mark in the book; the third by Robert Eagly, *The Structure of Classical Economic Theory* (1974), is a highly idiosyncratic work that fails to live up to its own announced intentions; and the fourth by Denis O'Brien, *The Classical Economists* (1975), is so good as to supersede almost everything that has gone before it: to my mind it is the most satisfactory and certainly the most comprehensive treatment of classical political economy available to date, perfect for students of economics and even for students of political science. The book under review, on the other hand, is clearly addressed to economists; it would be incomprehensible to a political scientist and it contains very little about the political philosophy of the classical economists, and even less about their views on concrete questions of economic policy.

For the student of modern economics, looking for a brief, rapid survey of the subject, however, Sowell's book is a godsend. Rather like a modern textbook of economics, his treatment of classical economics is divided into four sections: social philosophy, macroeconomics, microeconomics, and methodology. There are few quotations in the text but his footnotes are peppered with references to the writings of major and minor classical economists. Standard secondary sources are almost wholly ignored, and there are no suggestions for further readings. In short, this is a book for economics students who want to study classical economics, not for its own sake, but for what it may say about the concerns of the present.

Understood in this sense, it is on the whole an excellent treatment. To be sure, the chapters on macro and microeconomics stand up better than those on social philosophy and methodology, and, unfortunately, the chapter on macroeconomics is almost wholly a prècis of the author's earlier study on *Say's Law* (1972), which robs it of the novelty it would otherwise have had. There are a few slips, such as the statement that "Smith . . . advocated publicly subsidized schooling – not to be general in England for another century" (p. 21), when in fact Adam Smith advocated a measure of public subsidy, insisting however that fees must be charged and that teachers' salaries must not be wholly paid out of tax revenues; or the proposition that "Marxian value . . . is entirely a *measure* of value rather than a *theory* of value" (p. 102), a misinterpretation too complex to unravel here. In a different class is Professor Sowell's repeated assertion that the famous classical controversy on "general gluts" had absolutely nothing to do with the fear of permanent secular stagnation, being instead a controversy about the question of whether increased saving–and–investment would always cure depressions and promote economic growth (pp. 46, 48, 72). He bases his interpretation on a single quotation of Malthus, denying that there was any disagreement between him and Ricardo about the impossibility of such a thing as a permanent glut. But as Professor Sowell himself points out: "While the classical economists were never at their best in interpreting other viewpoints, their understanding of economic phenomena was much more sophisticated than their interpreters have allowed. Despite a penchant for dogmatic assertions, the classical economists had the saving grace of inconsistency which allowed them to recognize exceptions and modifications, and even to quietly incorporate some of the views of heretics, once orthodoxy had been vindicated" (p. 73). Applying the same argument to Malthus, one of the heretics, it will take more than a single quotation to persuade me that John Stuart Mill was wrong consistently to depict Malthus and his allies as believing in secular stagnation.

But these are *caveats* for experts. For the young economist getting his first taste of the "old writers," this is as good a book as any to start with. Students of political science, however, are better advised to start with O'Brien, particularly the last chapter on "The Policy Prescriptions of Classical Economics."

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The Argument and the Action of Plato's 'Laws.' By Leo Strauss. (Chicago: The University of Chicago Press, 1975. Pp. 186. $10.75.)

This commentary on Plato's *Laws* is the last book-length work completed by Professor Strauss before his death. Unlike nearly all of his other books, this one begins abruptly; Professor Strauss's introduction does not remind us of his long-standing concern with the continuing crisis of Western liberal democracy. Rather than restate his arguments pointing to the contemporary need for study of classical political philosophy, Strauss begins with an epigraph from the Medieval philosopher Avicenna. This quotation places the *Laws* in the context of the old quarrel between reason and revelation, or between philosophy and the religious or divine law. In his introduction Professor Strauss speaks also of a closely related old quarrel, if not another aspect of the same quarrel, by recalling that the philosopher Socrates was the victim of an Athenian law against impiety. And he suggests that the *Laws* examines the question of whether society can be transformed, in a manner convincing to law-bred and experienced political men (cf. p. 38), so as to make a more secure home for philosophy. As opposed to the "utopian" *Republic,* in other words, the *Laws* explores the practical possibility of establishing and preserving a just or rational society, one whose principles can withstand exposure to the light of free and honest inquiry. Professor Strauss still begins, then, from the question of what need, or even what room, there is in political life for the pursuit of philosophy. But he considers this question in its original, rather than its merely local and contemporary, form.

Strauss's book reminds us that human life is indeed quite possible without philosophy and science. And clearly, he does not assume philosophy to be an unqualified blessing. In speaking of cataclysms (floods, etc.), for example, Strauss notes that "oblivion of the initial (and final) terror is necessary for political felicity, for one can not act on a grand scale without hope" (p. 41). Yet he adds that the weakening of hope (in gods), which prepared the emergence of philosophy, is for that reason "not altogether a change for the worse" (p. 172). That this is so may be in part because Plato's spokesman, an Athenian stranger, outlines a natural theology which can supply a sounder basis than the traditional ones do for reasonable hope. But being a philosopher, the Athenian does not paper over the difficulties which surround his own theology, and thus the entire legislation it is meant to support (cf. p. 142).

Strauss's interpretation of the *Laws* makes hardly a reference to the important modern attempts to frame a just or rational society without religious foundations and to outline a political morality which can dispense with belief in divine sanctions. But this possibility is considered throughout the book, especially in the discussions of the problematic relation of law and reason (pp. 17–19, 57–61, 92, 130–133, 137 et al.). Apparently Strauss thought religion to be useful and even indispensable for the establishment and maintenance of sound politics; but that opinion does not begin to exhaust his concern with the religious question. "Being a philosopher, the Athenian [stranger] is concerned with the truth about the gods" (p. 141). Yet the beginning point for the Athenian's theological questioning is not so much his philosophy as it is his concern, as a normal human being, about the happiness and misery of honest and good men (cf. p. 8, 13, 28–30, 59–60, 117, 160, 167–168; compare *The City and Man,* pp. 240–241). As for Strauss's own interest in theology and Providence, it is combined with exceptional freedom from vestiges of the secular faith in progress.

One serious difficulty with Strauss's work is its external format as a commentary; at first it might seem like a mere retelling of the *Laws.* Yet a closer look shows this not to be the case. For there are many remarks which help to explain transitions from one theme in the *Laws* to the next. And further, even those passages which seem like mere translation are full of lesser and greater variations from Plato's text. Strauss leaves it to the reader – who he assumes will study his book together with the *Laws* – to observe these differences and to figure out their significance. His commentary is therefore unusual in the degree of active participation which it solicits from the reader. Let me attempt to illustrate this by focusing on one relatively conspicuous example of Strauss's way of writing.

In Book I, the Athenian stranger mitigates the harshness of his criticism of Spartan and Cretan (Dorian) laws by appealing to one of the finest, "as everyone must admit," (p. 10; compare *Laws* 634d5–7) among those laws. This Dorian law forbids any of the young to criticize any of their institutions; instead it stipulates that they should say with one voice that all their laws are fine since they were given by gods. Yet one of their old men may dissent and criticize when speaking to a ruler and men of his own age, provided no one young is present. It is not clear how the Athenian knows of the existence of this law. Strauss in his commentary entitles this two-fold law "the Dorian law of laws" (p. 11), a name of his own invention and one by which he refers to it repeatedly. At least once he even goes so far as to call this "law of laws" the *"highest"* of the Dorian laws (p. 16). Why does he allow himself such liberties in referring to this law?

To resolve this question it might help to note that in the same context Strauss points to Plato's clear indication (634d–e) that the Cretan Kleinias had been only pretending – in order to forestall hasty criticism – to believe in the divine origin of his city's laws. The premise of the divine origin of the Dorian laws, "which allegedly makes certain the goodness of those laws," is a fiction. "After this has been understood, he [the Athenian] can safely continue to speak from time to time in the old manner" (p. 11). Strauss's rule of reading Plato is that a word to the wise is sufficient; we are to take with a grain of salt any subsequent references to the Dorian laws as being divine.

With this rule of reading in mind, I now turn to Strauss's discussion of an early definition of law (*Laws* 644c–d). He translates as follows: "When that reasoning [as to what is better or worse with regard to pleasure and pain] has become the common decrees of the city, it is called the law" (pp. 17–18). (Note that a literal translation of the Platonic passage – 644d3 – would have read "decree" rather than "decrees.") In interpreting this passage, Strauss observes the difficulty that if the law is nothing but reasoning, once accepted by the city, then "the reasonable man will obey it but he does not need it for his own guidance. On the other hand, if we admit all 'laws' which are anywhere in force to be laws, can all of them be said to be reasonable?" (p. 18). In other words, unless one is prepared to deny that laws intend to make sense or to be reasonable – and this not even the old Dorians are willing to do (p. 7) – then not all decrees which are called "laws" or "the law" are rightly so named. The only laws in the true sense would be those which achieve, or at least aim toward, the reasonableness which is their spirit (cf. Plato's *Minos* 315a). The term "law" would thus have to be used in two senses, only one of which is seriously meant. Could this be the reason for Strauss's coining of the phrase "law of laws"? For this one law does more than attempt to consecrate the Dorian legal system in the eyes of their young. Isn't the one most reasonable Dorian law, the law in the truest sense, that which – by attributing divine origin to the whole code – implies that each "law" is good or reasonable and thus of necessity allows at least to old men the privilege of questioning every "law" which seems to be of deficient reasonableness? If something like this is Strauss's suggestion, his deviation from the letter of Plato's language would be a device to help the reader see more clearly Plato's own questions about law.

This tentative analysis of a single phrase is given in the hope of indicating the attention to detail which Strauss's important book demands.

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History and Will: Philosophical Perspectives of Mao Tse-tung's Thought. By Frederic Wakeman, Jr. (Berkeley: University of California Press, 1973. Pp. 392. $12.75.)

Assuredly, this is one of the most singular works of scholarship in living memory. Written by a distinguished social historian, it treats of ideas almost without reference to their social and political context. Concerned with a revolutionary leader who stresses as few have done the futility of "book learning" and the unity of theory and practice, it traces the origins of his thought and the wellsprings of his behavior in terms of the intellectual influences to which he has been exposed. Intended primarily for the Western public, it devotes as much space to a capsule history of European philosophy as to analyzing significant and characteristic aspects of the Chinese heritage. Because of the importance of the subject and the talent of the author, it is very much worth reading, but one is left in the end with the impression of a flawed masterpiece: fascinating, provocative, and yet somehow unsatisfying.

It is difficult enough to say even what "European civilization" means to a European or an American today. To grasp what various currents in Chinese and Western thought mean to a Chinese, and how they have interacted to shape his ideas, poses problems of a wholly different order of magnitude. The methodology which Mr. Wakeman has adopted in his attempt to come to grips with such a many-faceted subject is highly original. He describes his book as built up from the juxtaposition of "a series of essays which are thematically cohesive but not discursively sequential," a "group of reflections as in a hall of mirrors," which the reader is asked to hold in his mind until they are finally united in the "single focus of the Cultural Revolution" (p. xiii).

Given the many different layers of meaning and experience which intersect and overlap in the Chinese revolution, such an unorthodox approach has much to be said for it. Because the logical links between one phenomenon and another are often not spelled out, however, it becomes doubly important that the mirrors or signposts set up to guide the reader should not lead him astray. Unfortunately, one of the most fundamental signposts of all is here somewhat awry. During the early 1950s, says the author, Mao followed the Soviet example in stressing the importance of organization, but by the 1960s, he had adopted a view in which "struggle not only ideally determined organization in the People's Republic – it virtually replaced it *as a political form*" (pp. 27–28).

If Professor Wakeman comes to this conclusion, and constantly reiterates throughout the book that Mao's ultimate political vision is a "mass line realized by society as such," not "depending on organization at all" (p. 306), it is obviously because he reads the evidence in the light of the Cultural Revolution. To be sure, Mao's attitude toward organization is not identical with that of Lenin, or of Liu Shao-ch'i, and the events of 1966–69 illustrated some of the differences. They illustrated them, however, in exaggerated and distorted form, and even during the high tide of the Cultural Revolution, Mao declared, in a speech of February 1967 cited by Mr. Wakeman (p. 314, n.38) that anarchism was "most reactionary," and there would "always have to be heads."

Thus, in seeking an explanation for Mao's repudiation of organization and leadership, Mr. Wakeman has to some extent been searching for clues to a nonexistent mystery. It remains true, however, that Mao's emphasis on struggle, his conception of dialectics, and above all his glorification of the human will as the maker of history, distinguish his outlook from that of most Marxists.

Mr. Wakeman points the mirrors in which he hopes to catch the origins of these traits squarely at the pre-Marxist period in Mao's development. The young Mao, he says, "began with indignation and only later acquired a revolutionary ideology." Moreover, the "images of historical change" which he "found and tested in the turmoil of civil war" ultimately "overtook ideology" (pp. 92–93). This goes beyond the view, which has been put forward by many students of the Chinese revolution (and confirmed by the Chairman himself), that Mao's commitment to revolution antedates his assimilation of Marxist thought, to suggest that Mao is not a systematic political thinker at all, but a poet or enthusiast whose contribution lies in inspiration rather than in analysis. Mr. Wakeman explains this by the fact that Mao is Chinese. "One of the primary assertions of *History and Will,*" he says in the preface, ". . . is that Mao Tse-tung's dialectic was not truly Marxian because Chinese metaphysical constructions did not possess the universal ontological categories of European rationalism" (p. xiii).

Though many thinkers are discussed in the course of the book, this judgment is based primarily on Mr. Wakeman's analysis of a few authors, whom he sees as having given definitive expression to key ideas characteristic of their respective cultures: Kant, Hegel, and T. H. Green on the one hand; Chu Hsi, Wang Yang-ming and the late nineteenth-century reformers on the other. The Western writers he has singled out are those to whom Mao was most intensively exposed during his student days in Changsha sixty years ago. (Indeed, Mao himself has acknowledged that he then "believed in Kant's dualism, especially his idealism.") The choice of the early sixteenth-century philosopher Wang Yang-ming is somewhat more debatable. Wang's name is not even mentioned in the two or three million words of Mao's writings currently available, though these abound in references to a wide range of philosophers and literary figures. Mr. Wakeman, who holds that Mao "cannot but have been influenced" by Wang, at least indirectly (p. 238), recognizes that Wang Yang-ming's "doctrine of the unity of knowing and doing, which was designed to illuminate the existential path of sagehood rather than resolve ideological difficulties, was quite different from the Marxian theory of practice" (p. 271). He argues that Mao has nevertheless built a "bridge between them" (p. 273). Taking Wang Yang-ming's key concept of *liang-chih* (which can signify either "innate knowledge" or "knowledge of the good") as the symbolic expression of a Chinese tendency to identify the true and the good, he concludes: "If the forte of Kantian logic was its brilliant acuity and fine sensitivity to categorical distinctions . . . then the force of Neo-Confucian thought was its adumbrations of meaning, its suggestive shadows" (p. 255).

Mr. Wakeman also finds, in Wang Yang-ming's vision of "a practice which encompassed the entire cosmos and urged men to change things in order to know them" the impulse behind Mao's search for a way of relating "personal will to historical universals" (p. 273). In reconstructing the bridge which Mao has built at this point between Chinese and Western thought, Mr. Wakeman endeavors to secure the opposite end of it to those Western philosophers who emphasized most strongly the role of the will. Drawing together the various intellectual influences to which the young Mao was exposed, the author proposes the following "intellectual calculus":

> From Wang Yang-ming there certainly came a conviction that thought was to be expressed in action. From . . . the Neo-Kantians came the assurance that reason created social forms, liberating the self from customs. From K'ang Yu-wei, Yen Fu and the Darwinists came the notion of objective and universal laws of science which proved evolutionary change. From [the seventeenth-century thinker] Wang Fu-chih came the intuition that change operated within matter, through its own internal relations. And from T. H. Green came not only the intense glorification of will, not just the civil society of a Rousseau, but the depiction of political society as an instrument of individual realization (p. 293).

It is here suggested that Green's contribution provided the keystone of the arch of Mao's philosophical outlook, as it took shape during the May Fourth period, and has continued to determine his world view and the use he has made of Marxism. The parallel between Green's ideas and "Maoism" is perhaps not so "absolutely striking" as the author argues (p. 291). For Green, the value of "the institutions of civil life" lay "in their operation as giving reality to [the] total capacities of will and reason, enabling them to be really exercised" (p. 291). For Mao, on the other hand, the manifestation of the human will is primarily a means, the end being the revolutionary transformation of "the institutions of civil life."

And yet, such is Mao's obvious exhilaration in the exercise of his own will, and in calling forth the will of the masses, that we must recognize something of Green in him after all. In two passages, which Mr. Wakeman has rightly saved for his conclusion, Mao Tse-tung sums up his own ideas about the relation between history and will. In 1962, he said: "Freedom means the recognition of necessity and it means transforming the objective world. Only on the basis of recognizing necessity can man enjoy freedom of activity; this is the dialectical law of freedom and necessity. What we call necessity is an objectively existing law.

Before we recognize it our behavior cannot be conscious; it has elements of blindness" (p. 322).

In 1964, he placed the emphasis on man's role as the maker of his own history: "Engels spoke of moving from the realm of necessity to the realm of freedom, and said that freedom is the understanding of necessity. This sentence is not complete, it only says one-half and leaves the rest unsaid. Does merely understanding it make you free? Freedom is the understanding of necessity *and* the transformation of necessity – one has some work to do too" (p. 325). [Mr. Wakeman had available at the time of writing only incomplete and partially garbled texts of these two speeches; I have substituted translations based on better Chinese versions, which do not change the sense, but make the meaning clearer.]

"To stay even with history," comments Mr. Wakeman, "to will *against* it, is to accept it while understanding the need to face it. By finding an opposite force to confront, Mao knew himself as victorious revolutionary against overwhelming odds and as sage chairman conscious of historical development" (p. 327). Like other observers of the Cultural Revolution, he suggests that Mao sees the continued successful advance of the revolution as something of a miracle, achieved against the grain of history, and against the natural tendency of human society to backslide into revisionism.

To juxtapose the acceptance of historical laws, and the will to shape history, does not, however, in itself provide a solution to the philosophical problems involved. It might be argued that the pattern and the direction of change are, in Mao's view, defined by the principles of dialectics. But his dialectics, too, are more a mixture than a genuine fusion of Chinese and Western ideas. As Mr. Wakeman puts it, "Lacking the balance of *yin* and *yang*, [Mao's] 'unity of opposites' was not the same as the polarity of the *Book of Changes.* Lacking Hegelian categories like *Aufhebung* and stressing the inward complementarity of opposites, Mao's 'unity of opposites' was not identical with Lenin's dialectic" (p. 297). Having rejected the concept of the negation of the negation, and demoted the transformation of quality and quantity into one another to the status of a corollary, leaving the unity of opposites as the only "basic law," Mao was left with a theory in which the *structure* of the dialectic did not, in itself, point in any direction at all. In the last analysis his understanding of the historical process can only be deciphered in the light of his action. Mr. Wakeman's mirrors throw many fascinating sidelights on reality, but they are no substitute for reality itself. Let us hope that in his next contribution to the subject, the historian will reassert himself against the philosopher, and give us a work which not only talks about the unity of theory and practice, but illustrates it.

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Evolution and Revolution: The Rising Waves of Emancipation. By W. F. Wertheim. (Baltimore, Md.: Penguin Books, 1974. Pp. 416. $2.75, paper.)

This book takes issue with those scholars who accept stability as the normal state of society and rejects those developmental models which posit stages of advancement such as "traditional," "transitional," and "modern" as too simplistic as a general framework for the analysis of the evolutionary development of human societies. The author, who is professor of sociology and history at the University of Amsterdam, argues that increasing complexity, which is usually the primary organizing concept of such schemes, does not necessarily lead to technological progress or social advancement. In fact, following Geertz, he suggests that highly complex, adaptive social structures can inhibit evolutionary development in those circumstances where "involution" or overspecialization has occurred.

Instead, Wertheim argues that evolution can be interpreted as a general trend toward "emancipation" by which he means increasing freedom from the forces of nature and human domination through the spread of knowledge and expertise. The impetus for this trend is the "social divergencies" that occur because of differences among people in their perception and experience of societal structures and processes which result in "counterpoints" or "contrapuntal" value systems in all societies. Although social cohesion is maintained primarily through the acceptance of a set of values which dominate, contrapuntal values remain dormant, but virulent. Society, in the author's view, is not a harmonious, static structure but, rather, an uneasy coexistence among competing value systems in which "the persistent, mostly silent, struggle for the recognition of values neglected under the dominant social order . . . becomes one of the principal agents of fundamental social change" (p. 118).

In this context, the author conceives revolution as a specific form of the evolutionary process and distinguishes it by arguing that it is a rapid, "cataclysmic" change which greatly accelerates the society toward emancipation. Revolution is separated from other forms of social upheavals such as coups, rebellions, revolts, and the like by restricting the term to only those which aim at fundamental change in the existing social order. Wertheim suggests that revolution is most likely to occur when overspecialization has taken place because backwardness and stagnation result in dissatisfaction among significant sectors of the population. In such circumstances, "only a forceful and cataclysmic breakthrough can produce a reversal of the involutionary trends" (p. 75).

Wertheim does not believe that purely economic conditions account for the phenomenon of revolution. He suggests that the essential element is change among various sectors of society in relation to one another and with respect to the values which people hold. Such social change will produce revolution, however, only when a "mental rejection" of the status hierarchy among social groups has occurred and certain "psychic forces" have been released among large numbers of a society's members. This gives rise to the mobilization of power around an alternative set of values against the dominant hierarchy.

Although I generally concur with Wertheim's model of society and conceptualization of revolution as a type of social change, I do not believe he has adequately distinguished revolution from evolution. In spite of his rejection of violence as the distinguishing attribute, his choice of time as an alternative is unsatisfactory. It is not impossible for revolutions to drag on for interminable periods and for evolutionary change to be extremely rapid and reach cataclysmic proportions. Even though Wertheim recognizes that the dynamics of social change rather than change in a particular sector of society needs to be examined as a determinant of revolution, he does not specify which sectors must change and how much to produce the preconditions of this phenomenon. In other words, he does not provide a clear indication of the preconditions of revolution from those of its opposite, evolutionary change. To say that the building of centers of power against the dominant hierarchy is an aspect of revolution, as opposed to evolution, is to imply that the use of power to accomplish social change is not a normal occurrence in social life. Finally, nowhere in the author's discussion of psychological factors does he specify the precise linkage between societal and individual levels of analysis, nor does he spell out how the psychological tensions so vital to the outbreak of revolution differ from those which are generated during periods of normalcy and fuel the cooperative movements which are responsible for evolutionary change.

In spite of these problems, this book deserves the attention of students of revolution because it attempts to integrate a vast output of theoretical work from the conflict and consensus schools of social integration and seeks to understand the phenomenon of revolution in the context of general social development.

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Freedom and Reactance. By Robert A. Wicklund. (Potomac, Md.: Lawrence Erlbaum Associates, 1974. Pp. 205. $10.95.)

Since J. W. Brehm published *A Theory of Psychological Reactance* in 1966, some experimental psychologists – mostly Brehm's students – have been studying reactance. In *Freedom and Reactance,* one of them summarizes and assesses that work and suggests directions for future thought and research. For Wicklund, reactance is "the motivational state of persons whose freedom has been assaulted" (p. 8). Reactance theorists observe that persons value their freedom to choose and that they will react to protect that freedom when it is attacked. This assault can be an external "threat" that tries to influence the subject's choice, like a censor; it can be a "barrier" that makes the desired option more difficult to obtain than other, similar options; or it can be a "self-imposed limitation" where a person, simply by feeling a preference or making a decision, limits his options.

While reactance theory and experiments are quite sophisticated, the basic form of the experiment is rather simple. Subjects are made aware that they have the freedom to choose from among a range of options. This freedom is then "assaulted" by the experimenter, who manipulates the options by, for instances, the "threat" of censorship, physical removal, or authoritative pressure, or the "barrier" of a higher price or longer wait for the desired option. The subjects' responses – usually reactance and reassertion of freedom – are then ascertained. A specific experiment, thus, might involve giving subjects freedom to choose on a dichotomous political issue and then "assaulting" that freedom by a blatant attempt to persuade them to choose one side rather than the other; reactance theory suggests, and the experiments bear out (p. 33), that the attempted persuasion tends not to convince the subjects but to drive them to the opposite opinion, as they assert their freedom from the assault of persuasion.

Wicklund is usually clear as he reports the experimental findings and discusses their theoretical implications. In addition, despite his commitment to reactance theory, he tries to be fair and complete in assessing its merits relative to other theories. So he occasionally criticizes reactance theory, as well as suggesting where it may have more explanatory power than other theories, like Festinger's cognitive dissonance theory or Dollard and Miller's frustration theory, to which political psychologists frequently turn. Wicklund's book, then, is a fairly complete and balanced report on a new and potentially important branch of experimental psychology.

For the student of politics, however, Wicklund's book has two central shortcomings. First, his theoretical discussions of major concepts, like freedom and determinism, are sometimes naive and confused. For instance, Wicklund seems unable to decide whether (1) to retain his original stipulated definition that freedom depends on the subjects' beliefs that they can control their behaviors (p. 2); (2) to conclude that individuals must be "determined," if science is to be possible (p. 181); or (3) to decide that individuals are only free when they are totally "free from all constraints, whether those constraints arise from within [e.g., internal direction, knowledge] or without" (p. 183). Further, unlike Kant and T. H. Green, for instance, but like too many contemporary psychologists, Wicklund seems unable to conceive of freedom in terms of a knowledgeable and internally directed individual who makes reasonable choices for himself – although Wicklund describes just such a "competent" individual as the only type able to withstand social pressure when making judgments (p. 48).

Second, Wicklund's well-meant attempts to draw the political and social implications of reactance theory are inadequate. He does not consider how the differences between laboratory and *nonlaboratory* settings may affect attempts to generalize experimental findings to politics. While he rightly insists that "assaults" on freedom do not always produce reactance (p. 39), Wicklund nowhere tries to specify nonlaboratory circumstances where reactance does or does not occur; nor does he try to specify nonlaboratory operational definitions for key variables. Furthermore, the experiments reported by Wicklund use students, not adults, as subjects. The experiments are never extended in time, and thus ignore any long-term effects. They are never cross-cultural. They consider attitudes exclusively, and ignore behavior. (Indeed, when they consider social pressure, they just deal with manifest attitude, and do not even ask if the underlying attitude has changed [pp. 82–84].) The experiments almost always deal with the subjects individually, and not in a social setting; thus, the findings say little about the individual's behavior in a group or about group behavior. Finally, as laboratory experiments they are performed in an artificial setting; and it is a valid question whether the assaults on freedom (e.g., in Nazi Germany) that concern students of politics are of the same level and susceptible to the same analysis as are laboratory "assaults" on freedom.

In short, *Freedom and Reactance* should be of interest to specialists in political psychology, for Wicklund does present a little-known but promising psychological theory. But these specialists should be careful of Wicklund's excursions into political theory and implications; and the nonspecialist should be very careful about any political application of reactance theory in its present stage of theoretical underdevelopment.

On the purely mechanical side, the book has an excellent bibliography. The subheadings in the Table of Contents are helpful. The proofreaders did their job well. But the index is very bad: some items should be entered but are not (freedom, threat, barrier); and many entries omit relevant page numbers.

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Sociobiology: The New Synthesis. By Edward O. Wilson. (Cambridge, Mass.: The Belknap Press of Harvard University Press, 1975. Pp. 697. $20.00.)

It is impossible to review Edward Wilson's *Sociobiology.* How can one do justice to a massive volume of 697 double column pages (of which the bibliography alone comprises 65)? Can a short review deal with a comprehensive study of social behavior in species ranging from slime molds to *Homo sapiens*? Does one focus on the varieties of social organization in different genera? Or on the political implications of principles as diverse as "r and K selection," "inclusive fitness," and "social commensualism"? In either case, most political scientists would doubtless be bewildered.

It is impossible, however, *not* to review Edward Wilson's *Sociobiology.* Since the publication of Konrad Lorenz's *On Aggression*, it is hard to think of a book on the biology of animal and human behavior which has received the attention given *Sociobiology*. And, unlike Lorenz, Wilson has written a scientific volume – not a popularized version of technical papers, but a careful and exhaustive survey of a generation of theory and research in behavioral biology.

Confronted with this contradiction, the honest reviewer can only suggest that in this case – even more than most – the review should *not* substitute for a reading of the book. Political scientists desiring to learn more about the field of animal social life, and its relevance to humans, will find *Sociobiology* a useful though demanding guide. Since Wilson explains fundamental terms, provides exhaustive documentation, and has copiously illustrated his work, it is a valuable textbook for scholars in other disciplines who may be unaware of the breadth and complexity of contemporary studies of animal social behavior.

Those already acquainted with the field have responded quite variously to Wilson's argument. For some, *Sociobiology* is indeed a "new synthesis" which will provide the basis of research on social life, in both humans and other species, for a generation to come. For others, Wilson's work has a strongly ideological component buried in the mass of example, description, and theoretical development.

When dealing with such an imposing tome, however, it is simply too early to make definitive judgments on this overall level. More useful, perhaps, would be a comment on why *Sociobiology* is worth the awesome price of twenty dollars, and a warning against excessively facile uses of Wilson's work by political scientists.

First, the comment. Wilson's book has the indisputable merit of showing that the existence of complex societies is a biological phenomenon. By emphasizing the relationships between animal behavior and population genetics, Wilson compels us to recognize the evolutionary significance of events which social scientists often treat without reference to Darwinian biology.

In particular, this approach should lead to a major theoretical shift in our understanding of *Homo sapiens*. Most discussions of "human nature," whether by psychologists, philosophers, or political analysts, attempt to define the traits of the individual – and to derive social structures from the interaction between the "nature" of the human individual and the environment, technological level, or historical antecedants of a particular society.

By showing that social structures can be assessed in terms of population genetics and adaptive strategies at the level of the population or species, Wilson's approach reflects the trend of recent biological research away from the individualism of classic American behaviorism (notably in psychology). Moreover, Wilson can do this without "reducing" the complexity of human social systems to presumed "instincts" (be they a supposedly innate aggressiveness, territoriality, altruism, or what have you). Whether individual behaviors are learned or innate, the resulting social patterns must insure the continued reproduction of the species or it will become extinct. In this sense, at least, *Sociobiology* provides quite convincing evidence that *Homo sapiens* is a social animal . . . and that theories ignoring this essential fact are simply obsolete.

There is, however, a large gap between this fundamental proposition and the day-to-day research of most political scientists. It is at this point that a question must be raised about the relation between *Sociobiology* and the discipline of political science. It is one thing to argue, in principle, that human societies can only be understood in the context of evolutionary biology. It is quite a different matter to determine the precise implications of ethology, sociobiology, or population genetics when describing and explaining human politics.

Hence the warning. Premature attempts to introduce Wilson's perspective in political science research will rightly be subjected to skepticism. I have already seen an attempt to explain Supreme Court decisions on civil rights in terms of the spread of an "altruistic gene" among the Justices on the court. Such reductionist explanations are not implied by Wilson himself, but could easily be adopted by scholars not fully aware of the complexities of social behavior among the primates (not to mention in our own species).

Wilson's argument that a genetic predisposition to altruistic behavior can spread through a population (at least under some highly specific conditions) has received considerable attention. While this position is spelled out in detail in Chapter 5 of *Sociobiology*, Wilson has popularized it further in an article in the *New York Times Magazine* (October 12, 1975). Whatever the technical status of "group selection," however, this approach is not directly relevant to political analysis for several reasons.

In the first place, discussions of social behavior from the perspective of population genetics often make the simplifying assumption that a trait is produced by a specific gene. Thus Wilson speaks of an "altruist gene" (e.g., p.

120). Social scientists unaware of the richness of the descriptive field studies cited by Wilson may not understand the hypothetical character of such formulations, and therefore ignore the extent to which apparently competitive behaviors may have cooperative or "altruistic" consequences (and vice versa).

As Wilson indicates, contemporary theories of population genetics stress the natural origins of "ambivalence as a way of life in social creatures" (p. 129). When situating human behavior in this context (Chapter 27), Wilson stresses the "multifactorial" character of evolutionary processes. Emphasis on theories of "kin" and "group selection" in *Sociobiology* should therefore not lead political scientists to see simplistic genetic or biological "causes" for specific forms of cultural behavior.

In short, Wilson provides a fascinating and provocative survey of contemporary research on social behavior in other species, reminding us of the necessity to relate the social and biological sciences. But *Sociobiology* is not a "solution" to the problems facing the political scientist; rather, it is a crucial prerequisite for the next generation of research.

Wilson's work marks the beginning of a return to a tradition as old as Aristotle, in which cultural and historical variations in human politics are illuminated by an understanding of the natural origins and constraints of social behavior. If nothing else, therefore, Wilson's work should be required reading for political theorists who have forgotten that man is truly a "political animal."

ROGER D. MASTERS

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Sozialisation und Personalisation: Beiträge zu Begriff und Theorie der Sozialisation. Third printing. Edited and with a new introduction by Gerhard Wurzbacher. (Stuttgart: Ferdinand Enke Verlag, 1974. Pp. xix, 280. DM 18.80, paper.)

At its height early in the twentieth century, German sociology specialized in the formation and analysis of concepts, the detection of broad social trends, and a process described by its proponents as empathic understanding and dismissed by its more empirically oriented critics as speculation. For all that the present volume incorporates the findings of empirical studies done in Germany and abroad, it is very much in the old tradition of German sociology. The book is valuable because it is written from a national perspective different from our own, one which has profited, as German politics and society did not, from Germany's fateful leap into modernity. Whatever the process be called – empathy, speculation, or insight – some of the contributors have said important things which, to this reviewer at least, seem to be true.

As Professor Wurzbacher points out in his introduction to the third printing, the German interest in socialization developed out of the sizable American literature available on the subject, as well as the German need to inquire into such matters as the authoritarian personality and the possibility of increasing equality of opportunity through education. The book has contributions from persons in a variety of disciplines and subdisciplines, including criminology, social work, education, psychology, and political science. The general theme is socialization; political socialization is at most an implicit theme in one or two chapters. Nonetheless the book is useful to the political scientist, both for its conceptual analysis and for the substantive content of three contributions.

Wurzbacher asserts in the first chapter that the concept "socialization," standing alone, suggests that the process of the individual's adjustment to the demands of society is too automatic. Both to prevent an oversimplified account of how human beings acquire values and norms, and for heuristic reasons, he introduces the concepts "enculturation" and "personalization," which, he says, are needed to supplement "socialization." "Enculturation" refers to the more or less conscious internalization of the values and norms of society, "personalization" to an ongoing process in which the individual chooses how to behave and consequently affects society. Though the question arises whether socialization and enculturation are not to some extent two sides of the same coin, one cannot deny that it is both scientifically useful and ethically salutary to have a conceptual scheme which foresees a role for the individual in his own development. Professor Theodor Wilhelm would make yet another distinction, between socialization and education *(Erziehung)*, the latter being intentional and concerned with the development of moral responsibility; other social influences on the individual would come under the heading of socialization.

Both Wilhelm, whose field is education, and Professor Joachim Hellmer, a criminologist, have a deep respect for the family, the school, and the church as agents of socialization (and education). Their chapters bear on both the stability of democracy and the prevention of juvenile crime. The subtlety and tone of Wilhelm's contribution can be conveyed by a quotation:

> . . . we can hope for real success in the effort to prepare young people for the new kinds of opportunities afforded by society at large only if at the same time the ancient social wisdom of Western morality stays alive and retains the capacity to be experienced. Insofar as this is possible only in primary groups, their vitality remains a conditio sine qua non of social education even in the "secondary" mass democracy (p. 153).

The most puzzling contribution from the standpoint of political science is that of Professor Franz Ronneberger, who is concerned with the relations between socialization and nation building. Abjuring the inclusion of intellectual history in his concept of nation building, he nonetheless incorporates, in Weberian fashion, some ideational content into the ideal-typical process as he defines it. The usual questions arise: how does he know, and what are his criteria for selection? Although his ideal type contains much that is suggestive, it overstates both the extent to which nations provide men's lives with meaning, and the importance of a belief in the general will. A belief of this kind may have been an important factor in the development of nations on the European Continent, but it has been much less evident in England and virtually nonexistent in America.

GLENN N. SCHRAM

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Fame and the Founding Fathers: Essays by Douglas Adair. Edited by Trevor Colburn. (New York: W. E. Norton & Company, Inc., 1974. Pp. 315. $14.95.)

The bicentennial is finally over and we are still beset by its celebrations, an assortment that ranges from proper scholarly monographs to sporty "historical minutes." A principal virtue of this collection of essays is the vantage point it offers for trying to make sense of all this. Douglas Adair devoted his last years to unravelling what fame meant to the founders and clarifying what he called their "mental landscape" (p. 299), themes that place the present celebrations in perspective by revealing how our memories of the founders comport with how they hoped to be remembered. More than this, Adair shows that today's events are themselves indicative of the founders' intentions insofar as they arise out of the founders' self-aware and concerted efforts to impress the future. As Adair puts it, the founders were dedicated to fame and dominated by a concern about "the tribunal of posterity" (p. 22) and, hence, an analysis of our current behavior points us back to the founders' attempts to move us. A proper appreciation of the intentions of the founders, in short, begins with an appreciation of the fame which we accord to them.

The development of this idea may be traced through Adair's quarter century of writing and provides the thread that ties together the essays included here. For example, his interest in reputation and its sources reveals the nature of his fascination for the historiography of the founding, i.e., the manner in which fashions changed among historians and the general public alike with respect to the reputations of various founders. In Adair's hands, historiography becomes more than an attempt to trace history as it is written by the victors or an arid recital of how historical schools or theories developed. It is, rather, a vehicle by which the historian, through consideration of how ideas are received, may arrive at their original versions. Thus Adair's first and most widely known essay, "The Authorship of the Disputed *Federalist Papers,*" demonstrates that post-Civil War admiration for Hamilton's political philosophy led to acceptance of Hamilton's claim to numbers of the *Federalist* actually written by Madison. To dispel confusion over who wrote what, in other words, Adair establishes that the confusion was a consequence of Hamilton's attempt to impress posterity. In this respect, Adair accomplished two things. First he eliminated the major obstacle to an authoritative division of the *Federalist* (Adair's division, I might add, was later validated by Mosteller and Wallace's computer studies). Second, he illustrated how an investigation of historical reputation leads to a reconsideration of both the reputable and disreputable.

Adair repeats this procedure to good effect in his work on the tenth *Federalist.* Here, his exploration of Madison's novel attempt, for the eighteenth century, to provide stability in a large republic ("That Politics May Be Reduced to a Science") builds upon an examination of the historical fortunes of the essay ("The Tenth Federalist Revisited"). Long before it became fashionable – the essay appeared in 1951 – he critically examined Beard's attempt "to strip the 'sanctity and mystery' " (p. 85) from the Constitution and found that it forces a re-examination of the "relation of economic factions with those . . . growing out of noneconomic roots" and of number ten's "obvious preoccupation with 'justice' " (p. 92). This ability to examine fashion yet avoid fashionability also appears in Adair's work on Jefferson. Published here for the first time, "The Jefferson Scandals" takes up the notorious story, made even more so recently in the guise of psychohistory, that Jefferson "fathered a mulatto family by his slave Sally Hemmings" (p. 180). The subject is made to order for Adair, "the fervent admirer of John Dickson Carr" (p. 162). Not only does he determine, I believe beyond the possibility of serious doubt, that Jefferson is innocent of the charge, but he also shows why Jefferson might have been reticent to respond to it, and suggests that if anyone at Monticello was sleeping with Sally Hemmings – and someone probably was – it was Jefferson's nephew Peter Carr (pp. 177–183). "The Jefferson Scandals" will not eliminate the popular legend, but its service in restoring, for those who pay attention, Jefferson's reputation "as an honorable man" is appreciated, all the more so for clarifying "the known character of the real Thomas Jefferson" (p. 183).

In effect, these essays are all prelude to the title essay of the book. Here Adair reconstructs the meaning of fame and discloses its importance for "Washington and his greatest contemporaries" (p. 8). His adjective, we understand immediately, is not misplaced. For the founders, fame is a companion to extraordinary people, "event-making" persons, those, in short, who leave "the mark of [their] deeds and ideals on the world" (p. 11). There is no place in the founders' universe for the notion of a future in which, Andy Warhol notwithstanding, everyone will be famous for a moment. And, as opposed to today's understanding of celebrity or newsworthiness – e.g., *Time*'s Man of the Year – eighteenth-century fame was "neither ethically blind nor morally neutral" but, in keeping with eighteenth-century republicanism, something to be measured by the standard of "egotism transmuted gloriously into public service" (p. 12). Thus reconstructed, fame – as noted earlier – becomes a clue to the founders' intentions. Taking his cue from Hamilton, who described "the love of fame [as] the ruling passion of the noblest minds," Adair argues that such love "transmuted the leaden desire for self-aggrandizement and personal reward into a golden concern for public service and the promotion of the commonwealth as the means to glory" (p. 24). With this, his timely theme is joined to one that is timeless. He makes it clear that we need not – any more than the founders – view men as angels in order to consider them primarily in terms of their ideas. His teaching makes it once again possible to consider the founding with respect to motives other than those produced by economic interest, political desire, or twisted psyche; not only possible, in fact, but popular if recent trends in studies of early American history are any guide (see Robert Shalhope's bibliographical introduction to this volume, "Adair and the Historiography of Republicanism"). For this gift, it is proper that Adair has now achieved some measure of fame in his own right.

LARRY PETERMAN

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The Atomic Energy Commission. By Corbin Allardice and Edward R. Trapnell. (New York: Praeger Publishers, 1974. Pp. 236. $10.00.)

With national energy policy at a critical point, with the conflict over the safety and environmental consequences of nuclear energy, and with the incorporation of the Atomic Energy Commission into the Energy Research and Development Administration and the Nuclear Regulatory Commission, there is a need for a new look at the AEC and the politics of nuclear energy in the United States. Unfortunately, Corbin Allardice and Edward R. Trapnell do not fulfill this need.

The authors – long members of the AEC system – provide a simple and readable history of the Commission, a good introduction to its origins, scientific achievements, bureaucratic structure, personalities, and some of its political battles. However, the book has important limitations for use by both the student and the professional political scientist. It is a general history and an insider's view more than a study of the politics of the AEC.

The authors intended to present the AEC in its political setting. They say their goal is to "paint a portrait of an agency and to convey an impression of the determination . . . of the people in AEC to push the development of nuclear energy within the framework of adversary proceedings" (p. xiii). Two chapters are devoted to the origins of the Commission and one to the matter of J. Robert Oppenheimer. Chapters on the AEC bureaucracy and on the Commission's relations with Congress are included. Yet Allardice and Trapnell fail to capture and analyze the political dynamics of the subjects they discuss and do not discuss in any useful way some important political issues.

For example, the authors devote an entire chapter to the relations of the Commission to Congress and mention the relations between AEC and Congress at various points in the book. They describe the functions of the Joint

Committee on Atomic Energy, the personal relations of the various AEC and JCAE chairmen, and the "paternalism of the Joint Committee" (p. 171). But Allardice and Trapnell never discuss the unique and powerful role played by the Committee (cf. Harold P. Green and Alan Rosenthal, *Government of the Atom* [New York: Atherton Press, 1963]). Hints and references to the dominant influence of the JCAE are found at various points in the book – for example, the writing of the Atomic Energy Act of 1954 (p. 44) or the rejection of the 1969 proposal to transfer ownership of uranium enrichment facilities to private industry (p. 76) – but the significance of the Committee in shaping AEC policy is never examined.

Similarly, the chapter on the AEC bureaucracy is more a guide to the statutory role of the AEC and its organizational structure than an analysis of the internal dynamics of the AEC. This chapter and the chapters on scientific and energy development never confront one of the fundamental political issues of the AEC: the role of the AEC as both promoter and regulator of atomic energy. The definition by the AEC of its mission as active promoter of atomic-electric power raises major questions which the authors do not confront: the process which led to the emphasis on promotion over regulation; the relationship of the AEC and the nuclear industry, especially the Atomic Industrial Forum, and the influence of that relationship on AEC policy; the problem of secrecy and the AEC decision not to release to the public important studies questioning the effectiveness of reactor safeguards; the battle of the AEC with the environmentalists; the Commission's internal conflicts over the effectiveness of safety and environmental controls and over research in this field.

Allardice and Trapnell also fail to examine the role of the AEC in national and international energy policy. On the former it is difficult to fault the authors: their study was written before the energy crisis, the Arab-Israeli war, and the perceived need in the United States for a national energy policy. The authors devote a chapter to international atomic energy in which they discuss peaceful aspects of international cooperation. But their discussion is limited to a brief history of the Atoms for Peace proposal and the International Atomic Energy Agency and includes no analysis of the problems and possibilities of international energy cooperation.

Thus, Allardice and Trapnell do not prepare the reader for understanding the current issues and conflicts surrounding the development of nuclear energy. The uncritical stance of the authors and their historical perspective prevent them from confronting the issues which led to the demise of the Atomic Energy Commission and the issues of nuclear energy policy which now face the United States and the world. The book is already dated, less because events have developed since its publication than because its analysis does not prepare us for these developments.

JOAN E. SPERO

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Student Politics in America: A Historical Analysis. By Philip G. Altbach. (New York: McGraw Hill Book Co., 1974. Pp. 249. $6.95, cloth; $3.95, paper.)

Nostalgia buffs will take delight in this "historical analysis" of the development of American student organizations in the period from 1900 to 1960. I found myself reliving student days as I read of the activities of the YP's, YPSL's, SDA, and a host of other campus groups of the late 1940s and 1950s. Beyond the evocation of pleasant memories, this volume provides a useful background account of how student politics reached the peak point of the late 1960s when the Gallup Poll reported that student activism was regarded as the number one problem in the nation (p. 228).

Professor Altbach's method is descriptive and he deliberately eschews the use of sociological and psychological variables to explain his subject. This is understandable given the myriad of groups, factions, and mergers that he discusses. Political scientists accustomed to relying upon quantifiable data to present and analyze their findings will find the book a bit tedious at points. It makes one realize how far our discipline has come from historiography.

At first blush, some of the book's major findings seem rather surprising. For example, Altbach observes that only a small minority of students have ever been involved as members of activist organizations, that those that have have been mainly in liberal-radical rather than conservative or religious organizations, that there is no broad sense of community among American students, and that students have had a minimal impact on broad social and political issues and on the academic and political life of universities. Upon reflection, I realized that except for the late 1960s, which Altbach regards as an aberration, he is quite correct. Altbach identifies three main issues that have served as the foci of activism; peace and war, civil liberties, and race relations. Campus issues, e.g., governance, curricula, and student conduct, have not attracted much attention. Rather, organized student "concern for campus based issues" has been restricted to "political questions in society" (p. 211).

Why has there been no strong tradition of student activism, and why have students failed to become a political force in the United States as compared with continental European democracies? Altbach suggests several explanations: the great size and complexity of enrollment in American higher education have prevented university students from claiming exclusive status as the future ruling elite; the structure of curricula and the pattern of governance in American universities preclude extensive student activism; anti-intellectualism in American life has resulted in widespread hostility to student activism; and student organizations have been unable or unwilling to develop sustaining linkages with supportive political organizations. These hypotheses, although quite plausible, require testing and further substantiation before they can be accepted.

What, then, of the student movement of the 1960s? Altbach establishes its continuity with the past, but he sees no prospect for the emergence of a permanent student movement as a major political force. Indeed, recent events tend increasingly to support this assessment. Yet the activism of the 1960s has unique significance because students became a force to be reckoned with, if only briefly. They were in part responsible for turning public opinion against the Vietnam war, they played a major role in driving a president from office, and they had a substantial effect on university life.

The impact of the student movement on universities is well known, but worth noting: the end of *in loco parentis;* a reexamination of the curricula, goals, and social role of the university; an opening of campus governance to participation by students and junior faculty; and, perhaps most significantly for the future, a demonstration of the fragility of universities as institutions and of their vulnerability to political pressures from within and without. Altbach does not speculate on the consequences of these developments, but he does predict that student activism of the 1960s mode will reappear. Unfortunately, he does not suggest what specific social, economic, and political conditions will evoke such a reemergence of activism.

This book should be read by anyone interested in campus life and governance because it puts student activism and apathy in perspective. It provides a useful background for more systematic empirical studies of recent and contemporary student politics. Scholars pursuing such inquiries will want to look at Altbach's findings in light of survey data on student and mass opinion and to examine student organizations using conceptual frameworks that explain their development in terms of organization theory, theories of social movements, and in the context of pluralistic politics.

NORMAN C. THOMAS

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The New Left in America: Reform to Revolution 1956–1970. By Edward J. Bacciocco, Jr. (Stanford, Calif.: Hoover Institution Press, 1974. Pp. 300. $8.95.)

The New Left has been a thorn in the side of American social science in more ways than one. For example, social scientists were not notably successful in forecasting the rise of this very important movement, even during its stage of germination between 1956 and 1964. Although Edward J. Bacciocco, Jr. does not enhance our understanding of *why* there was a New Left in America in the first place, he does provide us with the first scholarly history of the movement and a competent analysis of the movement's ideological development. (We may expect more such post-mortems on the New Left in the imminent future.) While sympathetic with the phenomenon under study, Dr. Bacciocco treats it dispassionately and evenhandedly.

The New Left further challenged American social science by rejecting the implicit pluralist bias in much of recent social science. Pluralists not only described the American system but also seemed to be casting a halo of legitimacy over it. The New Left managed – for a few years at least – to undermine the notion that the American system is functional, that the only appropriate way to produce social change is to "work within the system." But ironically, pluralism managed to survive this onslaught fairly well, if only because the character and evolution of the New Left itself confirmed a key premise of pluralism, namely, that elites are inevitable.

The pluralist position is rooted historically in the writings of the classical elitists – Mosca, Michels, and others – who insisted that elites (whether monolithic or plural) are certainly unavoidable and perhaps even beneficial. The New Left, with its call for participatory democracy, frontally assaulted the alleged imperative of social stratification. But although the ethos and rhetoric of participatory democracy permeated the New Left, the ethos never became a reality in the movement itself.

Bacciocco successfully shows the importance of the thought of C. Wright Mills and Herbert Marcuse to the New Left. Both thinkers wrestled with the problem which always plagues radical intellectuals, namely, where to find a lever to move the world? At first the New Left looked to the "poor." But white radicals – themselves generally of middle- to upper-middle class origins – could not relate to the white urban lower classes. The problem was not so much with the common people as it was with the radicals themselves, whose concern for these people was nullified by their romantic fixation on the *black* poor (a fixation which was too often grounded in sexual fantasies and pursuit of penance for imagined guilt). It proved psychically impossible to emphathize with both the black poor and the white ("racist") poor at the same time. To be attracted to the former was to be alienated by the latter.

The New Left proceeded to forget the proverbial "forgotten man" whereas, notes Bacciocco, George Wallace was able to tap the forgotten man's frustrations in a way which was anti-establishment and radical. In abandoning the white worker, the New Left was merely following the lead of Mills and Marcuse. By taking this stance, New Leftists had nowhere to look but to themselves – affluent students and intellectuals – as the source of radical change. Bacciocco is at his best in demonstrating how this shift from a concern with "others" to a preoccupation with "self" marked the transformation of the New Left from a movement of democratic reform to one of revolutionary elitism. The event which catalyzed this transition was the Free Speech Movement at Berkeley in 1964. The FSM was an affluent white youth movement, concerned not with the grievances of the masses but with the grievances of affluent white youth. At the root of those grievances was that ambiguous ailment, "alienation." In surrendering to its narcissistic impulses, the New Left simultaneously lost touch with the political "realities" surrounding it. Bacciocco's analysis of the FSM harmonizes with my own analysis of the FSM – an analysis based on what was then enthusiastic participation. (See Dale Vree, "Ten Years After the Free Speech Movement: Reflections of a Participant," *Worldview*, 17 [November 1974], 25–30.)

Before the New Left exhausted itself, it vindicated the "elitist" premise of the pluralists in two ways: (1) In its early form as a movement focused on the uplifting of the deprived, the New Left was unmistakably a privileged vanguard unable to transcend the limits of its class, communicate with the poor, or surmount the dichotomy between leaders and led. (2) In its later form as a movement preoccupied with its own psychic needs and ideological imperatives, it lost even a modicum of respect for the common man and adopted the position of monolithic elitism.

Bacciocco is to be credited with producing a book which shows us the relationship between the social history and ideological development of a movement which no student of political theory or American politics can overlook, and with reminding us yet again that who says organization says oligarchy.

DALE VREE

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Law, Morality and Vietnam: The Peace Militants and the Courts. By John F. and Rosemary S. Bannan. (Bloomington: Indiana University Press, 1974. Pp. 241. $10.00.)

"Between 1965 and 1969, the American peace movement tried to draw the nation's legal system into the struggle against the Vietnam war. Peace militants prompted arrest by deliberately breaking the law and used their trials as forums in which to challenge both the morality and the legality of the government's military policy." So begins this book, whose purpose is to describe and analyze the courtroom conflicts which ensued.

On the analytical side the book is not especially strong. It opens up issues of great importance, such as the domestic status of international law principles, the ambiguous role of juries in our system, and the function of illegality as a means of making law. But these topics are not pursued in a rigorous way – legally, sociologically, or philosophically.

On the other hand, the book does a fairly good job of describing the courtroom conflicts it concerns. For each of six cases – including the trials of Dr. Spock and the Fathers Berrigan – it provides background on the defendants and their offenses, the legal arguments and maneuvers of the parties, and the way in which the courts responded. There follows a rather full summary of the views of resisters and judges, expressed in writings and interviews, on the subject of civil disobedience. On the whole I found this material interesting, particularly the judicial interviews. If someone wished to investigate these matters in moderate depth, the book would constitute a useful source of data.

For me, the book served another function – to provide a sharpened insight into what nonradicals of the Left expect of courts. I am not now thinking of the resisters about whom the book was written. Their views might be thought somewhat idiosyncratic. Rather, I have in mind the authors themselves, apparently decent, committed people, who remind me of law students and nonlawyers I know. My guess is that their views are shared by a large number of people.

Repeatedly the authors assert that the unique function of courts in our system is to furnish justice. And what is justice? Well, the authors are somewhat less clear than Socrates about that. At times they seem to define it in rather traditional ways – enforcing the rules or following balanced procedures. At other times the authors suggest instead that it means saying no to government activities which they regard as immoral, such as the Vietnam War. I suspect the latter definition was uppermost in their minds.

The authors do recite the views of leading scholars, in particular Alexander Bickel and Herbert Wechsler, who take a more limited view of the function of courts. But substantial lines of argument on complex issues, such as the political question doctrine, are ultimately ironed out with simple assertions: Courts are there to check the President, they should not give in to political fears, etc. There is very little sense of the moral constraints on these officials, whose warrant is neither the periodic consent of the governed nor the need for effective power.

I find this view of judges as vindicators of substantive morality a significant historical phenomenon. Twenty years ago, I suspect, the authors would have been much more conscious of judicial inhibitions. If this is so, it would be very enlightening to learn how the change in our climate of opinion came about. I suppose the Warren Court had a great deal to do with it, but I wish someone would lay out the subject in detail. It would also be interesting to know how long this view of judges is apt to prevail on the nonradical Left. I would guess quite a while, especially now in the wake of the Burger Court's abortion and Nixon tapes decisions. In any event, that their perspective may have been historically conditioned the authors seem to be quite unaware.

MICHAEL E. SMITH

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Choosing the President. Edited by James David Barber. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1974. Pp. 209. $2.95.)

Once again we are indebted to the American Assembly at Columbia University for a conference which led to a brief but thoughtful book. Edited by James Barber, the essays include contributions by such noted scholars as Erwin Hargrove, Donald Matthews, Austin Ranney, John Kessel, Fred Greenstein, Murray Edelman, and Richard Boyd.

Insofar as political scientists allow themselves the luxury of punditry, these essays are mainly in the *mea culpa* tradition. The goals set for so long by so many seemed at hand and the results have been painful. The strong presidency sank in the Tet Offensive, the increased prerogatives of the Chief Executive led to the revulsions of Watergate and the open nominating process brought a candidate who forfeited 49 states to Richard M. Nixon. No wonder, then, that each of these essays adds a dollop of the special brand of gloom afforded by the unique expertise of the writer.

Most intriguing is the Ranney contribution "Changing the Rules of the Nominating Game" if only because Ranney is the sole essayist who can look back on an activist role. As a member of the McGovern Commission, Ranney was one of those who voiced the hope that somehow more women, blacks, and youth could be part of the presidential selection process. He reviews the unintended quota consequences of that hope and concludes that the "views and interests of women, youth, and ethnic minorities are not represented only, or even necessarily represented best, by delegates with the same biological characteristics" (p. 79). (This would appear to follow the findings of Jeane Kirkpatrick's extensive paper, "Representation in National Political Conventions: The Case of 1972," delivered at the 1974 APSA Annual Meeting [and available from the Coalition for a Democratic Majority, 1823 Jefferson Place, N.W., Washington, D.C. 20036]. The Kirkpatrick paper concluded that on most major issues the Republican delegates were closer in their views to the Democratic voters than were the views of any of the candidate support groups at the Democratic convention.)

The Democrats emerged from the Kansas City mini-convention with the worst of both worlds – public relations hoopla which insisted that quotas are gone, but just enough languages in the guidelines to generate challenges. Ranney stresses his preference for a convention which will recognize the primacy of party regulars, yet his essay is a brilliant and all-too-brief summary of the depressing complexities which may foreclose that possibility. Indeed, many Democrats trembled at the chaos that could have resulted from erstwhile McGovernites' refusal to face up to a brokered convention.

Hargrove's essay on "The Crisis of the Contemporary Presidency" is a melancholy resume of the personality characteristics of some recent presidents and concludes that we require an "active-positive" leader who is both "a democratic character and a political man" (p. 31). Regrettably, the characteristics of a president are often evident only in retrospect, so that we ponder a brilliant argument with more respect than hope.

The Matthews essay on "Presidential Nominations" is an excellent summary of the chaotic nominating process since 1936. Judged by the test of whether the nomination goes to the most popular person in the party, the system is judged quite successful. Matthews would have political scientists focus more attention on how candidates become front-runners, a topic which will become even more difficult to analyze because of recent campaign finance reforms and the proportional representation rule of the Democrats, two changes which will undoubtedly increase the number of candidates with clout.

Kessel and Edelman focus on campaign strategies, indicating the relatively narrow ideological boundaries of successful campaigns. Greenstein's essay updates his extensive research on popular attitudes toward the Presidency and concludes that while the office still has a powerful hold on the psyche of citizens, the very strength of that hold becomes a source of frustration. Put otherwise, the children of the 1950s who grew up believing in the omniscience of the president are most likely to be repelled by the post-1965 events.

And finally Boyd discusses "Electoral Trends in Postwar Politics" in a closing lament on voter disaffection, the weakening of party ties, and the special problems posed by ideological candidates. The data are presented fully, clearly and plaintively, forecasting a decade of popular skepticism of the honesty and integrity of public officials. In his piece, Ranney defines an optimist, 1970s style, as someone who believes the future is uncertain; this definition makes Ranney the Pollyanna of these collaborators.

SIDNEY WISE

Franklin and Marshall College

The Constitution and the Delegation of Congressional Power. By Sotirios A. Barber. (Chicago: University of Chicago Press, 1975. Pp. 153. $12.00.)

As a constitutional problem the delegation of legislative power has generally been considered a dead issue since the flurry of the 1930s when the Supreme Court seized on this ground to invalidate three New Deal statutes. But Watergate and attendant concern about congressional abdication to an imperial presidency now suggest that the Court and the country may have too readily rationalized congressional blank checks drawn in favor of the administration. For this reason, Professor Barber's reexamination of the delegation probably is quite timely.

The book can be as short as it is only because of the rather strict limit the author has imposed upon his inquiry. The delegation theme could be construed so broadly as to cover almost all separation-of-powers problems. Barber lists the difficult policy issues that he is not going to discuss: Can Congress represent a pluralistic constituency and still transcend the serious political divisions of that constituency? May not Congress sometimes serve the public more effectively through broad delegations than through clear policy choices? Is it always bad for Congress to evade responsibility for decision through the use of broad delegations? Should the courts attempt to force Congress into adequate levels of decision in all cases or only in those affecting values seen as constitutionally preferred?

Barber's purpose is a more narrowly legalistic one – to determine "the least that the Constitution could reasonably mean for the practice of congressional delegations" (p. 6). Every constitutional commentator except John Roche has conceded that the Constitution does impose some limit upon legislative delegation. That minimal limit Barber deduces from, or attributes to, the concept of constitutional supremacy – i.e., from the very existence of a Constitution designed in part to govern, limit, and restrict certain of the procedures of government. A completely flexible constitution is inconceivable. Unlimited delegation, permitting any new arrangement of offices and powers in spite of the fact that the old arrangement had been established as supreme law by an authority conceived to be superior to all other legal authority, would be abdication. The Constitution authorizes Congress to exercise powers, not to abdicate them.

Barber contends that constitutional supremacy provides the only sound foundation, and the only necessary foundation, for the rule of nondelegation. Other possible rationales – separation of powers, the common-law maxim of agency, due process, or the idea of republican government – may sometimes seem to support the rule, but each in turn brings in values and problems which at other times weaken the rule. Consequently the scope of the rule need not extend beyond the limits imposed by that concept; the ban on abdication is the sole constitutional requirement to avoid delegation.

Obviously it then becomes essential to define "abdication." Abdication, says Barber, occurs when Congress "deliberately transfers to others the responsibility for decision among what public debate shows to be the most salient policy alternatives presented to it" (p. 38). Congress does not abdicate when it arrives at a clear policy decision among salient alternatives and then delegates as an instrument of this decision. The question is whether Congress delegates *pursuant* to a policy choice or in order to *evade* the responsibility for choice. And Congress may delegate to any agency, public or private, administrative or judicial, so long as its delegations are pursuant to choice among salient alternatives. Admittedly this is a permissive rule, but Barber contends that it is "at least as positive as any other norm of the Constitution, expressed or implied" (p. 50).

Barber then proceeds to test the Supreme Court's principal delegation decisions by the standard he has developed. Briefly, the decision in *Brig Aurora* (1813) upholding delegation of fact-finding responsibility to the President was correct. The delegation in *Field* v. *Clark* (1892) was also properly held constitutional, but the Court's opinion was incorrect in purporting to forbid any delegation of discretionary authority. The delegation in *Wayman* v. *Southard* (1825) of authority to "fill up the details" of a statute was constitutional, but if the statute had actually gone as far as Marshall construed it to go, it would have been unconstitutional.

The modern doctrine is that delegation is constitutional if Congress provides legislative standards for its exercise. Barber appears to agree with the Court that the delegations in the *Panama Refining* (1934) and *Schechter* (1935) cases were unacceptable, but he rejects the contention in *Carter* (1936) that delegation to a private organization is invalid. Whether the Tonkin Gulf Resolution was invalid depends upon whether Congress was authorizing or merely approving and supporting presidential action.

In the final chapter Barber considers whether various instruments of congressional oversight, such as the legislative veto, the appropriations power, or the concurrent resolution, can substitute for the rule of congressional nondelegation, and concludes that they cannot.

This is a serious analysis which seeks to formulate a theoretically sound standard permitting broad but not uncontrolled delegation. The problem is in operationalizing the standard. One wonders whether the "salient alternatives" test will prove any more workable than, say, the "clear and present danger" test.

C. HERMAN PRITCHETT

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Dixiecrats and Democrats: Alabama Politics 1942–1950. By William D. Barnard. (University, Ala.: The University of Alabama Press, 1974. Pp. 200. $7.95.)

George Wallace has dominated Alabama for so long now that it is difficult to recall the time when he was not the state's principal political actor. This book, by historian William D. Barnard, is set in the pre-Wallace 1940s and 1950s. The key figure is James E. "Big Jim" Folsom, two-time former governor of Alabama. Folsom's meteoric rise, his personal and political struggles, and his temporary demise are treated sympathetically by the author, who feels that Folsom has been misunderstood and inadequately appreciated. In addition to the Folsom story, the author emphasizes the related fight within the Alabama Democratic Party between forces allied with and opposed to the national party, which was tardily developing a commitment to black political equality. In this fight Folsom was a strong loyalist whose racial views were considerably more advanced than his contemporaries'.

Barnard views Folsom as a latter-day Jacksonian who was done in by political conservatives anxious to put a stop to the social and economic changes wrought by the New Deal, some of whose principal proponents had been Alabama politicians proud to be identified in the public's mind as "liberals." Folsom's victory in a 1946 Democratic primary runoff was decisive even though his opponent attempted to portray "Big Jim" as dangerously soft on the issue of white supremacy. The author views the 1946 campaign as particularly significant because it foreshadowed the more successful exploitation of racial prejudice as a vote-getting technique in the post-Folsom era.

Folsom's ambitious hopes for supplanting a constitution drafted primarily for the purpose of suppressing black political participation and for greatly increasing state spending for education, welfare, and other social services through reform of a highly regressive tax system lay in ruins only two years after he took office. Conservatives had been unable to elect the governor of their choice but they wielded tremendous power in the Legislature. The Alabama governor has considerable resources at hand to deal with a recalcitrant legislature but Folsom was unable to use these resources to advantage. Barnard attributes Folsom's failures to a combination of erratic and naive political behavior and personal dissipation. Folsom could not work well with other liberal politicians in the state; neither could he transfer his own popular following to other candidates with similar views – a problem that even George Wallace has had from time to time. For all his failings, Folsom deserves credit for his unwavering commitment to racial tolerance and for his attempts to prod Alabamians into doing something to right the wrongs done to blacks. The author shows how deserved such credit is.

The other major concern of the book – the struggle within the Alabama democracy between forces loyal to the national party and those seeking to disassociate it from the national mainstream – is expressed mainly through statements of key state politicians – former Governor Frank Dixon, party chairman Gessner McCorvey, Senator Lister Hill, and Folsom. Barnard shows Folsom to have played a subordinate role in efforts to maintain Alabama's national party ties because of his unreliability as a "team" player. He believes the schism between Folsom and other loyalists, "combined with liberal defensiveness on the issue of race, eased the way for the conservative resurgence that had been in the offing since the death of F.D.R." (p. 103). Probably Barnard overemphasizes the need to control the State Democratic Executive Committee; Governor Wallace, after all, has been able for many years to pursue his independent brand of politics successfully without control of the committee.

The Barnard book is a very useful account of an intriguing period in Alabama history that has been insufficiently studied by scholars concerned with the state's political development. It abounds in details often repeated, but has been painstakingly researched using many sources hitherto untapped. The book is easily the best available account of the period when George Wallace was an unknown young state legislator, quietly learning how to build a powerful electoral coalition based both on liberal, New Dealish spending policies and conservative, Dixiecratic racial policies.

WILLIAM H. STEWART, JR.

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Prophets With Honor: Great Dissenters in the Supreme Court. By Alan Barth. (New York: Knopf, 1974. Pp. xi, 248. $7.95.)

Here is an example of that relative rarity, a popular book in constitutional law. It has been widely advertised; it was reviewed glowingly in the *New York Times Review* (July 28, 1974 [Sec. 7, p. 2]); and, its author has ornamented several TV talk shows.

Prophets With Honor is likely to be read by many people with appetites for political information, but no more background knowledge of constitutional law and politics than comes through hit-and-miss attention to one newspaper and the network news. In many ways this is a good book for such folk to read. Alan Barth has watched the Supreme Court and civil liberties conflicts as an editorial writer for the *Washington Post* since 1943. His work is highly literate, and he does not make technical mistakes in rendering comprehensible the output of the Justices. His 1952 book, *The Loyalty of Free Men*, coming during the worst of the McCarthy period, earned him a small but honorable place in the history of American liberalism.

It is precisely because this present book and its author have so much to commend them that it is worth calling certain drawbacks to the attention of legal and political science professionals. As we address our students, and from time to time other segments of the public, we are goind to find ourselves encountering vulgarized Barth. As is so often the case in assessing popular work, one feels boxed in by ancient cliches: "something is better than nothing," and "a little learning is a dangerous thing."

Certainly it is unreasonable to expect that books intended for a large nonspecialist audience will render constitutional law in all of its richness and complexity. These qualities must be substantially sacrificed if wide intellectual access to the text is sought. It is also necessary for a popular book in this field to proceed in a strongly opinionated fashion. We really cannot expect the general reader to be attracted to "some students say . . . but other commentators respond." While granting this, however, it can be asked that Barth do better next time.

Prophets With Honor is a celebration of six famous dissenting opinions: the first Justice Harlan in *Plessy* v. *Ferguson*, Brandeis in *Olmstead*, Black in *Betts* v. *Brady*, Stone in the first flag salute case, Black in *Colegrove* v. *Green*, and Douglas in *Dennis*. In each instance the background to the litigation is crisply sketched, and the eventual vindication of the dissenter's position is presented. The difficulty with the case studies involves the manner in which underlying issues of public policy are presented. Simplification is fine, but unreflective assertion of simplistic "truths" is not.

The chapter on *Colegrove* v. *Green*, to pick one example, opens with the assertion that "... when a democracy is to be ruled by representative government, it follows necessarily that it must be bottomed upon equality in the voting booth." Now it may well be that a democracy based on equality in the voting booth is the best kind to have, but to suggest that a tradition as variegated as representative democracy *necessarily* implies numerical equality in voting is seriously misleading. And Barth compounds the intellectual felony by immediately suggesting that the principal historical source of the meaning of political equality in the Anglo-American setting is the Leveller movement. Of course, Barth is free to prefer John Lilburne to John Locke (or twenty others one could mention), but this does not excuse misleading the innocent.

In instance after instance throughout the book, one is moved to wonder whether, on balance, it will help or harm most of those who read it. It is the custom of the academic specialist politely to ignore popular productions on his subject. It is thought unseemly these days to appear to speak from authority, or pull intellectual rank. But while avoiding pedantry is generally to be desired, academics in America must risk it a bit more, more often. With the vastly expanded population which is in some loose sense "college educated," the market for pop politics, pop law, pop sociology, and so on, is commercially very attractive. Journalists, both print and electronic, regularly debouch into complicated policy areas with hastily "researched" productions. It is just because Barth is the best sort of popular writer that his work deserves scholarly criticism.

RICHARD E. MORGAN

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Navigating the Rapids 1918–1971: From the Papers of Adolf A. Berle. Edited by Beatrice B. Berle and Travis B. Jacobs. (New York: Harcourt Brace Jovanovich, Inc., 1973. Pp. 859. $18.50.)

The Diaries of Edward R. Stettinius, Jr., 1943–1946. Edited by Thomas M. Campbell and George C. Herring. (New York: New View Points, 1975. Pp. 544. $12.50.)

The Crippled Giant: American Foreign Policy and Its Domestic Consequences. By Senator J. William Fulbright. (New York: Random House, 1972. Pp. 292. $6.95, cloth; $1.95, paper.)

John Foster Dulles: A Statesman and His Times. By Michael A. Guhin. (New York: Columbia University Press, 1972. Pp. 404. $12.95.)

The Storm Has Many Eyes: A Personal Narrative. By Henry Cabot Lodge. (New York: W. W. Norton and Co., 1973. Pp. 272. $7.50.)

Why a single review of these five dissimilar books? Presumably because Berle, Dulles, Fulbright, Lodge, and Stettinius were significant personalities involved in the shaping of American foreign policy in the momentous years of unprecedented American involvement in world politics – World War II, the creation of the United Nations, the cold war, the Suez Crisis, intervention in Korea and Vietnam. Indeed the span runs from the isolationist days of 1937 to the pull-back of United States troops from Southeast Asia. A central concern throughout: America's role in the world and conflicting perceptions of how to act within the changing international system to support American values and interests. These five statesmen played widely different roles in the global dramas of which Roosevelt and Marshall were the principal World War II actors, and Truman, Acheson, Eisenhower, Kennedy, Johnson, Nixon, and Kissinger were among the major postwar actors. Only Dulles (of our five characters) ranks as a top-level foreign policy decision maker (because of his great influence on Eisenhower).

These books serve various purposes, reflect different perspectives, and cover uneven time periods. Two are diaries (Berle, 1937–1971; Stettinius, 1943–1946). One (Lodge) is a personal narrative highlighting his career in domestic politics and diplomacy. The fourth book (Guhin) is a thoughtful re-interpretation of John Foster Dulles, the "Cold Warrior," an attempt to understand a statesman in the light of his environment and his times. And the fifth is J. William Fulbright's (Chairman, Senate Foreign Relations Committee) sweeping indictment of the Johnson and Nixon Administrations' foreign policies.

The most provocative of these books is that of Fulbright, written while American armed forces were still in Vietnam. The most scholarly of the five is Guhin's reevaluation of Secretary Dulles. In contrast the Stettinius Diaries, while throwing some light on the political in-fighting that surrounded the policy formulation of the Roosevelt presidency, is "lacking in candour." Its greater value is in portraying Stettinius's role in major diplomatic conferences engaged in planning for postwar international organization (Dumbarton Oaks, Yalta, Mexico City, and San Francisco).

Perhaps the most readable of these volumes is the personal narrative of the politician, statesman, and diplomat, Henry Cabot Lodge, a self-portrait of a public spirited American heavily involved in the cold war and in Vietnam. The Berle volume reveals the spirit of a brilliant New Deal lawyer, scholar, and public servant throughout his long career in and out of government but is most interesting for the World War II period when as Assistant Secretary of State he worked closely with Roosevelt and Hull. Dedicated to liberal democracy and suspicious of great power politics and imperialism, Berle had assisted Secretary Hull in formulating the principle of nonintervention at the Lima Conference (1938). Berle stood for a policy of continentalism and democratic inter-Americanism both before and after World War II, and pushed for unity of the New World Hemisphere. Indeed his predisposition towards regionalism never let him become a believer in "One World" as a means of organizing world order. He even had doubts about the wisdom of Lend Lease and the Roosevelt-Churchill Atlantic Charter of August 1941. Not surprisingly Berle was not invited to participate in the 1944 Dumbarton Oaks Conference (U.S., U.S.S.R., U.K., China), but was active at Mexico City (February 1945) where his efforts together with those of Nelson Rockefeller and Senator Warren Austin strengthened the emerging Inter-American System. Although not included in the delegation to San Francisco, his views and those of Latin American governments, gained partial acceptance.

The Berle volume also touches on the bureaucratic rivalries of the wartime Department of State. Hull vs. Sumner Welles, Berle vs. Acheson (a keen rivalry), and Hull vs. Secretary of the Treasury Morgenthau. Berle's influence in Washington depended heavily on his friendship with the president, his position in New York City politics (close to LaGuardia), and his personal capabilities as an economic and political analyst and skillful draftsman of presidential and other State papers.

The Diaries of Edward R. Stettinius, Jr., 1943–1946, is a one-volume selection from the voluminous Stettinius papers that the editors assert "fully document his three years at the center of American foreign policy" (p. xxvi). They have attempted to include "those entries which deal with the most significant problems Stettinius encountered" and "provide information not available elsewhere" (p. xxviii). These objectives are in general achieved, although those familiar with previous publications on the preparations for the drafting of the U.N. Charter may not be impressed with the freshness of the account. A fragmentary glimpse is provided of American diplomacy at Dumbarton Oaks, at Yalta, at Mexico City, and at San Francisco (April–June 1945). Indeed the evidence as set forth can hardly justify the authors' claim that "his work at Dumbarton Oaks, Yalta and San Francisco gives him solid claim to the title of architect of the United Nations" (p. xiii). It would be more accurate to characterize the United Nations as the product of much experience and of a large firm of architects! What the Diaries do not show adequately is the role of the State Department specialists and advisers in the shaping of the U.S. position. Thanks to improved relations with the White House which Stettinius had brought about following the resgination of Hull, the State Department took an intimate part in the deliberations at Yalta (p. 233), and Stettinius is credited as being "influential in winning over" F.D.R., Churchill, and the Russians to the Department's proposal on voting in the Security Council. The treatment of the veto issue at the San Francisco Conference is useful, but it would be difficult to evaluate the Mexico City Conference (February 1945) based on the spotty evidence presented. Indeed, the Berle account is more interesting (Berle, p. 472–4).

Stettinius was brought into the outdated State Department to reorganize and modernize. But in policy matters, FDR clearly wanted to be his own Secretary of State (an example later followed by Kennedy). As Stettinius lacked the political experience to have created a power base in his own right in the few months that he was Secretary, it became nearly inevitable that he would be replaced by President Truman at the earliest opportunity, the more so because Truman, with his own limited executive and administrative experience needed, in the ranking Cabinet post, some one who could add political strength to his administration. The authors are probably correct in saying that Stettinius played an important negotiating role, but in the substantive area of policy his impact was minimal.

John Foster Dulles: A Statesman and His Times is a strong reply to some of the earlier critics of Dulles, who have vented their animosity on him as a "moralist" and "Cold War ideologue." Guhin sets out to redress the balance and concludes that Dulles has been unjustly treated by many of his critics of the past two decades. The contention is that his basic philosophical foundations and assumptions on conflict and politics properly place him in "the realist school" (p. 2). Guhin wants us to understand that "the Secretary of State remains, to varying degrees, a prisoner of Washington politics, of the prevailing Congressional and national moods, of past policies, of international circumstances, and in a real sense, of the very complexities of the issues themselves and of the immediate demands and crises. . . . Since his personality colored almost every major facet of American foreign policy during the Eisenhower Administration, John Foster Dulles represented a very important ingredient in the overall policy process – but only one" (p. 314).

Guhin rejects Kennan's view of Dulles as one with a propensity toward "legalism" in international affairs (p. 88) and argues that in the Suez crisis of 1956 Dulles's moves followed a course of action consonant with perceived national interests "based on hard political power considerations" (p. 265). And if Prime Minister Eden continued to misunderstand Dulles's position (following Dulles's Press Conference of October 2, 1956), "then the misunderstanding probably stemmed from what has been described as 'wishful thinking,' not from Dulles's statements and from the views expressed in the Eisenhower letters" (p. 284). Guhin apparently finds that the world views of Dulles and Kennan were very much alike, and that "difference in style probably stemmed more from the fact that Kennan and Dulles often played disparate roles," for instance, Kennan "was never really confronted with the problem of rallying public and congressional support" (p. 157). As for Dulles's Southeast Asian policy in 1954 and his failure to support "Titoism in Vietnam," Guhin believes correctly that "a move resembling concession to unification under Ho Chi Minh would have spelled political disaster at home on a scale not greatly different than the so-called China debate" (p. 248). While conceding that Dulles made mistakes, he finds the later Dulles more mellow and less rigid than during his early years as Secretary. One of the best parts of Guhin's analysis is the "eleven principles and conclusions" regarding the nature of the Soviet-American conflict in the early cold war period, principles that for the most part guided Dulles's understanding of the cold war throughout the remainder of his career. Not all students of Dulles will share Guhin's interpretations; but his is a serious effort with considerable validity. Nonetheless, because he underemphasized the McCarthy years and Dulles's solo performance in policy formation, Guhin should be read together with Townsend Hoopes, *The Devil and John Foster Dulles* (Little, Brown and Company, 1973).

The most important of these books is that of Fulbright, strong defender of democratic, constitutional government and sharp critic of the imperial presidency. The Arkansas Senator calls Congress to task for abdicating to the executive its war and treaty powers. In the 1960s the United States, he believes, came too close to becoming a traditional empire, while neglecting urgent needs of the American people at home. This distortion resulted from applying the unilateralism of the Truman Doctrine to Asia in particular. He argues that the United Nations did not fail, that in reality the will was lacking to use it as an instrument of collective security. His emphasis is on "the dangerous obsoleteness of traditional power politics in the nuclear age" (p. 156). It was the misadventure in Vietnam and related policies of the mid-1960s that brought Fulbright, an early supporter of the containment policy, to urge that we "recover our [traditional] mistrust of power – in the Presidency and wherever else it is found" (p. 242). He asserts (in 1972) that once again the American people have the responsibility to decide where they want to go in the world . . . what kind of country they want America to be. Fulbright would have us return to "first principles" and acknowledge "the inseparability of foreign and domestic activities."

The strength of Fulbright's analysis is his contention that "the liturgy of realism" warrants review. The weakness of his analysis consists in his deemphasis of the realities of the political world which blocked the creation of a genuine collective security system during the past generation. The political nationalisms of both the U.S.A. and the U.S.S.R. (and a host of other countries large and small), the new threat of nuclear weapons and other advanced weapons systems held by the super powers and a growing number of other states, the political explosions within the former colonial empires, and other revolutionary developments, magnified the difficulty of operationalizing the liberal Wilsonian concept of world-wide collective security against international aggression. Fulbright's admirable rule of reason would have exercised greater control in a more stable age. "The liturgy of realism" in power politics which he decries is clearly only part of the problem. How can the unreasonable, aggressive drives in human and national behaviors be controlled? Resort to violence within countries

(Lebanon, North Ireland, Pakistan-Bangladesh, Portugal, Angola) as well as between states (U.S.S.R.-Hungary, India-China, India-Pakistan, Egypt-Israel-Syria, U.S.S.R.-Czechoslovakia, Cyprus, Vietnam), is a frequent characteristic of our world. Fulbright demanded and got reappraisal of U.S. policy in Asia. He correctly asserts that U.S. decision makers misperceived how to safeguard the national interests in Vietnam in the 1960s, but his analysis of the malaise of our times probably exaggerates the blame that can correctly be placed on American leaders. The multiple wars and revolutions of this century – the technological, political and cultural dynamism – have thrust the world into a stormy sea of uncertainty and danger. On balance American leadership has been a healthy, statesman-like effort to cope with a period of staggering complexity and upheaval in world affairs. We shall need to do better in the years ahead. Fulbright has given a warning. But neither he nor the others have systematically analyzed current options as faced by today's decision makers.

The Storm Has Many Eyes: A Personal Narrative is a short, fascinating sketch by Henry Cabot Lodge of his long career in public service for his country. When in 1960 he lost the race for Vice-President, he had already served 28 years in some sort of public office, chiefly as U.S. Senator, with the U.S. Army during World War II, and as United States Ambassador to the United Nations (1953 to September 1960). His role in the "Draft Eisenhower for President Movement" (1952), and as Ike's campaign manager is interesting. Unlike Stettinius and Dulles, Lodge never became Secretary of State, although in 1952 General George C. Marshall expressed the hope that he would. In foreign policy Lodge was a strong supporter of the Truman Doctrine, the Marshall Plan, and U.S. containment policy. After having served as Ambassador to Vietnam (1963–4 and 1965–67) he wrote (1973) that if the Indochina question had been wisely handled, "the United States need never have gone there" (p. 205); but that, when President Kennedy had asked Lodge to become Ambassador to Vietnam, he accepted as "a duty," because "regardless of how we got there, Americans were in Vietnam and were in combat" (p. 205). As for the coup against Diem in November 1963, he refutes the charge made in the *Pentagon Papers* that he authorized CIA participation in tactical planning of the coup. "I well remember that I was specially ordered by the President not to help in the planning, and that I scrupulously obeyed orders." He states that "the allegation in the *Pentagon Papers* that in October we cut off aid to Diem in order to give a green light to the generals is wrong. It was done in order to get Diem to strengthen his political position at home by sending brother Nhu out of the country" (pp. 210, 211).

As for the value of the United Nations, Lodge generally favored collective security (pointing to the prompt U.S. response to aggression in Korea in 1950 as making "for a more peaceful world" (p. 226). With reference to Vietnam, he states that the UN did nothing "partly because it lacked the tools, but fundamentally because it lacked the will" (a confidential survey made by the Johnson Administration found only about 12 votes could be found to support a U.N. initiative in Vietnam) (p. 151). He predicted that the U.N. "will cease to be a respectable organization" if it fails to block the use of force by members in cases other than self-defense (pp. 152–153). On the other hand he comments that "a blind determination to stop aggression regardless of the cost is as dangerous as a blind determination never to suppress it at all." In each case of aggression, he asks for prior measurement of the costs of acting and of not acting militarily (p. 225).

On the question of secrecy in foreign policy behavior, Lodge notes the dilemma, "because free speech is at the heart of our system, the problem is not solved simply by saying that the public has a right to know. The public also has a right to be protected against the dangers caused by injudicious revelation of official secrets" (p. 226).

Lodge would have the American people retain enthusiasm for the ideals of the Declaration of Independence and goals of the Preamble to the Constitution, as the bicentennial of the nation's independence departs. The five American statesmen could agree on that, and on much else, despite differences of role and interpretation, and perhaps many of the rest of us could agree that they took quite seriously their own responsibilities to serve the national interest in the light of their best understanding of a dynamic age. While a post-Vietnam agenda for America is quite different, there is value in pondering these books even now, primarily as a reminder that the evaluation of foreign policy in the midst of a sweeping torrent of world events constitutes a controversial undertaking heavily weighted with value-laden assumptions and misperceptions.

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The Free Soilers: Third Party Politics, 1848–54. By Frederick J. Blue. (Urbana: University of Illinois Press, 1973. Pp. 350. $10.95.)

At a time when Walter Dean Burnham, James Sundquist, Everett Carll Ladd, and others are presenting hypotheses about party development and patterns of political realignment, a book like *The Free Soilers*, by Frederick J. Blue, is particularly useful.

Professor Blue is an historian. His book is a monograph, originally a doctoral dissertation, and it bears the marks of this genre. Readers who are in a hurry will be impatient with the pains taken to describe each aspect of the Free Soil convention of 1848, or each gesture and feint in the transition in Massachusetts from Conscience Whig, via Free Soil and Free Democrat, to Republican parties. But those who are developing hypotheses about third parties or the realignment process can be grateful to Professor Blue, and to the University of Illinois Press, for a book that is based directly on primary sources and is sufficiently detailed to inspire confidence that generalizations carefully derived from it are on sound ground.

The pity is that this book, so meticulously researched, is itself so weak in interpretation. Compare, for example, the boldness of *The Price of Union*, by Herbert Agar (Boston: Houghton Mifflin Company, 1950). Agar argues that Lewis Cass, the Democratic nominee in 1848, "might have won New York if it had not been for the Free Soil Party, and if he had won New York he would have won the presidency" (p. 325). In a footnote, Agar adds that without Free Soil, the Whig candidate, Zachary Taylor, would probably have won Indiana and Ohio, and thus the election, even without New York.

Blue is more straightforward, asserting simply that "the Free Soil party did not change the outcome of the election" (p. 144). Having stated this conclusion, he embarks on a detailed examination of election returns, focusing on several counties of the more significant states: New York, Massachusetts, Ohio, Michigan. Heavy with tabular summaries and other details, his analysis nevertheless remains impressionistic and inconclusive. At one point, Blue wearily remarks that "the results of the election of 1848 are not easily summarized" (p. 149). The muse of V. O. Key is desperately needed in these pages. Although he devotes ten pages to the effort, Blue fails to provide a convincing explanation of the impact of the Free Soil campaign on overall voting patterns in 1848.

But the book is best taken for what it purports to be: "a leadership study . . . based primarily on the writings of the major participants" (p. x). Blue probes deeply into the machinations of countless politicians whose restlessness produced the third parties of the 1840s and 1850s. The opportunism of Salmon P. Chase, the hesitant idealism of Charles Francis Adams, the awkward strength of Joshua Giddings, the cunning vengeance of Martin Van Buren – all are presented here, not always with the imagination and art with which these figures have been treated by more gifted writers than Blue, but clearly and concisely.

One wishes that greater attention had been paid to the economic and social conditions that contributed to the rise of political "anti-extensionism" in 1844 and 1848, its deceptive eclipse between 1851 and 1853, and its dramatic revival during the Kansas-Nebraska Act debates. One is grateful to be spared a *superficial* treatment of the environing conditions, but when Blue is stating the differences between the impact of Van Buren's campaign of 1848 on Michigan and Wisconsin, or the Western Reserve and Southern Ohio, a sense of the social environment would have enriched the account of personal and political interactions.

In the absence of serious attention to these other dimensions, Blue is often unable to tell why important developments occurred. For example, the book concludes with a brief discussion of the transition from the dying Free Soil to the new and vigorous Republican party. He sets the stage by noting the collapse of the leaderless Whigs and the fecklessness of the Pierce Administration. Then, out of the blue comes the "catalyst": Stephen Douglas's bill to organize Kansas and Nebraska on the basis of squatter sovereignty.

No theory is offered to explain why the astute Douglas gave such opportunity to his opponents. (Others have suggested that Douglas was desperate to foil Senator Jefferson Davis's plan to build the transcontinental railroad through the newly acquired Gadsden Purchase.) Nor is an adequate explanation given of how the Republicans succeeded where the Free Soilers had failed, despite the similarity of their programs. Blue does allude to "the difference in the temper of the times" (p. 287), but this weak phrase is now seen by most social scientists as a confession of defeat, rather than a triumph in analysis.

But again, I am close to complaining that Blue has not written a different book. On its own terms, it makes a useful contribution, one that will be welcomed by students of this most difficult period in American political history.

DONALD ALLEN ROBINSON

Smith College

Busing: The Political and Judicial Process. By James Bolner and Robert Shanley. (New York: Praeger Publishers, 1974. Pp. 272. $17.50.)

There are many books on busing. This book, however, substantially differs from these other (mostly case) studies. For here, the emphasis is not upon the locality's busing experience, but rather, upon busing as a *national, political* issue. While the authors do examine two local implementations of busing, their primary concern is the busing issue at the hands of Congress, the courts (especially the U.S. Supreme Court), and the president and the executive branch.

The authors are eclectic in their methodology in this study. The court case approach, the case study method, content analysis, roll-call analysis, and more recently developed statistical techniques are all employed at different places in this book – no single approach being permitted to dominate the others. Of these different approaches, the traditional court case analysis is particularly strong. The authors are able to identify for us most of the recent legal doctrine on busing – including old doctrine in new guises (e.g., Justice Douglas's resurrection of "separate but equal") and in new disguises (e.g., "separate but equal" now read as "compensatory education").

In their final two chapters, the authors – while not ignoring him – look beyond the undergraduate student of political science and his needs, and speak also to the graduate student, the administrator, and the school-board member. The findings of other busing studies are compiled and alternative means of working toward school desegregation are set forth.

The ambitiously broad scope of this book is its greatest asset. That breadth enables the reader to see, so much more than any other study, the complex interrelations of the different participants in the busing controversy. For instance, of course we read about *Swann v. Charlotte-Mecklenburg;* we learn that in that case the U.S. Supreme Court permitted a district court to order busing as a remedy for *de jure* school segregation. But we are given a so much fuller and truer picture of what that decision has meant and has not meant, when the authors supplement analysis of *Swann* with the observations that: (1) *Swann* primarily applies to school segregation in the southern states; (2) President Nixon expressed strong opposition to busing in his 1972 reelection campaign; (3) in his reelection he received unprecedentedly heavy support from southern cities; (4) while publicly expressing his duty to comply with *Swann,* the president sent memos to the Departments of Justice and Health, Education, and Welfare, threatening transferring or firing of any officials who deviated from the policy of keeping compliance with *Swann* as "bare-bones" as possible (p. 149). *Swann,* we now see, was not the winning of the battle, but only the "opening shot." The "wider picture" which this book presents, allows us to make sense of much which before didn't quite make sense. As I see it, this breadth is the book's greatest virtue.

Unfortunately, the book has shortcomings too. It is badly balanced: there is too much description and too little explanation, analysis, and evaluation. Statements are made which need support and elaboration – but get neither. For instance, the authors posit that opposition to busing is really just a symbolic expression of fear of all school desegregation (with or without busing) and fear of residential integration. In other words, the heated opposition to busing is but a statement of the depth of racism in America. This is an interesting – if not particularly novel – hypothesis. But it is not a self-evidently true one. (Does it account, for instance, for the strong opposition to busing – approximately 40 per cent – in the black community?) Such an hypothesis *might* be correct: let us hear the arguments and explanations; let us see the supporting data. The arguments are not worked out for us; the explanations and testing data are not presented.

Bolner, Shanley, and the Praeger Publishing Company commit other – easily avoidable – errors which sorely detract from the worth of this study. The book is often sloppily written and presented. Terms are used (e.g., action by the Supreme Court "on certification" (p. 38), "magnet schools" (p. 40) which require but do not receive definition and explanation. There is much bad editing, rough and inexact expression, and many grammatical and typographical errors. (In this book's 227 pages of text, I count 35 such errors.) The reader finds his train of thought continually interrupted and deflected by these altogether needless distractions. Putting the matter as kindly as possible, the book has a very unfinished feel.

The scope of this book – its locating of the political-political and the political-judicial interrelationships at all levels of the American political process – is broader, more "daring" and more useful than any book on busing which has preceded it. Unfortunately, it is also true – for the reasons I have pointed to – that the study loses some of its "understandableness" and thus, some of its utility. *Busing: The*

Political and Judicial Process is, to paraphrase Ralph Waldo Emerson, a deliverance which doesn't quite or completely deliver.

HOWARD SHERAIN

California State University, Long Beach

The Political Economy of America's Environmental Dilemma. By Michael J. Brenner. (Lexington, Mass.: Lexington Books, 1973. Pp. xx, 177. $12.00.)

The major virtue of this broad, theoretical study is its critique of the approaches of mainstream political science and economics by a committed environmentalist. Brenner shows how pluralists failed to anticipate environmental concerns and are unable to conceptually deal with these problems.

This insightful work assesses major ideas and institutions affecting environmental programs in addition to making some tentative suggestions for reform. While specific environmental issues, especially transportation, are discussed, Brenner's orientation is policy analysis in its more philosophical sense. And it is a call for more comprehensive and collective-interest types of environmental policies and programs.

Brenner begins with a useful distinction between tangibly adverse environmental effects which can be viewed in quantitative economic terms and those which are less hazardous to health, but nevertheless are equally important as social "disamenities" affecting quality of life. In a penetrating survey of political science literature, he shows how its liberal incrementalist bias hinders coordinated, anticipatory planning which would give a higher priority to environmental concerns. He claims that the aversion of leading political scientists to a broad, public-interest orientation hampers environmental policy formulation and implementation. Similarly, the biases of most economists (Galbraith significantly excepted and admired) to tangible benefits and growth goals has made it difficult to reach a fuller understanding of environmental impacts.

In an especially valuable chapter, Professor Brenner contrasts the values of environmentalists with those of more traditional liberals, welfarists, and pluralists. At the same time, he criticizes Marxist approaches for being conceptually and descriptively inadequate. In addition, he discusses the environmental implications of a wide variety of policy ideas, from PPBS to effluent charges and revenue sharing. Finally, he uses mass transportation as a case study to illustrate "the inadequacies of our present approaches to environmental problems, the biases of our political system, and the distortions of dominant economic theories" (p. 51). He finds transportation policy to be the best example of the operation of the market myth, pluralist politics, incrementalism, and an aversion to public expenditures.

Brenner, therefore, pleads for a more systematic, rational, and communal perspective to prevent environmental misfeasance and disjointed planning. He advocates "planning for human use [which] depends on some explicit conception of human good, of a desirable social condition" (p. 66). He is a qualified pessimist, however, about changing approaches and specific policies. He does favor regulatory agencies, along with a strong emphasis on technology assessment and proposes using the Cabinet as an administrative coordinating mechanism for environmental purposes. On a longer-term basis, he discusses the interdisciplinary role the university can play in addressing environmental problems.

Political scientists may quibble over the feasibility of Brenner's policy reforms. More fundamentally, one can question whether educational reform which is confined to the university is sufficiently basic to change our system of beliefs, or whether environmental institutions can be analyzed almost exclusively at the federal level without a much broader exposition of the key role played by state and local agencies.

Brenner fails to resolve the inherent dilemma of environment as an issue which in theory should transcend conventional interest-group politics and disjointed planning, but in practice does not. Nevertheless, this is a useful contribution to the emerging literature that places environmental problems in a broader political and economic context.

HARVEY LIEBER

The American University

The Civil Aeronautics Board. By Robert Burkhardt. (Dulles International Airport, Virginia: The Green Hills Publishing Co., Inc., 1974. Pp. 222. $19.50.)

The "independent" regulatory agencies of the United States Government (CAB, FCC, FMC, FTC, ICC, SEC, etc.) are currently the subject of a tide of skeptical criticism. They are assailed from the political right for hobbling "free enterprise" and from the political left as protectors of the giant corporations they are supposed to regulate. Moreover, those agencies charged with the economic regulation of our common carrier transportation systems face criticism by some economists that the whole theory underlying such regulation is erroneous and results in less efficiency and greater expense for the consumer.

The book under review is a study of one of these agencies, the Civil Aeronautics Board, written by a veteran reporter, writer, and editor for the aviation trade press who earlier authored a book on the executive agency responsible for air navigation and safety – *The Federal Aviation Administration* (New York: Praeger Publishers, 1967). From close observation over the years the author seeks to show how this agency really operates – the procedures, pressures, and even the personalities which result in the output of policies and decisions determining the routes, rates, and practices of the airlines.

The first two chapters, and several later chapters, deal with specific CAB cases and airline problems, and would be of minimal interest to a political scientist. But the next two chapters involve the reader in the relationships among CAB staff components, between CAB and Congress, and between CAB and the president. Certain later chapters discuss pressures from the airlines and their lobbying associations, from consumer groups, and from segments of the financial community such as life insurance companies which hold much of the massive debt that enables the airlines to purchase their expensive fleets. The balancing of different aspects of the public interest and the parallel balancing of pressures both from within and without the government are shown, using specific cases as illustrative.

Other chapters consider the complex relationships within the government with respect to international airline service, and we see the "independent" regulatory agency enmeshed in the executive-dominated foreign relations field. Then Burkhardt turns his attention to interactions of foreign governments and their airlines with U.S. government agencies, notably the CAB and the State Department.

No one outside the aviation field is likely to wish to read this whole book through. Indeed, the author might be criticized for attempting to do two different jobs: (1) to analyze relationships and pressures within and about a government agency, and (2) to familiarize a reader with the economic problems of the airlines. Moreover, the author has not been deeply analytical in pursuit of either task. Often he expresses opinions without showing the other side or sides of the argument or even indicating that another side exists.

Yet, accepted for what it is rather than for what it is not, the book might be of value to a student of American government, perhaps as a counterweight to the excessively theoretical or legalistic treatment that the study of the workings of government often receives.

WILLIAM E. O'CONNOR

Embry-Riddle Aeronautical University

Urban Politics and Decentralization: The Case of General Revenue Sharing. By David A. Caputo and Richard L. Cole. (Lexington, Mass.: Lexington Books, D. C. Heath and Co., 1974. Pp. xix, 180. $15.00.)

General revenue sharing, which is perhaps the most significant domestic policy legacy of the Nixon Administration, is certainly one of the most studied phenomena in current political science. Among the earliest workers in this vineyard have been Professors David A. Caputo of Purdue and Richard L. Cole of George Washington. The book under discussion here reports the findings of two surveys they made of all U.S. cities of over 50,000 population, in January of 1973 and 1974. Their response rate was approximately 50 per cent, or about 200 cities.

Caputo and Cole's data are in some ways problematical, since most of those working on revenue sharing have found that the official reports filed by communities and the "official" responses of city managers and finance officers tend to obscure the actual uses of the funds as much as to illuminate them. Two values, however, survive that problem. First, the officials' views of what it is best to report (e.g., that the money is being spent on bricks and mortar or on public safety services, rather than on more controversial human service programs of an exploratory nature) themselves constitute interesting data about current perceptions of what the public will "take." Second, the context of questions that the authors establish is thoughtful and suggestive of further research worth doing as ways are found to insure more accurate data (perhaps working from trend-line bases, etc.). The context they chose is to throw revenue-sharing decision making against the benchmark of urban policy making, and to ask how the expenditure decisions were made in each city, and what impact differential degrees of public participation in that decision making had on the substantive directions of policy. Further, they pose general theoretical questions of the political and environmental factors associated with policy innovation at the municipal level. Although the data are far from conclusive, their evidence does provide a substantial counterweight to the looser rhetoric of those politicians and local taxpayer groups that have contended that a great burst of urban innovation would occur if only the strings of categorical grants were untied and federal funds were put at the disposal of local imaginations.

Alas, no such burst has been evident. One has to admit, however, that the case is quite inconclusive, because the circumstances have changed so much between the time when Heller, Pechman, and others argued for revenue sharing as a way of applying a contemplated federal surplus (!) to local public sector needs, and the present, when most cities of any size have had to swallow up all of their revenue sharing grants in their regular operating budgets, often vainly attempting to maintain services despite galloping inflation.

Perhaps the moral of the story is that our ability to engage in effective policy planning is still sharply limited by our inability to control all the contextual elements within which we would "engineer" a policy or program change. In any case, Caputo and Cole have done a thoughtful job of placing some of the early data on shared revenue allocations into a meaningful perspective. Students of the subject should heed their materials and their thought-provoking questions.

MICHAEL D. REAGAN

University of California, Riverside

Maryland's Revolution of Government 1689–1692. By Lois Green Carr and David William Jordan. (Ithaca, N.Y.: Cornell University Press, 1974. Pp. xviii, 321. $15.00.)

On August 1, 1689, Lord Baltimore's proprietary government surrendered its authority to a group of "loyall Protestant Subjects Inhabitants of the Province of Maryland." The insurgent Associators ruled the colony during the next three, surprisingly uneventful years. Royal government was established with the arrival of Governor Lionel Copley in 1692. So ended the virtually bloodless revolution. Except for the (temporary) demise of the proprietary, nothing much had happened.

Why, then, is the Glorious Revolution in Maryland interesting? Historians are of two minds about this and similar revolts in New York and Massachusetts. According to one view, they reflected fundamental tensions in the imperial constitution. Working this vein, David Lovejoy's *Glorious Revolution in America* (New York: Harper & Row, 1972) finds different colonies seeking similar constitutional guarantees for autonomy and equality in the empire. The ongoing search for the ideological origins of the American Revolution encourages such an approach. The antiauthoritarian, republican predelictions of the Americans were fully developed before the Imperial Crisis. Could this "ideology" have already taken hold by 1689?

The other interpretation sees these same colonies in the throes of a transition from "colony to province": colonial polities were undergoing the growing pains of modernization, this transformation was accompanied by political and social instability and violence; the "constitutionalism" of the revolts was therefore entirely fortuitous.

Maryland's Revolution of Government does not support either view. Indeed, Lovejoy's revolution seems to have taken place elsewhere. And though Carr and Jordan propose to devise "a model for examinig the interaction of institutional development and social change" in other colonies (p. xiv), they conclude that the "supposition that these events were visible evidence of a period of great social disruption is false" (p. 230). This is the book's dilemma: the inapplicability of either the constitutionalist or modernization model leaves its various themes largely disconnected. There is a fine chapter on the social context of the revolt; an extended treatment of the issues that rankled the last proprietary assembles; and a good chapter on imperial politics. But "whether or not Lord Baltimore retained control . . . apparently had little to do with . . . basic social and economic developments" (p. xiv). These themes *do* converge in subsequent decades, but not within the limited time period of this study.

In the meantime, Carr and Jordan make some interesting suggestions about the sources of political stability in Maryland. Their analysis of the emerging provincial elite is superb. They argue that "in local government there was political as well as institutional continuity, and together these provided stability. . . . The struggle for power at the top was kept under control by pressure for order at the broader based county level" (p. 139). The elite leadership pool was extremely small, including perhaps 290 heads of households in 1689 (p. 185). Though the Calvert-Catholic group had monopolized the most prestigious and lucrative positions, there was more than enough status to go around at the local level. This was a stabilizing factor. At the same time, stable "hereditary local power groups" had not yet formed (p. 187). But order was nonetheless maintained, perhaps because of "traditions of behavior established for exercising power," and, among the citizenry, for submitting to it. These behavioral patterns might have been all the more effective precisely when the legitimacy of all government was cast into doubt by the overthrow of the proprietary.

The real crisis for a maturing colonial society comes with the over-enlargement of the leadership pool and subsequent chaos and confusion in leadership selection. The formation of a stable, native elite in the years following – one that would later be challenged by the return of the proprietor – marks the real passage from colony to province in Maryland. This story is begun in the present volume; it needs to be completed before the authors will have done full justice to their provocative suggestions and prodigious research. One hopes Carr and Jordan, so eminently qualified, will see fit to do so.

The current interpretative paradigms of early American history place an inordinate stress on events like the Glorious Revolution, either as political-constitutional type, or social symptom. Regrettably, these paradigms have dictated the scope of this study; still – undeniably – it is the definitive work on its subject.

PETER S. ONUF

Columbia University

Planners and Local Politics: Impossible Dreams. Volume 7 of the Sage Library of Social Research. By Anthony James Catanese. (Beverly Hills, Calif.: Sage Publications, 1974. Pp. 189. $10.00, cloth; $6.00, paper.)

The subtitle of this short volume succinctly captures Anthony James Catanese's assessment of the role of planning in urban America. In his view planners and the planning process are inevitably at odds with politicians and the political process. Planners are quixotic figures (pp. 30–31) dreaming impossible dreams of a rational world divorced from the parochial concerns of special interest groups, while politicians are Machiavellian figures dedicated primarily to maintaining themselves in power through accommodations they work out with the very interest groups that the planners avoid.

For planning to make a significant impact on urban problems, Professor Catanese believes that some basic changes must occur in the relationships between planners, politicians, and the citizenry. He outlines three alternative planning roles – a technocratic role, a participatory role, and an activist role. The technocratic role is the traditional planning role of claiming to speak for the common good of the community, eschewing attachment to special interests, and solving urban problems through the application of technical expertise. The participatory role, which is similar to that of advocacy planning, rejects the notion of planning for a public good and instead seeks to involve people in ways that will create political support for the planning objectives. Catanese rejects both of these roles in favor of the activist role. In this role, planners are deeply involved in the political process, often "serving as political aides to elected leaders" (p. 18).

If more planners will just adopt the activist role, Catanese is quite optimistic that they will contribute more significantly to dealing with urban problems. His prescriptions for maximizing planning's potential contributions are for more planners to adopt the "overt-activist role" (p. 146), for planners to become more like politicians, for politicians to become more like planners, and for planners to view themselves as "*managers of change*" (p. 160) rather than as drafters of planning documents.

In coming to these conclusions, Catanese presents a critical analysis of the intellectual and ideological underpinnings of much contemporary urban planning. Of particular note is his rejection of the systems analysis and PPBS approaches, which he claims have been adapted uncritically by contemporary urban planners. These approaches, which worked so well in the space mission, do not necessarily work for the "urban mission" (pp. 54–60).

Because of Catanese's stature as a planner as well as scholar, his views are of considerable interest to political analysts. Nevertheless, professional political scientists will find the book marred by serious shortcomings. The analysis relies excessively on extremely short synopses of numerous case studies, none of which are examined in much depth. This is not a very systematic way of presenting evidence to support the author's many generalizations. And some of the generalizations seem highly dubious in the light of recent empirical research. For example, the assertion that "Retention of political power is of paramount importance to the state and local politicians" (p. 69) is too sweeping in light of the high turnover rates in state and local public offices and in light of James David Barber's findings about the reluctance of many state legislators even to serve in office (*The Lawmakers*. [New Haven: Yale University Press, 1964]). Finally, the analysis is poorly documented. There are only 71 footnotes in the entire book.

While the above criticisms are valid and will certainly bother the critical reader, some of the criticisms may be irrelevant to Catanese's purpose. He specifically rejected writing the book as "yet another academic treatise" (p. 9). For these reasons, scholars might find more substance in Catanese's other contributions, such as his *Scientific Methods of Urban Analysis* (Champaign-Urbana: University of Illinois Press, 1972). And for more complete analyses of planning and politics, they may be better advised to turn to works such as David Ranney, *Planning and Politics in the Metropolis* (Columbus, Ohio: Charles Merrill, 1969) or Thad L. Beyle and George T. Lathrop, eds., *Planning and Politics: Uneasy Partnership* (New York: Odyssey Press, 1970).

While *Planning and Local Politics* will be of limited utility to scholars of the planning process, its brevity, viewpoint, and straightforwardness of writing style make it very valuable for the many undergraduate students who are currently entering M.A. programs in urban and regional planning in the hopes of finding something useful and meaningful to do with their university educations. This book is pervaded by a sense of *realpolitik* about the limits on the roles which idealistic altruists can play in a political world that is dominated by interests which are neither idealistic nor altruistic. Coming from an author who has engaged with considerable success in the planning world, the political world, and the scholarly world, the ideas in this book would seem well worth pondering by those about to embark on the planning venture. Of particular interest would be Catanese's prescription that they should not seek to spend more than a small minority of their careers in public service. Rather, they should spend the majority of their careers in business, consulting, or education. This, Catanese feels, would give the planners a better perspective on public problems and would also make their proposals more palatable to these other groups.

JOHN J. HARRIGAN

Hamline University

The Witness and I. By O. Edmund Clubb. (New York: Columbia University Press, 1974. Pp. 314. $9.95.)

In the early '50s, when Joseph McCarthy's irresponsible charges of "Communist" agents, influence and activities in government dominated headlines across the country, no group was more victimized than the China Service of the Department of State. Four capable and intelligent Foreign Service officers bore the brunt of those attacks: O. Edmund Clubb, John P. Davies, John S. Service and John Carter Vincent. Each was finally dismissed or induced to resign. For years, they suffered in silence. But recently, with the lessons learned through Vietnam and Watergate and the end of official American antagonism towards the People's Republic of China, it has become possible for them to speak out. In the present volume, O. Edmund Clubb is not so much concerned with U.S. China policy as with his experiences before the State Department's Loyalty Security Board, and the "inner workings of a republican government led to resort to devices of authoritarianism" (p. xii).

It is a fascinating, frightening, Kafkaesque tale. Earlier scholars have commented on the Truman administration's self-protective appointment of Republicans and conservatives to the various loyalty boards. (See, e.g., Alan D. Harper, *The Politics of Loyalty: The White House and the Communist Issue, 1946–1952* [Westport: Greenwood Press, 1969], pp. 47–53.) Clubb shows us the consequences of that policy in vivid human terms. We learn, therefore, a great deal about the inner workings of the government's loyalty program, matters which were previously shielded by the confidentiality of the proceedings.

On January 2, 1951, Clubb was handed an interrogatory. In it, the Loyalty Security Board (LSB) listed eight vague allegations about "pink" tendencies, or "marked preference for some Communist principles" in the period between 1931 and 1940. It asked about his relations with such people as Agnes Smedley and Owen Lattimore. Finally, the LSB inquired if he had, in 1932, delivered a "sealed envelope" to the left-wing *New Masses* magazine in New York (pp. 28–30). By June 27, following Clubb's written reply to the allegations, the LSB adopted the allegations as its own charges and Clubb was suspended (pp. 126–128). In the hearings that followed, Clubb was never allowed to see the evidence against him, to confront his accusers, or even to know their identities. In his words, "The LSB sat in the joint capacity of accuser, prosecutor, judge, and jury in a matter where the accused was not enabled to know what it was all about" (p. 147).

Of course, the injustice of this procedure was fully consistent with the regulations of the loyalty program. What is most striking and new in Clubb's account is the evidence that the LSB was often more zealous than the regulations required or, in fact, allowed. On several occasions, the Board misinterpreted or *expanded upon* allegations against Clubb instead of either questioning the allegations or checking with their source (pp. 162–164). Elsewhere, the regulations were clearly controvened, as when information was leaked to HUAC, in clear violation of the principle of confidentiality of the hearings.

One such incident of LSB-HUAC cooperation is particularly noteworthy. By reference to his diary, Clubb was able to establish that he had indeed visited the *New Masses* in 1932. He had carried a letter of introduction to an editor who had already left the magazine, and was introduced to the editor's replacement: Whittaker Chambers. Any reasonable reading of the diary entry (pp. 110–111) indicates Clubb's discomfort in the offices of the *New Masses* (which he calls a "horrible rag") and his distaste for the "shifty-eyed unkempt" Chambers. Nonetheless, the LSB chairman purported to find a "Red thread" running through the entry and demanded to see the entire diary (pp. 182–189). When Clubb, despite warnings that it could be "prejudicial" to his case (p. 191), insisted on his constitutional right to privacy, he was quickly confronted with a subpoena from HUAC to appear with his diary (p. 192). This compelled Clubb to turn the diary over to the Department for clearance. When the diary came back from the House, Clubb was informed that "the Department in its discretion has seen fit to turn over the diaries for the examination of the Board" (p. 228), which then embarked on a rather extended fishing expedition. In the end, Clubb was found a poor security risk for being, he was told, "less than frank with the Board" (p. 256). An appeal reversed the negative finding, but Clubb's career was clearly finished. Assigned to the State Department's Division of Historical Research, he chose instead to resign.

Clubb's narrative is compelling and highly readable. His assessment of the effects of the loyalty program seems sound: an "urge toward conformity" (p. 283) supported a "Cold War mentality" (p. 291) which in turn justified America's "own colonial Indochina War" (p. 293). What is less compelling is his consistent analysis of this phenomenon in terms of the "important military element" in the State Department following State's absorption of the various wartime agencies (p. 31). Here Clubb is clearly reflecting the mistreatment he received from the two former military men on his review board and from the ex-colonel who headed the security program (pp. 146–147, 267). But I question the rather typical academic liberal belief that all the ills of this era can be attributed to a military mentality. After all, the General Snow who chaired both the full LSB and Clubb's own hearing had been a Rhodes Scholar, an editor of the *Harvard Law Review*, and for twenty-one years a lawyer in New Hampshire. In fact, of the 59 persons who had served on the LSB up to 1952, the twenty who had served in the military were outnumbered by the twenty-two who had attended Ivy League schools. There was clearly more to the "Cold War mentality" than military influence. It could infect even those of the best education and upbringing. And it did.

JOSEPH W. ESHERICK

University of Oregon

The Public's Impact on Foreign Policy. By Bernard C. Cohen. (Boston: Little, Brown and Company, 1973. Pp. 222. $4.95, paper.)

"Unless politics were involved, Mr. Clifford would not even be at this conference. This is a serious matter of foreign policy determination, and the question of politics and political opinion does not enter into it." The presence of White House counsel Clark Clifford at a May 1948 meeting to consider recognition of a Jewish state in Palestine triggered this outburst by Secretary of State George C. Marshall.

The incident illustrates several of the main themes in Bernard Cohen's analysis of the linkage between public opinion and foreign policy. Marshall was appealing to the norm widely held by American foreign policy elites that domestic politics should not have any impact on the decisions and actions of the American government. Nevertheless, it does: Truman allowed Clifford to remain, and even though Marshall's determined stand temporarily forestalled recognition, domestic politics eventually triumphed. The result would not surprise Cohen. Public Opinion, writ large, has little discernible impact on foreign policy, he argues. Sometimes particular segments of the general public do have an impact, but it is only through the mediation of officials directly involved in the policy process who either act as surrogates for them or choose to heed their voices for reasons of their own. Public opinion matters insofar as officials' perceptions of it matter *to them.*

It is his attempt to reorient the theory linking public opinion to foreign policy that makes Professor Cohen's book worth the time of every student of the subject. If he does not draw a detailed roadmap for the theory builders and data gatherers, Cohen at least erects signposts to indicate the direction in which to proceed.

Obligatory obeisance to the dictates of normative democratic theory and professional preoccupation with methodology, Cohen suggests, have diverted social scientists from a close examination of existing explanations of the linkage between popular views and governmental action, and have inhibited empirically founded theory building. There is conceivably more ideological claptrap written about public opinion and public policy than about any other topic in American political science. Moreover, the intellectual fascination with survey research techniques has led to elaborate descriptions of public opinion without much attempt to show what difference it makes to policy outcomes. Case studies of decision making seldom introduce public opinion as a variable, and do so only when it seems to have affected outcomes, thereby providing little basis for comparing those conditions under which public opinion matters with those under which it does not.

Cohen subjects to critical scrutiny some of the metaphysics that he himself, as well as others, has accepted as theory and it evaporates before his eyes – and ours. One to which I am a devoted adherent is the "limit" or "channel" theory: "Like channel markers or gravitational fields, public opinion is presumed to hold policy makers to a clearly delineated area of acceptable decisions" (p. 17). Yet, Cohen notes, the limits are hard to pinpoint empirically. In the absence of precise definition and measurement, what these limits are depends on who is looking: "They are, in other words, not bounds or dikes at all, but clouds" (p. 18). And since these limits seem to change with the times, they are moving clouds at that.

Based primarily on his own unstructured and unsystematic interviews with fifty State Department officials in 1965–66, Cohen finds that State has no institutional mechanism for and devotes few resources to the analysis of public opinion, and that officials there see little benefit in survey research. In assessing the public mood, "it is every man for himself" at State (p. 72). Instead of formal departmental analyses and polls, officials tend to rely on their own intuition – and hence on their own beliefs and preferences, both personal and organizational – in reading the public's "mind." Cohen's findings suggest, though he does not, that the impact of public opinion on policy takes the form of an argument in the brief of one side or another in an internal policy struggle and nothing more, and that most officials prefer it that way. Instead of polls, interest group pressure, editorials, and the flow of mail, Cohen finds,

> From the perspective of the foreign policy official in Washington, the opinion landscape takes on rather different contours. The polls and editorials move into the background, along with most of the mail. Organized interests and particular groups are somewhat more common, although in narrow areas of specialization and with varying entrées to the front. In the foreground stand the Congress and the 'working press' and all the other individual 'contacts,' formal and informal, that . . . make up the private and professional worlds of the foreign policy official (p. 125).

State Department officials, in short, see foreign-policy making as an elite game in which the public is, by and large, out of sight and out of mind.

To the extent that they do think about public opinion, they take an *instrumental* view of it: will it help them to attain their policy preferences, and if not, how can they change it? Officials' perceptions of opinion, insofar as it evokes any response at all, more likely lead them to attempt to manipulate it rather than to change their policy preferences, if their recollections are an accurate reflection of their behavior.

Some of Cohen's findings may be artifacts of his method. Open-ended interviewing taps the prevalence of bureaucratic ideology over democratic norms – the dominant belief among State Department officials that "we foreign policy experts, not the people, know what's best for the nation." Interview data may well constitute evidence for the proposition that "officials at all levels except the very top generally reject any formal obligation to consider public opinion as part of the definition of their role or job" (p. 188). The data may not, however, warrant the conclusion that "while officials routinely deny any *obligation* to pay attention to public opinion, in practice they admit – sometimes readily, sometimes reluctantly – to responsive *behavior* . . ." (p. 188). How responsive their behavior is, open-ended interviews alone cannot demonstrate. Their reluctance to acknowledge even the obligation to pay attention to public opinion may explain why presidents invite their own aides to join the "experts" in foreign policy deliberations.

LEON V. SIGAL

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Citizen Participation and the Urban Policy Process. By Richard L. Cole. (Lexington, Mass.: D.C. Heath and Company, 1974. Pp. 178. $12.00.)

Politics and Poverty: Modernization and Response in Five Poor Neighborhoods. By Stanley B. Greenberg. (New York: John Wiley and Sons, Inc., 1974. Pp. 282, $12.95.)

As OEO fades away, a final wave of books on that ill-fated program has appeared. OEO's motivating idea held that the previously apathetic poor could be organized to exercise influence over the various agencies designed to serve them. It seriously believed pluralist political science's promise that any group, no matter how resource-poor, could (with the right help) influence important decisions. Of course, OEO was also born during tremendous social divisions and declining political legitimacy. Small wonder, then, that so much academic and polemical attention has been focused on the Community Action Program, citizen participation, and their allied political and rhetorical experiments. To liberals, they promised constructive reform and the re-establishment of legitimate authority. To conservatives, they portended the further undermining of established institutions. And to neighborhood militants and radicals, they seemed in the final analysis to be only a more sophisticated version of the welfare paternalism OEO promised to transcend.

Professor Richard Cole of George Washington University claims to come to grips with this experience. He applies quantitative methods in search of environments conducive to neighborhood participation and personal impacts growing out of that participation. While this is not an impossible task per se, Cole's study lacks the conceptual and methodological power to get the kinds of results he wants. In consequence, it is difficult to take his findings seriously.

Cole's strategy is simple. His findings parallel those of other multicity studies. City size and the prevalence of blacks seems to predict higher participation rates, but ethnic, socioeconomic, and regional variables do not. Reform-type city governments have less participation, but among mayor-led cities, those with weaker mayors (at least by Cole's measures) have more participation. Cole also finds a strong relationship between urban disorders and participation, and between federal poverty program expenditures and participation.

Cole's finding about individual participation also parallels well-established patterns. Active poverty program participants turn out to be relatively well off, well educated, and politically aware people interested in neighborhood improvements. People who rank high on these criteria, he finds, tend to distrust local officials, perceive conflicts within their programs, and become skeptical of the programs' success.

Such findings will not surprise the informed reader. Unfortunately, they might well mislead him, for they are constructed on a pervasive web of methodological and conceptual confusion (not all of which, by any means, is solely Cole's fault). He frequently imputes causality where none can reasonably be inferred, since his data are cross-sectional and the supposed underlying relationships are never clearly specified. He wrongly assumes that factor analysis will help him avoid all the problems of multicollinearity. He also mistakes the main points of several studies upon which he relies heavily. For example, he believes his multicity analysis has produced evidence against the Banfield-Wilson ethos theory even though Banfield and Wilson explicitly reject arguments based on the prevalence of particular ethnic groups across cities.

Cole's measures also leave a great deal to be desired conceptually. His analysis relies on reducing all dimensions of citizen participation, even the two he initially distinguishes, into one simple index. This index completely trivializes the experience of the 1960s and in all probability hides as much information as it reveals. These and other difficulties, it should be added, inexorably draw Cole into some unfortunate and ultimately unsustainable policy conclusions.

Professor Stanley Greenberg of Yale University contributes a study of five poor neighborhoods which goes a long way toward providing us with a more accurate understanding. Unlike Cole's book, it is both methodologically and historically sophisticated. Like Cole's, it relies heavily on survey methods and the analysis of attitudes and beliefs, yet the development of these tools is sensitive and elaborate. Greenberg focuses on a critical question: if poor neighborhoods are similar in demographic and historical terms, why have their political responses varied so widely? In answering it, he makes three major contributions to our understanding of the political role of the poor. First, he points out the widely varying political cultures among poor neighborhoods, neatly torpedoing any generalizations about "the poor." Second, he shows clearly that the "lower-class culture" concept characterizes only a minority of those living in poor neighborhoods and in any case bears little influence on the political propensities of the poor. Finally, he offers an extraordinarily rich attitudinal analysis of ethnic consciousness, class, alienation and receptivity toward different forms of political action. In the process, he gives us one of the very few studies of Chicano political attitudes and how they differ from those of blacks.

Greenberg, whose study was conducted largely through OEO's evaluation of the Community Action Program, analyzes five poor neighborhoods, three black (Detroit, Philadelphia, and Atlanta); one Chicano (San Jose); and one Appalachian migrant white (Hamilton, Ohio). Greenberg argues in the first part of his book that the historical roots of poor communities are quite similar and that they score the same on many demographic dimensions: they are poor, poorly educated, poorly served, and high in the incidence of crime and disease. Why then, given these similar "objective" factors, do political responses vary from neighborhood to neighborhood? Greenberg says the differences are due to the culture, or more accurately, the constellation of political attitudes and beliefs characteristic of each poor community. Further, he argues that these constellations vary significantly, not only across but within racial groupings.

He rejects "present-orientedness" ascribed by culture theorists to the lower-class. Instead, he finds that Chicano and black communities rate consistently high in terms of policy dissatisfaction and rejection of the present system (with Atlanta consistently low among the black neighborhoods), and that blacks rate high on their sense of political inefficacy. Chicanos, along with poor whites, rate relatively low on this scale. These differences in alienation, Greenberg argues, together with differences in group consciousness, account for the differing political responses in each neighborhood. Unlike Chicanos and whites, blacks tended to define themselves in opposition to white society. As a result, the great dissatisfaction, the low sense of efficacy, and the high degree of group consciousness in Detroit, according to Greenberg, make it a highly politicized and violence-prone place. Greenberg's ultimate thesis is that the formulation of an oppositionally defined group consciousness is the key to vigorous and unified political expression within poor neighborhoods.

As powerful and thoroughly elaborated as Greenberg's argument is, however, it cannot establish its main points conclusively. First, Greenberg never systematically investigates or controls for alternative explanations, particularly those having to do with the institutional or structural aspects of the communities under study. Second, he never explains where political attitudes come from, so that even if they are important, we cannot know if they only mediate some more important variable. What gave rise, for example, to Detroit's remarkable group consciousness found in Greenberg's 1969 survey?

Finally, because Greenberg relies so heavily on attitudinal survey methods, we can never be sure he has identified anything more than a consistency of mind among his informants. The link between attitude and action is hardly simple, and can never be taken for granted. Greenberg provides only a weak treatment of the actual consequences of the attitudes he describes. Further research on the nature and fate of actual movements in these neighborhoods would have been highly illuminating.

My own view differs substantially from Greenberg's. I believe that certain things he hints at, which are consistent with the patterns he presents, are critical. First is the degree of leadership and independent resource bases located within the neighborhood; second, are the specific, damaging grievances each neighborhood faces; and finally, I would point to the degree of social control, both physical and ideological, which the larger society exerts over these neighborhoods. By these criteria, one would predict that black neighborhoods would be most active, that Detroit would be the most active among them (since social control is most attenuated there), and that the Appalachian whites would trail behind.

These criticisms are not meant to belittle, but to underscore the importance of Greenberg's book for those interested in how those we label "disadvantaged" can come to influence their destiny. His work is tentative and methodologically bounded, but on the whole it is provocative and challenging. In the final analysis it makes an important contribution to a lackluster area of the discipline.

JOHN MOLLENKOPF

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Governing New York State: The Rockefeller Years. Edited by Robert H. Connery and Gerald Benjamin. (New York: The Academy of Political Science, 1974. Pp. 262. $4.75, paper.)

Governing New York State: The Rockefeller Years is a collection of 20 essays describing developments in New York State government and politics while Nelson Rockefeller was governor. The articles in the volume are organized along three major themes. The first set is intended to provide a broad overview of the political institutions and processes of the state. The articles focus upon the governorship, the state legislature, and the state's party system.

The next set of essays describes the state's relations with the federal government and the localities. These articles examine the changes and continuities in intergovernmental relations during the 15-year stewardship of Nelson Rockefeller. The last section, containing about half of the essays in the book, covers developments in various policy arenas during this period. The articles describe and analyze developments in such policy areas as welfare, health, transportation, education, and criminal justice.

In evaluating the various essays as discrete pieces, one must take into account their format. In general, they are largely descriptive pieces, with a limited analytical component. There is little concern in these essays for methodological rigor or understanding the conditions which gave rise to the developments under discussion, in part because of limited space available to cover such broad subject areas and the contemporary and controversial nature of many of the events under study which restricted the availability of hard and reliable data.

The basic question regarding this volume – whose format is by no means unique within our discipline – is its significance to the literature of political science. A study of the Rockefeller administration can be justified if it leads to increased knowledge and understanding of political behavior and political systems or if it aids in obtaining knowledge useful for understanding substantive policy problems. Since the articles were not required to meet scientific criteria, this book clearly did not intend as its goal to advance the knowledge base of the discipline.

Instead, the objective of the book is to aid in the resolution of current policy concerns. The editors note, in their preface, that New York State has traditionally "been among the first to confront emerging problems, and its solutions have often been emulated by other states" (p. ix). In helping to shape public policy, however, political scientists must not forego scientific concerns. They must direct their research efforts toward accumulating reliable data and explaining rather than merely describing political events. It is true that the achievement of these objectives is far more elusive when the object of study is current policy issues, such as those covered by this book. But there is a considerable difference between research guided by scientific considerations and studies that resemble insightful journalism.

In other words, the central problem with *Governing New York State* is the emphasis of the volume. The editors chose to sacrifice scientific criteria in order to provide a comprehensive survey of the Rockefeller era. Future students of this period in New York State politics will find this volume a useful informative introduction for the more rigorous studies one hopes will follow.

STEPHEN M. DAVID

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Congressmen in Committees. By Richard F. Fenno, Jr. (Boston, Mass.: Little, Brown and Co., 1973. Pp. xvii, 302. $7.95, cloth; $4.95, paper.)

Although Richard Fenno is quite modest in the claims he makes for the "bits of theory" which structure his studies of congressional committees, the preeminence he has come to enjoy among scholars in the field is nonetheless due in large part to the conceptual and analytical guidance he has offered. *The Power of the Purse,* Fenno's 1966 study of the appropriations committees, demonstrates the utility of the "idea of system" ("The study conceives of the House Committee . . . as a *political system* – having certain identifiable, interdependent, *internal parts,* existing in an identifiable *external environment,* and tending to *stabilize* both its internal and external relationships over time.") for developing a plausible and coherent account of committee behavior. It is thus particularly interesting to find Fenno making new conceptual departures in *Congressmen in Committees,* a comparative study which represents the culmination of the research effort first heralded by the appearance of his influential article on "The Appropriations Committee as a Political System" in 1962. Fenno acknowledges that he finds "the conceptualization of the social-systems literature less appealing" than previously, or at least less serviceable for purposes of comparative analysis than for the illumination of the "self-contained" world of the appropriations committees (p. xvii). But Fenno's book is instructive not only in the new departures it makes but also in the extent to which it retains the earlier frame of reference, for in both aspects it points up important strengths and weaknesses of the systemic approach.

When Fenno terms *Congressmen in Committees* "more individualistic" in approach than his previous work, he refers to the fact that *member goals* serve as one of the two independent variables in the analysis. The goals examined are those which members express in seeking a *particular* committee seat, and Fenno does not doubt that subsequent life on the committee has a further selective impact on its members' goal structures. But his main concern is to show how member goals and expectations *shape* the committee's life and work, which can fairly be termed a reversal of the causal relationship stressed in *The Power of the Purse*. Fenno's initial categorization of his six committees thus distinguishes those whose members are primarily interested in influence within the House (Appropriations, Ways and Means) from those fueled by a desire to make "good public policy" (Foreign Affairs, Education and Labor) or to insure re-election through constituent service (Interior, Post Office).

Fenno's second independent variable, environmental constraints, refers to the four prominent "clusters of outsiders" – the parent House, executive branch, clientele groups, and party leaders – which have "an interest in committee behavior coupled with a capacity to influence such behavior." Here Fenno reverts more completely than one might have anticipated to the conceptual patterns established in his earlier work; the focus is not so much on the opportunity structure or the incentives which confront purposive members as on the *adaptations* that are required of them as the committee encounters "constraining" forces. "Each member of each committee faces this strategic problem: how shall I proceed in the committee to achieve my personal goals, given the environmental context in which my committee operates?" (p. 46). Committees thus tend to develop *strategic premises* which represent systematic patterns of accommodation between member goals and environmental pressures. Ways and Means members, for example, seek House influence in a context where House members want the committee to be strong and independent but also responsive and where the preferences and priorities of party leaders and the executive also loom quite large. The committee thus pursues two derivative, and sometimes conflicting, strategic premises: "Write a bill that will pass the House" and "Prosecute policy partisanship." Foreign Affairs, by contrast, finds its work far less salient to the House and its environment more exclusively dominated by the executive. Here the main strategic premise has been to "help pass the annual foreign aid program," although increases in environmental conflict in the wake of Vietnam have made some members see supporting the executive as a less adequate means of implementing their desire to make "good public policy."

Having delineated his independent variables and the strategic premises that grow up around them, Fenno turns to the dependent variables of the analysis, *decision-making processes* and *decisions*. The former category includes patterns of partisanship, participation-specialization, and leadership. The linkages with the independent variables vary a good deal in their tightness; differences between Ways and Means and Appropriations in level and style of partisanship he is able to relate quite persuasively, for example, to the character of the "external demands" they face, but he is not able to account as convincingly for their contrasting patterns of centralization-decentralization and leadership. Fenno generally seems content to show that decision-making processes are *congruent* with member goals and strategic premises. This leaves open the possibility that alternative processes might "fit" equally well, though in the fates of maverick chairmen Adam Powell and Tom Murray he has convincing evidence that the constraints imposed by his independent variables can be tangible enough.

Decisions, the second of Fenno's dependent variables, are discussed in an extended and often illuminating final chapter. He frequently describes committee output in terms that permit generalization and comparison: volume, rates of floor approval, scales of partisan division, closeness to executive and/or clientele group requests. But Fenno finds the possibilities for developing "uniformly applicable and uniformly appropriate descriptive measures" quite limited (p. 192) and chooses instead to abstract, on a committee by committee basis, those decision characteristics which are particularly salient in terms of member goals and environmental constraints. Education and Labor bills, for example, increase markedly in both volume and "strength" under favorable environmental conditions (Democratic president, large Democratic majority in House); at the same time, the goals of committee members and of their clientele groups dictate that environmental conditions notwithstanding, Education and Labor decisions be such as to excite higher-than-average rates of partisan division and House disapproval.

It is the chapter on decisions which most obviously invites one to assess the payoffs and remaining limitations of Fenno's conceptual shifts. He is able to add nothing new to his earlier analysis of the decisions of the Appropriations Committee, but his focus on member goals has surely added to his capacity to understand the organizational permeability and decision-making boldness of a committee like Education and Labor. It is sometimes unclear how much explanatory power Fenno wishes to attribute to his own scheme. He does not specifically justify his shift to a more "individualistic" mode of analysis in terms of a greater capacity to explain "output," and he often reverts, in a manner reminiscent of *The Power of the Purse,* to a characterization of his analytical framework as a *descriptive* device and to a relating of his variables in terms of their functional congruence. But comments scattered through the book (compare, for example, pp. 276 and 280) and his explicit adoption of an independent variable/dependent variable scheme suggest that Fenno does expect his new conceptualizations to move us toward better *explanations,* explanations of *decisions* in particular. It thus seems appropriate to evaluate Fenno's book not only with respect to its internal logic and coherence – from that standpoint it must be regarded as a masterpiece of functional analysis – but also to ask how helpful it is in answering the questions we might have, intuitively or as a practical matter, about congressional policy making.

Had Fenno in fact *started* with a set of puzzles in the form of policy outcomes and then attempted to ferret out persuasive explanatory factors, I suspect his treatment might have differed in at least four ways: (1) He would almost surely have treated "decision-making processes" as independent as well as dependent variables. Styles of committee leadership, the way committee authority and resources are distributed via the subcommittee system, and modes of partisan accommodation would have become interesting not merely by virtue of their congruence with other elements of committee life but because of the marked and independent effect they have on a committee's propensity and capacity for law-making and oversight and, often, on the content of its decisions.

(2) Fenno also would probably have been led to look at the group and agency environments of his committees somewhat differently – not only as interested outsiders who must be adapted to but also as a field of incentives, opportunities, and constraints that shape the priorities and strategies of policy makers. For example, the level of *conflict* among environmental forces and the perceived *public salience* of a given issue are important determinants both of the incentives a congressman has to take initiatives in a particular policy area and of the character those initiatives are likely to assume once taken. In short, Fenno's introduction of individual goals as independent variables is blunted in its explanatory power by his retention of a portrayal of the *context* of individual action which stresses adaptive necessities rather than the range of options and the opportunities for more or less "profitable" involvement which confront the policy maker.

(3) The goals themselves might have emerged in a somewhat different guise. I suspect, for example, that an analysis of where a set of Ways and Means decisions came from and how they were shaped would fairly quickly reveal Committee members to be seekers of constituency and group approval and of "good public policy" as well as of House influence, however helpful the latter goal might be in explaining certain Committee norms and practices. Moreover, the goals and purposes that would prove most helpful in explaining specific initiatives and outcomes might prove less homogeneous and amenable to generalization than Fenno's threefold categorization would suggest. In fact, the results might prove rather disappointing to those who yearn for a "general" theory of legislative behavior, but surely a theory must ultimately be tested not merely by its elegance but also by its capacity to illumine the phenomena which gave rise to the theorizing in the first place.

(4) Finally, an attempt to account for concrete outcomes might lead one to focus more explicitly on the prospects and possibilities for *change.* One would gain a better sense of the sorts of decisions that are *not* made or are generally aborted on one committee or another, and of the functions (e.g. agency oversight) that are irregularly or minimally performed. And in analyzing a range of decisions and nondecisions in a variety of committee settings, one might be able to pinpoint the sorts of member goals, incentive structures, and decision-making processes that make for "positive" outcomes *and* are open to manipulation or "reform." Fenno's conceptual focus on functional "fit" and adaptive necessities tends to divert him from analysis of this sort. His discussions of reform in the book and in testimony before the House's Select Committee on Committees in 1973 mainly stress the *difficulty* of penetrating, especially with across-the-board sorts of intervention, the webs of roles and relationships that have grown up around House committees. "Don't automatically propose," he warns, "equal reforms in equal dosages for all committees" – sound advice indeed. But one must ask whether his mode of analysis – particularly of the "decision-making processes" which reformers have generally (and, I think, correctly) assumed to have a critical and independent impact on committee performance – is likely to give an adequate sense of the system's indeterminancy or to point up the

kinds of intervention and change, at *either* the "retail" or "wholesale" level (p. 290), that might move performance in desired directions.

I make such observations in a context of great admiration of and indebtedness to Fenno's work. *Congressmen in Committees* represents an important extension and refinement of his earlier studies; the intricacy and ingenuity of the analysis are often dazzling, and the volume is bound to become a benchmark for future comparative work. It is important, however, to take notice of the extent to which the book, despite some significant conceptual departures, remains grounded in the "idea of system." And it is even more important to be aware of the limitations of this mode of analysis as a means of explaining purposive political action or of apprehending the possibilities of institutional change.

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Politics of the Purse: Revenue and Finance in the Sixth Illinois Constitutional Convention. By Joyce D. Fishbane and Glen W. Fisher. (Urbana: University of Illinois Press, 1974. Pp. 199. $3.45, paper.)

In 1970 the State of Illinois rewrote its constitution, replacing the one which had been its basic law since 1870. *Politics of the Purse* deals with the formulation of the revenue and finance articles of the 1970 Constitution during the Sixth Illinois Constitutional Convention. The authors' analysis is based on interviews with members of the convention, a postconvention questionnaire, and their own participation. Fishbane and Fisher, in addition to being astute observers, were also participants in the Constitutional Convention. They were administrative assistant and staff counsel, respectively, to the convention's Committee on Revenue and Finance. Their proximity to their subject matter gives them a familiarity with the issues involved which is reflected in their in-depth treatment of each issue as it was dealt with by the committee and convention.

The finance article for the new constitution was surrounded by little controversy. The main provisions made the governor responsible for submitting the entire budget and gave him authority to reduce items within an appropriation bill. There was also a requirement for a balanced budget. The authors characterize the formulation of the finance article as rational decision making because there was general agreement on the need for the provisions, expert witnesses were listened to, and there was little controversy over acceptance of the article either in committee or by the convention.

The formulation of the revenue article provided the major source of contention. There was disagreement on whether to write a very general article, leaving discretion for tax structure with future state legislatures. Some thought it was in the nature of constitutions to provide only general guidance and avoid detailed restrictions, thus preserving flexibility. This view was not shared by certain interest groups who favored specific restrictions on state tax structure. These restrictions were the subject of most of the debate over the issues in committee and on the floor. Since the delegates did not agree about the goals of the revenue article, the decision-making process is taken by the authors to exemplify a "social choice" process, marked by conflict and a lack of consensus, as opposed to a "rational process."

The three major sources of conflict at the convention were real estate classification, income tax limitation, and personal property exemption. The process of resolving these issues at the convention, the authors argue, revealed in microcosm the politics in the state of Illinois. The main cleavage was between Cook County and "Downstate" interests. While Republicans constituted an actual majority of the delegates, they were not united and prevailed on only one major issue.

Fishbane and Fisher do a good job of reporting the taxation issues confronting Illinois and the way in which the 1970 Constitutional Convention dealt with them. They go into the background of the development of the revenue article, listing expert witnesses and associating individual members with their positions on issues. The authors' status as participant observers provides the study with the advantages of detail and depth.

Unfortunately, the book does not go beyond a very narrow view of its subject matter. The "rational" and "social choice" models are not developed, and there is little attempt to employ any other conceptual structure. Although the authors intend the study to be useful to other states in rewriting their constitutions, there is no effort to put the issues or processes into any context larger than Illinois politics. The Illinois experience in constitution-making may provide lessons for other states, but the authors do not point them out.

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Ethnic Alienation: The Italian-Americans. By Patrick J. Gallo. (Rutherford, N.J.: Fairleigh Dickinson University Press, 1974. Pp. 254. $10.00.)

In the innocent days of the early 1960s Nathan Glazer speculated that when Italian-American intellectuals became numerous enough they would mount a steady criticism of the Italian-American life style, which he characterized as increasingly middle class, suburban, and rigidly Catholic. What has happened instead is that they, like intellectuals in other religious, nationality, and racial groups, have begun to celebrate their ethnic heritage and the distinct qualities of the Italian versions of living in America today. Italian-American writers and scholars are making their own contributions to ethnic studies and the contemporary wave of ethnicity.

Patrick J. Gallo's *Ethnic Alienation: The Italian-Americans* is the work of a young political scientist whose primary scholarly interest is ethnic politics. Like Michael Parenti, whose study of ethnic and political attitudes was a pathfinder ten years ago, Gallo undertook his research as a doctoral candidate. Unlike Parenti, he has framed his study as one of "ethnic alienation." Parenti saw ethnic structures and identifications as positive forces among Italian-Americans and not simply as defensive responses to rejection by the larger society. Parenti also recognized that while gains in occupational, educational, and geographical mobility do not mean assimilation for Italian-Americans, these improvements insure that Italian-American ethnicity will not be a constant and all-encompassing bond for the group. That all Italian-Americans do not unite and share in the revival of ethnic feeling as defined by writers and scholars is the coral reef upon which the ethnic wave breaks up.

Even though promotion of ethnic studies may not reflect the preferences of great numbers of Italian-Americans, these studies ought to be encouraged because they add to general knowledge of U.S. society, whose pluralistic complexity demands the widest scope of study. Gallo's particular concern with ethnic alienation is a reflection of the specific time (1970–1971) and place (New York metropolitan area) in which he conducted and interpreted his research.

For liberal intellectuals, analysis of the Italian vote in the elections during the period 1968–1972 seemed to reveal a perfect example of "white backlash." Gallo goes beyond this interpretation and explains the voting behavior of Italian-Americans as the reaction of a politically alienated group, which sees itself excluded from the economic and political processes of the larger society. To support his interpretation, Dr. Gallo conducted in-depth interviews with forty-five individuals in Brooklyn and northern New Jersey. Fifteen were first-generation immigrants from Italy, fifteen were native-born Americans of Italian descent, and his control group consisted of fifteen white Episcopalians. As Gallo acknowledges, the limited nature of his data makes it difficult to support generalizations about an entire ethnic group. But his data are not without meaning. For example, he concluded that individuals who experienced political alienation tended to be young, blue-collar workers. Possessing a limited education, they felt powerless and excluded from the political system.

Gallo, however, also includes better-educated Italian-Americans among the politically alienated. Their negative attitude he attributes to inadequate gains in status, income, type of occupation, and place of residence. Here it is essential to have sufficient data so as to generalize properly about the group. Are Italian-Americans still the victims of discrimination, or have they begun to "make it"?

Americans of Italian descent harbor a lingering resentment over the bitter experiences of their forebears, who were the most abused and despised of the European immigrants who came to the U.S. in the first half of this century. As Alexander De Conde has observed in his excellent history of Italian-Americans (Alexander De Conde, *Half Bitter, Half Sweet: An Excursion into Italian-American History.* [New York: Charles Scribner's Sons, 1971]) the American experience was very bitter for most of the immigrants, but life became much sweeter for their children and grandchildren. After the Second World War, Italian-Americans were able to take advantage of the general economic expansion and obtained steady blue-collar jobs. Many of their children obtained white-collar jobs, and significant numbers of them established careers in the professions, government and business.

In Brooklyn and northern New Jersey, as in other urban areas, Italian-Americans since the end of World War II have been entering the political system. Gallo's respondents may not have been satisfied with their representatives, but the problem was not one of lack of representation by "one of their own." Italian-American politicians ought to be studied with care so that what is published is not limited, as it has been, to chauvinistic publicists, at one extreme, and muckrakers, at the other.

Italian-American voters, too, ought to be studied, but election analyses and survey research, not in-depth interviews of a tiny group of respondents, must be employed if broad generalizations are to be made. Inadequate data support Gallo's generalizations. In addition, his preoccupation with "white backlash" and "ethnic alienation" has a dated ring today. During the current national economic malaise, the mass of Italian-Americans cannot be presumed to be much exercised by issues other than the broad economic ones which affect all groups. Whether significant numbers of Italian-Americans were "ethnically alienated" at any time during the last thirty years is debatable and is not answered in Dr. Gallo's study.

NORMA DE CANDIDO

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The Progressive Era. Edited by Lewis L. Gould. (Syracuse, N.Y.: Syracuse University Press, 1974. Pp. 238. $9.95, cloth; $4.95, paper.)

This collection of eight essays originated through an invitation by the editor several years ago to a group of "younger historians" to take a "fresh look" at the significant problems of the Progressive Era.The results are in some ways a celebration of the politics of that period: nearly all of the essays comment positively on the degree to which specific reformers and reforms served effectively to correct real problems. This perspective may well reflect a changing climate of opinion among younger historians. But such a change, apparently, does not necessarily shed new light on "Progressivism" or its "era"; unfortunately, these essays do not tell us much that is new about either. In fact, it might even be argued that *The Progressive Era* helps document the contention of some that the groups, reforms, and concerns of "Progressivism" are too varied, disparate, and conflicting to justify lumping them together under one rubric.

The problem is partly one of definition, but it is not simply that. Political change is often symptomatic of a much more pervasive social transformation, and one mode of political response to such a transformation is hardly an adequate handle for it. What we call the Progressive era had retrogressive facets this book scarcely mentions. While the problem of defining a historical period is acknowledged, little headway is made here on the problem. The book also suffers from the editorial tendency to select materials and subjects that are themselves unified and to ignore those that are not, thus lending an air of coherence to history it does not in fact possess.

These caveats notwithstanding, *The Progressive Era* is still a useful book. The essays are coherent and lucid and based upon recent research; they are carefully edited and, despite some inevitable repetition, internally consistent. Louis Gould has written a trenchant, helpful introduction, and Thomas K. McCraw summarizes the collection neatly in an essay entitled "The Progressive Legacy." Between the two are treatments of Progressivism's origins, intellectual tendencies, and party politics, and three essays addressed to conservation, urban reform, and diplomacy. The best of these are James Penick's regrettably brief analysis of three themes from the first conservation movement and Melvin Holli's study of urban reform, taxes, and administrative costs. Holli's essay contains the portentous observation that urban government cost bore a direct – not inverse, as one might expect – relationship to the size of the governmental unit.

The Progressives rode off in many directions and tilted at many windmills. Part of this diversity arose because they were responding as individuals and groups to a pervasive, accelerating cultural dislocation that had many aspects and consequences. Not all Progressives realized the vast interrelated dimensions of the problems they were dealing with; had they, their confidence that social problems could be solved piecemeal by legislative enactment might not have been so firm. Considering the horrendous cultural schism of the 1970s against the background of Progressivism, as McCraw invites us to do, inevitably forces us to measure the effectiveness of their reforms in more modern comparative terms. That the Progressives were energetic, intelligent, and moral is undeniable, but whether their collective reforms added up to an adequate response to the challenges of urbanism, industrialism, modern capitalism, and democracy is debatable.

The contributors to *The Progressive Era* agree that the Progressives were by no means radical and ask that they be judged by what they did indeed accomplish. The total of the Progressives' legislative efforts from 1900 to 1917 certainly appears impressive on the statute books, but how fundamentally America was changed by such legislation is unclear. The contours of the 1920s and 1930s suggest to many that the Progressive reforms were inchoate and directed at symptoms rather than causes. A more charitable view of the Progressive reformers may well be in order but to those still concerned with the same problems in the

1970s, Progressivism can only be regarded as a halting and fundamentally conservative beginning.

DAVID W. MARCELL

Skidmore College

A History of American City Government, 2 vols. By Ernest S. Griffith. (New York: Praeger Publishers, 1974. Vol. I: *The Conspicuous Failure, 1870–1900.* Pp. 308. $10.00; Vol. II: *The Progressive Years and Their Aftermath, 1900–1920.* Pp. 352. $10.00.)

In 1938 Ernest Griffith's *History of American City Government: The Colonial Years* was published as the first volume of a projected series covering the entire history of American city government. Now, after a delay of thirty-five years, the remainder of the series is appearing. The two volumes under consideration here treat the period from 1870 to 1920; a fourth volume, done in collaboration with Charles Adrian will cover the years 1775 to 1870.

I remember, as a graduate student writing a paper on urban politics, appreciating the utility of that early work. It was a disappointment to find the remainder of the series unpublished. The present volumes come too late to help my nearly forgotten paper, but they are nonetheless timely, for they aim at filling what has remained a significant gap in the literature. Students of urban politics know that specialized studies are widely available: local histories abound, scholarly treatments of particular policy questions are plentiful, there is an awesome amount of literature by and for administrators, and the perceived urban crisis of the '60s precipitated an avalanche of research and comment. But there is still a need for an older style of scholarship, for the wide-ranging treatise by a mature scholar who can competently cross interdisciplinary boundaries, sum up the state of knowledge, and provide a foundation for criticism of the dominant ideas in the field. The presentation of such an authoritative treatise is evidently the goal Professor Griffith set for himself in this effort to "do justice" to the years included in these companion volumes as part of a "complete history of American city government" (Vol. I, p. ix).

The range of the author's concerns are now broader than they were in his book on colonial city government; that earlier work had confined itself to charters, governmental structure, functions, finance, and the legal definition of the electorate. The present volumes treat these matters but go on to include material on the political status of ethnic minorities, the relation of municipal finance to national economic concerns, the role of religious values in political activity, and, most emphatically, the tension between corruption and reform. In the course of this, Griffith makes available a wealth of fascinating information. The tendency for reform politicians to come from the legal profession (Vol. II, p. 22), the wide differences in electoral participation from city to city (Vol. I, pp. 128–9), and the role of at least one publisher in corrupting school boards (Vol. II, p. 223) are examples of what the reader will find. Civil libertarians may find today's "law and order" advocates less distasteful after reading that in 1884 a Cincinnati mob, outraged by the acquittal of accused criminals, burned the courthouse, took over the city, and were stopped finally by federal troops at a cost of over fifty lives (Vol. I, p. 89). Apologists for the good old days will find it difficult to wax nostalgic about yellow fever epidemics and citywide outdoor toilets (Vol. II, p. 192).

But, informative as they are, the books are regrettably flawed. Some of the flaws are minor. In the introduction to the first volume the author generously offers personally to make his bibliography available to interested scholars, but it is unfortunate it could not have been included in the books. Footnoting is relatively skimpy, and the footnotes themselves are placed at the back of each volume. A final annoyance is the lack of clarity in presenting quantitative data; more attention to the tables (e.g., Vol. I, p. 87) would have made them more helpful.

The serious flaw of these volumes is the author's failure to support convincingly his central argument – that the thought of the traditional urban reform movement still provides an adequate foundation for understanding urban politics. The very titles suggest what the text of each repeatedly underscores; Griffith sees the struggle of the reformers against the bosses as the battle of good against evil. The bosses were essentially instruments of corruption; the reformers were embodiments of civic virtue. But to the now-fashionable charge that many of the Progressive reforms were efforts by middle-class WASPs to control the masses and minorities through closing the channels of participation, the author responds only that the reformers were sincere, that they had good intentions, and that what they did was in accord with their religious values (Vol. I, pp. 95, 253–255; Vol. II, pp. 145, 152). Surely this misses the point. What the critics of reform have argued is precisely that the reformers imposed a rather restricted set of values on people who had good reason not to share them. To be sure, the author acknowledges a distinction between "those who were motivated by their Puritan, Calvinistic Protestant heritage with its ethic of honor and duty and the predominantly Catholic immigrants for whom personal relations were more important than abstractions" (Vol. II, p. 23). But this sort of analysis, coupled with jibes at "boodlers" and "saloon-keepers" will do little to disarm the critics of reform.

The basic problem is how Griffith misuses values to justify the reform position. If the sole test of a given set of values is, as he suggests, whether they are sincerely held, then the old strictures about the need for a "value-free" political science were right: values are simply a matter of personal preference and should be excluded from political science; what counts is observable behavior. But the arguments for the reintroduction of the study of value into political science are most compelling where they assert that this is not the case at all. Preferences and values should be included to extend the reach of political science and to bring under examination the interaction of political life and the individual's sense of right and wrong. Those who want to use the new interest in the discussion of value to shield a given list of policy preferences would do well to remember that the two great sources of norms for our culture have never been very hospitable to such efforts. Socrates, asking questions even to the detriment of his own political position, must remain a troubling figure to those who would call on the philosophical tradition to support their program. Partisans will find the religious tradition of Christianity an equally unstable support. After all, St. Paul, despite his authoritarian tendencies, felt it necessary to confess "the good that I would, I do not; but the evil which I would not, that I do." The revivification of the urban reform movement will require a much more penetrating effort than Griffith has made here.

Nevertheless, these volumes will be welcome additions to college library collections and the personal collections of urban politics specialists.

JOSEPH F. FREEMAN

Lynchburg College

From Poverty to Dignity: A Strategy for Poor Americans. By Charles Hampden-Turner. (Graden City, N.Y.: Anchor Press/Doubleday, 1974. Pp. 300. $8.95.)

In this, his second book on the problems of American politics and society, English social critic Charles Hampden-Turner attempts to construct the theoretical basis for black capitalism – the Community Development Corporation. He begins by recognizing that Black America is caught between the American Creed and Marxism (p. 36) and by recognizing that only blacks can find their way off the treadmill of marching but never arriving (quoting Harold Cruse). Nevertheless he proceeds, perhaps on the faith that a white man can help so long as the white man is not an American white man.

Mr. Hampden-Turner's theory consists largely of ten "principles of human development" for the empowerment of the poor. These principles are so fundamental that the main concepts do not require definition. Each proposition is true in itself, and each relates progressively or "synergistically" to each of the other propositions, as separate fingers on a hand. When the author applies them to a specific situation he tends to put the key concepts in capital letters, perhaps to enhance their value or provide a greater sense of relevance. For example, proposition I is as follows: "*The principle of* FREE EXISTENCE . . . *the poor denied the right to originate* . . ." (p. 2, emphasis in original. Excision marks also in original.) No "proposition" is stated as a complete sentence with subject and verb. Each is an expression of the absolute value position of the author, but is posed as a universal principle of value, therefore, by logical extension, the value position of ghetto blacks.

The author first applies his propositions to an empirical treatment of the consequences of racism among the black poor, particularly those who live in racially segregated ghettos. He offers a number of very relevant and very touching insights into the ways poor blacks react to various kinds of gestures made by outgroups. But nowhere is it very clear that he needed his ten propositions and the elaborate incantations of the first chapter in order to come up with his excellent insights.

He then launches into a very useful attack on some of the shortcomings of social science approaches to the poor. Some of the passages in Chapter 3, "Social Science Against the Poor," are well worth the attention of any serious student of social science and social problems. Many of the observations would serve well as an antidote to the ambitions and the confidence of social scientists who have an urge to make public policy. For example, he analyzes how the War on Poverty wrested "from the Civil Rights movement its initiative and momentum" on the basis of social science theories about matriarchy in the black community (p. 84). For example, he tries to deal with the problem of how social science can lead policy makers toward the practice of deducing specific situations from general rules without searching for any sensory experience. Here his general observations and the several illustrations drawn from the poverty field cannot be dismissed lightly (p. 80). He also suggests with considerable force that a great deal of the fallacious public policy thinking comes from the central tenet of social science, the principle of "considering social facts as things" (quoting Durkheim on p. 74). It is quite possible, implies the author, that social science categories may contain more effective prejudice than the perspectives of the most bigoted (p. 84).

Alas, the author should have drunk more of this brew himself. He is ultimately unable to escape many of the same dilemmas faced by any social scientist who involves himself in solving the problems of others. Throughout the remainder of the book he proves himself unable to escape the position of moral superiority inherent in any analysis that attributes feelings and experiences to a group of citizens and then proceeds to define their goals and their means of attaining them. How does this differ from the flaw he observed in other social scientists of deducing specific situations from general principles? For him as well as for the others, these actions combine armchair empiricism with old-fashioned patronizing.

The author was also unable to escape another problem inherent in any attempt to build social reforms, and he is even at greater fault here because he failed to recognize that it was a problem at all. This problem is *the state* and his attempt to build social reforms without taking it into account. There is an important lesson here for any social scientist, and it bears further exploration: A social scientist takes what he believes to be the values of a group; he develops a theory showing why these ought to be the goals of that group; he then translates those goals into social programs, or rather, he hypothesizes that the relevant reforms will come to pass or that their not coming to pass is then evidence of bad will or a sick society.

The state is left out of the process entirely, and in its place is the author, *in loco parentis.* The author's theory of "empowerment of the poor" culminates at almost exactly the halfway point of the book with the proposition that "the type of institutions and social structures which are best suited to ghetto and rural development are Community Development Corporations. . ." (p. 110; the substantive part of the book ends on p. 223). He then links this proposition to the original theoretical base with some empirical statements that range from completely untrue through half-true to correct but only by virtue of an ambiguous use of terms. The following are representative examples: ". . . historically *no* poor migrant groups in any country, at any time, have climbed out of poverty on therapeutic ladders proffered by an elite" (p. 105). Only "an organization of victims could conceivably command the necessary effort and sacrifice . . . to cut the giant leeches off the face and body of their community. . ." (p. 110); and "Urban ethnic groups in America have emerged from poverty, *as groups,* by creating and manning their own institutions" (p. 105, all emphases in original). Note particularly how the author uses "group" as a classification of persons and then uses it to mean an organized and self-conscious collectivity.

This is how the downtrodden bourgeoisie got its property rights: by identifying all those actions against person and contract that required intervention by an agency of the state. This is how blacks will ultimately succeed, and ghetto capitalism *may* be one of the outcomes. But taken as the goal itself, it will probably never happen, or, worse, could be imposed upon many who don't want to be black capitalists.

Having attempted to build his theory and his reforms on the black community itself, this excellent English thinker has simply joined the in-group: one more white man indulging in an old privilege of patronizing the nonwhite.

THEODORE J. LOWI

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Nonpartisan Elections and the Case for Party Politics. By Willis D. Hawley. (New York: John Wiley & Sons, 1973. Pp. 202. $9.95.)

As its title hints, this is one of that new breed of scholarly book that is at the same time both empirical analysis and reformist appeal. Given the strength of both empiricism and reformism in the traditions of American political science, one can hardly object. But one is left, in effect, with two "books" to review.

The main body of the book (after some cursory history) is empirical monograph. Drawing on data from 88 cities in the San Francisco Bay Area, Professor Hawley sets out to find what, if any, biases result from nonpartisan elections and to see if those biases have any policy consequences. It is, as the author points out, an attempt to see if nonpartisan elections influence who governs and then what difference it makes who governs. He confirms the conventional wisdom that nonpartisan elections work to the advantage of Republicans but that the magnitude of the bias is not as great as many of the conventionally wise would suggest. But in the larger cities the bias is clearer, and it increases with the size of the city and with increases in proportion of lower SES citizens. Furthermore, by showing that the issue orientations of Democrats differ from those of Republicans, and to what extent, Hawley infers the consequences for policy making of the bias resulting from nonpartisanship. "Republican nonpartisans are the least likely to see urgency in the need to deal with social concerns," he writes. "Democratic partisans are the most likely to focus on the solution to social as compared to other problems" (p. 118).

Inevitably, the book suffers some of the usual limitations of the monographic genre. Its data base is limited to one part of one state, and its exposition is sometimes slowed by explication of the procedures, assumptions, and logic undergirding it. But it also has a number of virtues by no means common to the genre. There is always a proper caution in interpreting data and in constructing arguments. There is also intellectual sophistication and familiarity with a wide range of ideas and literature within political science. In addition, there are extensive and useful footnotes. All in all, it is a thoughtful and incisive addition to the literature on nonpartisan politics and a necessary point of departure for future scholarship.

The second "book" within the book – the reformist appeal – emerges in the longish final chapter. In it Hawley makes the case for party politics which the title promises. To the question of how best to organize mass political power in the cities, he responds with an unabashed preference for something very close to classic "party government." The argument, in other words, goes far beyond the substitution of partisan for nonpartisan elections. Hawley prescribes increases in party activity, the open selection of party leaders, public financing of the parties, as well as broad reforms in elections themselves. The reform proposal is thus only partly related to the analysis of nonpartisan elections, and it evokes a great many of the controversies that have long swirled about proposals for party government. Given today's cynicism about parties and the declining incidence of party identifications, the proposal seems in fact to come from an earlier and more innocent time.

Nonetheless, the chief issue remains the defensibility of nonpartisanship in American local elections. Hawley's case against it is well argued, but not completely persuasive. For example, I doubt that the restoration of party elections or even party government would insure elections contested on party issues or policies dictated by the issue commitments of the winning party. In addition to all of the usual difficulties in instituting party government, one faces the fact that the major parties' issue commitments are largely to national issues. To put the point differently, it is not only nonpartisan elections that blur or confuse the issues in local politics. Different configurations of interests, lower levels of political information, and fragmented governmental authority, *inter alia*, all contribute. Moreover, are nonpartisan elections to be judged only by their inability to represent accurately the social interests and values in the city? Is it not possible that they facilitate the recruitment of political leadership? Or that they increase the acceptability and legitimacy of local government? I really don't know, but the questions seem to me worth asking.

In conclusion let me raise an issue which Hawley's study suggests only by example. Does the distinction between partisan and nonpartisan elections – resting as it does on the presence of a word or two on the ballot – really serve us well? Many nonpartisan elections are in truth more partisan, involve more party activity, than many partisan elections. Furthermore, our nonpartisan elections are that only in a partisan political context, that is in a context in which partisan cues and loyalties (both of candidate and of voter) are never very far away. Perhaps it would be well to place our elections along a single continuum in which party cues, activity, and visibility are present in one degree or another. Perhaps, simply, it would be well to remind ourselves that in reality we do not have any strictly nonpartisan elections.

FRANK J. SORAUF

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Bringing Home the War: The American Soldier in Vietnam and After. By John Helmer. (New York: The Free Press, 1974. Pp. 346. $12.95.)

The blurb on the cover introducing John Helmer's *Bringing the War Home* cites the book as the "first complete analysis of American men at war since the landmark *American Soldier* series." The information in this "enormously important book," the blurb continues, "radically alters traditional views of the combat experience and forces a total re-evaluation of conventional knowledge about the American soldier."

It is difficult to come down to earth after that type of introduction. Were it true, nothing much need be added. The book would be required reading for social scientists of all persuasions, and the debate might center on how the new research expands our substantive and methodological frontiers. In actuality, the book cannot be considered within such a context. Perhaps the fairest approach is to begin afresh by describing exactly what the book is and what it attempts to do. Any comparisons with *The American Soldier* are best not introduced into the discussion.

Helmer's book is a carefully drawn, limited study of a small number of veterans in the Boston area who returned from the Vietnam War. In style and approach it is similar to the many articles on aspects of the problems treated in sociological journals. It retains a 1960ish antiwar flavor, and the culture of the 'sixties and early 'seventies (demonstrations, frustration, drugs, etc.) colors the presentation in the nature of the concerns presented, the uneven support for the war at home and the veterans' depiction of their plight. Given the previous American experience with war, Vietnam was a most unusual occurrence. These problems are reflected in the presentation.

The data gathering centers on personal interviews with ninety veterans, thirty chosen from each of three groupings: the Veterans of Foreign Wars (the "straights"); opiate addicts (the "addicts"); and the Vietnam Veterans Against the War (the "radicals"). These ninety are then examined extensively and compared on such dimensions as drug use, policy positions, political background and views, parents' attitudes (as perceived by respondents), motivations for enlisting, readjustment to civilian life, etc. Any appreciation of the book and its contributions must begin with an acceptance of the obvious limitations built in by the circumscribed research design.

The book is carefully written and fully documented. In fact, given the thoroughness of the author's review of previous research and of his sifting through the particles of data, it is unfortunate that he did not have a full-scale national survey with which to work. The main emphasis is on working-class attitudes (among the sample) toward the war and the effect of the war and readjustment on this particular group.

As to findings, obviously the attitudes of young men who fought in Vietnam during the 'sixties and early 'seventies is sharply different from those of the 1940s. The author does a nice job of illustrating these differences and of contrasting them with the full range of previous studies. Within this context, the comparisons with the conclusions of *The American Soldier* make clear that a different generation fighting a quite different war varied significantly from its predecessors. The earlier explanations about group solidarity and personal loyalties of the World War II era need redefinition within a military culture more sensitive to the alienation and the anti-war feelings abroad in the society at large.

The author has conscientiously executed his work. He has cast his questions and his reflections in broad theoretical and social terms. The conclusions of the book, as with the war itself and the ambience that surrounded it, are not heartening. The inequities of a war that took its undue toll of the working class – psychologically and socially, as well as in mortality rates – continues: "For working-class Americans the Vietnam War, together with the economic recession that it indirectly caused and that accompanied its later years, was the most serious crisis since the Depression of the Thirties" (p. 298). Historically, the working class is not disposed towards effective political organization. The group's inability to equalize the war's burdens and its continuing economic sacrifices in the war's aftermath testify to how costly in stark personal terms its political ineffectualness has been.

WILLIAM J. CROTTY

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Redistribution Through Public Choice. Edited by Harold Hochman and George Peterson. (New York: Columbia University Press, 1974. Pp. 341. $12.50.)

This book is a baker's dozen of good articles on the economics of redistribution policies. These articles were first presented at an Urban Institute conference in 1972. Careful selection of authors and topics and revision of the articles to reflect discussant comments has made this an unusually good collection of conference papers. The dominant perspective is "centrist": government has an important redistributive role but often performs it inefficiently. There is no clarion call for reform; there is no presentation of a radical Marxist, conservative, or libertarian perspective. One gains the impression that these careful scholars are nice people who are somewhat bored by the topic. The dominant effect of these articles is to question whether any change from present policies is clearly desirable.

An introduction by the editors provides an efficient summary of both the other papers and their own perspective. William Breit summarizes the development of the history of economic thought on redistribution and the contemporary attempts to rescue theory from the agnosticism of Pareto and Robbins on this issue.

James Buchanan addresses redistribution in a federal system and concludes that "Centralization tends to widen the set of possible distributional outcomes and, simultaneously, to reduce the pressures for efficiency in public-good provision" (p. 31). David Bradford and Wallace Oates address the hypothesis of suburban exploitation of the central city and find it wanting; a careful empirical analysis of the New Jersey metropolitan area indicates that the formation of a metropolitan government with uniform taxes and per capita expenditures would substantially reduce the efficiency of the supply of government services, redistribute the tax burden among landlords, and somewhat redistribute expenditures from rich to poor – and have no significant effect on the distribution between suburbs and the central city.

William Baumol concludes that environmental measures redistribute income toward the rich, are no less desirable for this reason, but should be paired with other measures to offset these distributive effects. George Peterson analyzes the effects of performance contracting in schools; an empirical study of the Banneker experiment indicates that the expected effects were observed and that the perceived failure of this experiment was due to a poorly formulated contract.

Martin Feldstein proposes a formal method to weigh the distributional preferences of politicians in benefit-cost analysis; from my perspective, this approach begs the important questions and is not very useful. James Rodgers summarizes the three major contemporary economic models of redistribution by government:

(1) The "Robin Hood" model.
Transfers are made by an effective coalition of recipients to increase their income.

(2) The Insurance model.
Transfers are made by an effective coalition of donors to provide insurance against specific contingencies or life-time income averaging that is more costly to provide through the market.

(3) The Public Goods model.
Transfers are made by an effective coalition of donors because they are either concerned about the welfare of recipients or about community effects (crime, riots, blight, epidemics, etc.) that might be alleviated by the transfers.

Rodgers concludes that the present level of transfers is probably best explained by the Robin Hood model but that the form of many transfers is best explained by the Public Goods model. Richard Zeckhauser further develops the Insurance model and the reasons why the market does not provide some forms of desirable insurance. Mitchell Polinsky further develops the life-time income averaging form of the Insurance model and the implications for the tax structure.

Otto Davis and John Jackson analyze the Senate votes on the Family Assistance Plan; they conclude that these votes were a function of constituency interests but that the nay vote by most southern senators must be explained by other conditions. Julius Margolis presents an intriguing sketch of a general model of democratic government and uses it to analyze the behavior of urban governments. Harold Hochman concludes with a thoughtful essay on transitional equity, addressing the important but still unresolved questions concerning the conditions for which losers should be compensated when policies are changed.

Most contemporary discussion about equity, including that in this book, confuses two meanings of the word. Equity is often used to describe an equality of conditions, such as income or expenditures. Equity is also used, however, to describe a fair or just process. And the implications of these two meanings are enormously different. Private crime (probably) equalizes income but is not widely regarded as just. Are coercive transfers by the government in any way different? Only the first meaning gives any content to the textbook trade-off between equity and efficiency; in the latter sense, no change can be efficient unless it is just. The fundamental political conflict of our time is between those who would use any process to achieve more equal conditions and those who would accept any outcomes of a just process. In a literal society, equity is synonomous with transitional equity. Hochman's final essay sets the stage for a new and more profound development of our perspectives on this issue.

WILLIAM NISKANEN

Ford Motor Company

The Schools, the Courts, and the Public Interest. By John C. Hogan. (Lexington, Mass.: D.C. Heath, 1974. Pp. xvi, 262. $15.00.)

The author of this learned book is an administrator for the RAND Corporation, and Adjunct Professor of Education and Administration of Justice at California Lutheran College. Following a brief introductory chapter on education and the public interest, and the concept of educational opportunity, a second short chapter discusses generally the evolving view of the role of the courts in education, namely, that decisional law tends to regard education as largely under the supervision of the courts. While even the most casual student of American public law senses that judges in our times are much more willing than ever to get involved with educational problems – even student grading in some instances – on the whole the author seems to believe that they have not been too involved, and that the litigated cases, while numerous, cover only a small part of the whole subject.

The lengthy third chapter of this book reviews the court decisions dealing with race and education. The general development of the law since *Brown* is spelled out in considerable detail, and specific problem areas (e.g., Richmond, Detroit, Atlanta, Denver) are reviewed in depth. The general conclusions are: (1) that state laws requiring separate schools for blacks and whites violate the equal protection clause of the Fourteenth Amendment; and (2) that policy decisions based on considerations of the race of students or teachers which contribute to increasing racial segregation in the public schools are also in violation of this guaranty. Wherever race is a consideration, the "strict scrutiny" test is applied by the courts, which means that the state carries the burden of proving the existence of a "compelling state interest" to pass muster in the courts.

The next chapter deals with wealth and education, placing special emphasis on the California Supreme Court's decision in *Serrano* v. *Priest* (1971) and its progeny in other states, and the decision of the Supreme Court in the *San Antonio School District* v. *Rodriguez* (1973), which denied federal relief for state financing of school districts on an unequal basis. The problem of equal financing of public schools has been considered by only about a dozen states, and it cannot be said that the courts have found a solution. A few courts seem to know what is forbidden, but what must be done positively seems to be beyond the resources of courts to handle.

The next chapter deals with various liberties and rights of students and teachers, including freedom of expression, hair-styles and grooming, campus discipline, student pregnancy, corporal punishment, admission and graduation requirements, suspension, expulsion, pupil placement, teacher employment and promotion, and the use of tests. Many court decisions have been recorded on these subjects; indeed, it is perfectly amazing to observe how judges regard themselves as more expert than those who administer the school systems of the country.

A brief final chapter seeks to state some general concluding propositions. While the courts have not yet identified the right to demand a public school education as a specific federal constitutional right, federal courts have quite firmly recognized the right to equal educational opportunity. In addition, at least since 1960, federal judges have adopted the practice of retaining jurisdiction over decided cases, so that they are in a position to supervise how school officials carry out court orders. Furthermore, it is clear that educational classifications based on race or wealth are inherently "suspect," and thus subject to the "strict scrutiny" test, in place of the former "reasonableness" test, which means that the burden of proof is on the state to demonstrate a "compelling state purpose." Beyond this, American courts are in disagreement on many subjects. For example, courts are divided on whether the hair length of students is a justiciable issue, and cases going both ways have been denied review by the Supreme Court. Apparently, the Supreme Court Justices think that they have more important questions to work on than the issue of hair length.

This book covers a great deal of ground in summarizing the case law now available on the treatment of a wide variety of educational issues by the courts. It seems clear from this analysis that the law in this area is in a developing and transitional state, and that generalizations must be stated with great caution. In fact, on a number of important educational issues Supreme Court decisions have been handed down since this book was written. It is, of course, the fate of books in the public law field that they are out of date by the time they are finally published. In addition, while the author cites a great many cases properly, he often fails to identify which federal district court or federal court of appeals he is talking about. Furthermore, the author relies very heavily on California material, particularly at the administrative and legislative levels; it is not clear how generalizable the experience of one state is, even a very important and progressive one like California. Finally, the author notes that courts would be less concerned with school management and operation if the legislatures were to do their job properly, to which one may say, quite fervently, "Amen!"

DAVID FELLMAN

University of Wisconsin

Commentaries on the Constitution of Virginia. 2 Volumes. By A. E. Dick Howard. (Charlottesville: The University Press of Virginia, 1974. Vol. I: Pp. xxviii, 1–558; Vol. II: Pp. xvi, 559–1208. $25.00, set.)

The last decade has witnessed more rewriting of American state constitutions than any comparable period since the Reconstruction era. To the body of literature growing out of this activity the two-volume work by Professor A. E. Dick Howard of the University of Virginia School of Law is a unique and valuable addition. Although focused on the 1971 Constitution of Virginia, sixth in the constitutional development of the Commonwealth, these volumes trace the evolution of American constitutionalism from its English and colonial antecedents to 1974. Excepting the federal Constitution, the organic law of no state better embodies the main currents and themes of American constitutional development than the Virginia document. As Professor Paul A. Freund of the Harvard Law School states in the Foreword to Howard's *Commentaries*, ". . . the Virginia experience has served as an important model and in itself has reflected the play of forces that have been at work in the nation at large" (p. vii).

An Introduction to the text of the *Commentaries* provides a general overview of the salient events and main currents of constitutional development from the original 1776 document through the four constitutional conventions that prepared the succeeding documents and finally gives more attention to the framing of the 1971 Constitution. An 11-member blue-ribbon Commission on Constitutional Revision, including two former governors and chaired by one, Albertis S. Harrison, Jr., prepared the initial draft in 1968. The Virginia General Assembly approved the proposed document with some modifications in two successive sessions and submitted it to the voters in the form of four questions, which were adopted in 1970 by a two-to-one margin. The main body of the text follows in 12 chapters, each devoted to an article of the 1971 Constitution and containing a section-by-section commentary.

Typically, the commentary on each section traces the evolution of each provision from its origins, and records related historical events and the legislative history of the 1969–70 revisions, including both the Harrison Commission's proposals and actions by the General Assembly. Attention is given also to present meaning and operation. Although the commentator cautions the reader that the work is neither an annotation nor exhaustive, the exposition is thorough and the documentation extensive, including a rich store of citations from cases, laws, and other pertinent literature. The author draws liberally on judicial opinions, opinions of attorneys general, reports of study commissions, views of commentators, and comparative data on other states. Throughout the exposition, the themes and topics of constitutional development in the Commonwealth are intermeshed with, and related to, the main currents in federal constitutional law. Although most substantive work was completed by January 1973, the material was updated selectively to the spring of 1974.

Born in Virginia and educated in Virginia and at Oxford, Professor Howard served for two years as clerk with Justice Hugo L. Black before his appointment to the law faculty at the University of Virginia. He was appointed executive director of the Harrison Commission, served as counsel to the General Assembly during the constitutional revision sessions, and as director of the successful ratification campaign in 1970. Active involvement at each stage of the development of the 1971 Constitution was ideal preparation for authorship of the *Commentaries*. Obviously, in their writing, he made full use of the extensive memoranda on 140 subjects prepared under his direction by the staff of the Commission on Constitutional Revision.

Howard's experience as a Rhodes Scholar at Oxford and his interest in, and authorship of, works on the English origins of American constitutionalism (especially *The Road from Runnymede: Magna Carta and Constitutionalism in America* [Charlottesville, Va.: The University Press of Virginia, 1968]) may account in part for the exceptionally heavy emphasis on Article I, Bill of Rights, in Volume I. The commentary on Article I covers more than half the entire text in Volume I and is twice that on any other article. The importance of the Virginia Bill of Rights, however, as the principal source of the federal Bill of Rights and its influence on other state documents fully justifies the attention devoted to that part of the Constitution which Justice Black once described as "that remarkable collection of 'Thou shalt nots' " (quoted on pp. 50–51).

Professor Howard points out that in developing its recommendations a guiding principle of the Commission on Constitutional Revision was to maintain a balance between tradition and change, thus preserving the heritage of the past while proposing appropriate revisions designed to render the Constitution more responsive to future needs. On balance, in Virginia, tradition appears likely to weigh somewhat more heavily than innovation.

Of the main themes and currents of constitutional development in Virginia, one of the most significant is the key role of the General Assembly in the governmental structure. Although all three branches of government have been strengthened, the legislature retains more than usual power. Besides the normal functions of state law-making bodies, the Virginia General Assembly selects all justices and judges of courts of record, members of the State Corporation Commission and may establish its procedural rules, has final authority over the "standards of educational quality" prescribed by the Board of Education, and may override rules of practice and procedure promulgated by the Virginia Supreme Court. The Assembly's protectiveness of its powers was manifest during the 1969–70 revision sessions. Illustrative were rejection of executive initiative in administrative reorganization and willingness to accept only a watered-down approach to local government problems. Especially noteworthy are the provisions of Article XII which vest in the law-making body the sole power to initiate the process of constitutional change.

The 1971 Constitution is a substantial improvement over its predecessor. Documentary gains included enhanced clarity, better organization, and excision of much excessive verbiage, reducing the length from 35,000 to 18,000 words. Major substantive innovations were elevation of education to the status of a fundamental right and a mandate to establish "an educational program of high quality," an antidiscrimination guarantee, a uniform system of permanent registration, annual legislative sessions, a unified judicial system, provision for executive succession and for handling gubernatorial disability, liberalization of debt limitations on the Commonwealth, and inclusion of a new article (XI) giving constitutional recognition to the policy of the Commonwealth "to conserve, develop, and utilize its natural resources, its public lands, and its historical sites and buildings."

Despite these and other gains, weaknesses remain, both of retention and omission. Illustrative of the former are continued use of technical and legalistic language in some sections, the ban against two successive gubernatorial terms, and retention of the former local government debt ceiling. Omitted are provisions often included in revised constitutions, such as automatic reapportionment periodically by a nonlegislative agent, joint election of the governor and lieutenant governor, authority for intergovernmental administrative arrangements, and a periodic referendum on the question of calling a constitutional convention.

In appraising the 1971 Constitution, a key consideration is that the voters approved it. This success probably reflects the political and legal experience and sagacity of both the Harrison Commission and the General Assembly.

Professor Howard's *Commentaries* will be a valuable reference source to non-Virginians as well as Virginians, to both the practitioner in government and the scholar, to journalists and others interested in public affairs, and especially to state constitution makers. Professor Howard has made a distinguished contribution to the literature on American constitutionalism. In the tradition of the founding fathers of the Commonwealth, his work sets a high standard worthy of emulation in other states.

ALBERT L. STURM

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Jews and American Politics. By Stephen D. Isaacs. (New York: Doubleday, 1974. Pp 302. $8.95.)

Jews and American Politics is an attempt by a journalist working for the *Washington Post* to describe and explain the very substantial role which has come to be played by Americans of Jewish background in the political life of the nation during the past thirty years. Lawrence Fuchs's *The Political Behavior of American Jews* dealt with similar themes back in 1956, but was primarily concerned with electoral behavior and attitudes. Isaacs, on the other hand, deals extensively with the Jewish role among political elites. The book is primarily a journalistic effort, and hence relatively unsystematic, but it does contain a good deal of information of value to political scientists.

As Isaacs points out, the political influence of Americans of Jewish background (with some outstanding exceptions) is mostly at one remove from the bastions of power. Jews vote in larger numbers than their compatriots of other religious persuasions in certain key states where they are concentrated; they have donated very substantial funds to the Democratic party, and they are excellent organizers, fund raisers, and speech writers. In most of these roles they are primarily concentrated in the liberal or "new politics" wings of the Democratic party, although they have played some role among the Republicans. Jews have also held important positions in a few "activist" federal agencies such as H.E.W., and, although not represented in large numbers, have done key work on the staffs of some Congressmen and Congressional committees. On the other hand, they still tend not to run for elective office either because they fear (or those non-Jews with political clout fear) that their Jewish background will tell against them at the polls.

By and large, Jews, especially since the New Deal, have been liberals or radicals politically, and Isaacs is of two minds about this. On the one hand, he shares with Larry Fuchs the view that Jewish "liberalism" is related in some way to traditional religious values and to a history of oppression, including the holocaust. On the other hand, he sometimes argues (rather unsuccessfully I think) that Jewish liberalism is a myth, and that Jews act on the basis of perceived self-interest "just like other folks." It is true that the attitude of some on the left towards Israel, as well as conflicts over "community control," and the placement of low-income housing for blacks in Jewish neighborhoods, may have pushed some Jews in a more conservative direction in the early 1970s, but, as Isaacs himself points out, two out of every three Jews still voted for McGovern in 1972.

Isaacs is also ambivalent as to the extent of Jewish influence. He seems rather pleased that Jews are relatively influential, but his anxiety lest Jews appear to be too powerful has probably led him to underplay their role in some key areas. For example, he characterizes Agnew's attack on the "liberal" Eastern media establishment as having anti-Semitic implications, even though Agnew never used the word Jew. He then attempts to "prove" that the media establishment is not Jewish. Isaacs never does tell us why an attack on the Eastern media should have anti-Semitic implications if Jews are actually *not* heavily involved with such media. More important, he is rather disingenuous as regards such involvement. It is certainly true that most of the big name commentators have been WASPs until recently, but, as almost everyone knows, Jews are heavily represented among producers, directors, and writers, and are quite influential.

Isaacs's anxiety about the Jewish image becomes even more clear when he discusses the radical movement of the 1960s. Not only does he soft-pedal Jewish involvement somewhat, but when he discusses it he can only note that the radicals were decent kids with unrealistic ideas who were "bearing witness" because of the holocaust. The discussion seems just a bit soft for a supposedly hardheaded journalist.

Pete Hamill, reviewing Isaacs's book for *The New York Times,* criticized it on somewhat similar grounds. Emphasizing contemporary American culture heroes and their creators (Dylan, Mailer, Roth, Heller, etc.) Hamill argued that American Jews had brought marginality front and center with profound effects on the society, and that Isaacs, by concentrating narrowly on politics proper, has underplayed the real extent of Jewish influence on the culture and politics of the society.

Even Hamill's focus is too narrow. As Lipset and Kadushin have demonstrated, Jews have come to play key roles at elite universities and among the intellectual community in general, at a time when the categories which a younger generation of Americans uses to explain the world are increasingly determined by the ideas such groups develop. And since Jewish intellectuals were, in the early 1960s, rather to the left of their Christian counterparts (and much more productive), they certainly played a role in the radicalization of college youth.

In short, Americans of Jewish background have served as an important radical leaven during the past fifteen or twenty years. It would be both useful and interesting to trace their role in detail and to attempt to understand it, using the same kinds of categories that social scientists (and journalists) are willing to employ when analyzing the political attitudes and role of police, the military, right wingers, or other groups. Until we are willing to do so with some honesty, the political revolution of the 1960s will never be fully understood.

STANLEY ROTHMAN

Smith College

Protest by the Poor: The Welfare Rights Movement in New York City. By Larry R. Jackson and William A. Johnson. (Lexington, Mass.: Lexington Books, 1974. Pp. 286. $15.00.)

This study of the welfare rights activities of the 1960s views welfare not as an economic problem, nor as a matter of social justice, but as a political issue, a question of who gets what, when, how. In this case, the "who" is New York City's poor nonwhites who organized during the "War on Poverty" in order to obtain for themselves a larger share of the "Great Society's" resources. For a brief while, the authors recount, organized welfare recipients were able to make the political system work for them. Bureaucrats responded to their pressure, the rules became flexible, and grants were increased. But the success was short-lived, lasting only until the "establishment" was able to mobilize its vastly greater resources, particularly its power to change the rules so as to erase the poor people's gains.

A major hypothesis of the authors, scholars affiliated with the New York City-Rand Institute, is that, despite claims by friend and foe alike that the welfare rights movement bore heavy responsibility for New York City's burgeoning caseload during the late 1960s, such was not the case. They contend that the movement's primary achievement was not to swell the welfare rolls but to increase the benefits to those already receiving assistance. They also deny often-heard claims that the growing caseload was primarily attributable either to administrative laxity or to liberalization of welfare policies. Each of these notions is controversial.

While the book is organized into eighteen chapters, the reader will observe that there are really only two parts: Chapters 3–11 contain a chronology of the welfare rights activities of the 1960s; the remainder are analytical and theoretical. Although both sections contain much that is of interest, each suffers from important drawbacks. In addition, the absence of any real connection between the two parts is a serious shortcoming. The chronological material is not used in the development of theory, nor is the theory applied to explain what happened on the streets.

The account of how the welfare rights movement originated, blossomed, and finally began to wither is generally well written, seems fairly complete, and sustains interest. Its weakness comes from the fact that although the authors explicitly acknowledge the political nature of welfare, they unconsciously allow their personal emotions to intrude upon their effort at objectivity. Their selection of sources, for instance, appears to be skewed toward a favorable view of the movement. Source materials originating from within the establishment tend to be overlooked or dismissed lightly, while those from within the movement are quoted at length, even when they are extraneous, such as a seventeen-point list of alleged practices by welfare officials which the movement considered illegal (pp. 37–38). Even worse is the authors' liberal use of emotionally-charged, yet undefined and unsubstantiated, words. For instance (italics added):

> . . . Until late 1967, the City-Wide Coordinating Committee . . . functioned in organizational *chaos*. . . . The Committee experienced *repeated crises*. . . . *Numerous disputes* and personality *conflicts* created *disunity*. The turnover in its leadership and staff was *appallingly* high. . . . Yet despite this early organizational *chaos*, the City-Wide Coordinating Committee managed to develop into an *effective* and *genuine* organization by the end of 1967 (pp. 97–98).

The difficulty with the analysis is that the data fail to sustain the hypotheses, yet the latter are restated as if convincingly proven. The data consist largely of statistics concerning the number of applications for public assistance, the acceptance rate of those applications, and the rate at which cases were closed, from 1966 to 1970. Each of those variables changed during that period in the direction of an increased caseload. After manipulating the data, the authors find that the higher number of applications is responsible for only thirty-three per cent of the increased caseload; this absolves the movement from the onus of increasing the welfare rolls. The lower rate at which cases were closed accounts for twenty-six per cent of the caseload increase; thus, administrative laxity cannot be blamed. And since higher acceptance rates explain only about eight per cent of the increase, liberalization of welfare policies surely cannot be at fault. These denials seem rather empty in light of the authors' finding that, acting in concert, the three factors caused sixty-one per cent of the caseload increase. If, as the authors contend, they are not responsible, then what is? The authors do not say. The answer, they tell us, is beyond the scope of their inquiry.

Although *Protest by the Poor* is, in a number of respects, an inadequate study, its assets should not be overlooked. It is an interesting case study in public policy making, it shows welfare dependency in a political light, and its hypotheses, while not convincingly verified, certainly are interesting points of departure for discussion and, perhaps, for additional research.

MICHAEL V. GERSHOWITZ

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The Political Character of Adolescence: The Influence of Families and Schools. By M. Kent Jennings and Richard G. Niemi. (Princeton, N.J.: Princeton University Press, 1974. Pp. 357. $12.50.)

A prize should be established for the first political socialization research report which rather than unraveling strands of possibility suggested by correlation or percentage tables until each falls away to a thread, instead weaves a tapestry with a texture which includes a conceptual framework for understanding the process. This book leaves the reader with admiration for the authors' accomplishment in handling a large mass of data collected from a national sample of nearly 1700 high-school seniors surveyed in 1965, their parents, and those social studies teachers who bore the largest responsibility for their education. It also leaves the reader with a sense of disappointment that threads have been unraveled rather than tapestries woven.

The major part of the book presents a detailed analysis of similarity between parents and their high-school-senior children, and between teachers, peers, and these same students. The book is organized primarily in terms of the agents of socialization, with the largest section presenting the relationships between the attitudes of high school seniors and their parents concerning political parties, public policy issues, knowledge, and citizenship roles (within a series of groups divided by control variables). A second section discusses the impact of the educational system as reflected in the civics curriculum (a chapter modified from a well known article co-authored with Kenneth Langton), the relationship between social studies teachers' beliefs and those of their pupils (co-authored with Lee Ehman), and a chapter entitled "The Political Texture of Peer Groups" (co-authored with Suzanne Koprince Sebert). The third major section is entitled "Longitudinal Perspectives," leading readers to expect the long-awaited results of the subsequent retesting of these students as young adults. Rather than true longitudinal data this chapter includes a comparison of the students and their parents with SRC election study data from adults of different ages.

A major purpose of this book is to "redress the balance" between assessments of the home and of the school as socializing agents. The authors conclude that parental attitudes as a source for student modeling make a unique contribution even when other influences are controlled; schools, on the other hand, have a less substantial impact. Students' attitudes do not seem to be overdetermined by these two agents, however. Young people make up their own minds; their particular cognitive abilities and personal needs place a clear imprint upon their attitudes.

Specifically, Jennings and Niemi conclude that when parents agree in their attitudes or issue positions, this homogeneity can produce student-parent congruity on responses to structured questions on issues which are as high (or nearly as high) as previous studies have found for partisanship and voting choice. From the point of view of parents, however, much of this socializing process is low-key and unintentional. The salience and concreteness of the issues considered are, of course, important in determining the level of congruence. For example, when more abstract concepts were tapped with more open-ended questions, only seven per cent of students and parents presented the same ideological view of the parties. The authors also report that when parental attitudes are at variance with those of the adult majority, students are more likely to "defect" from parental views.

The authors' findings concerning the redundancy of secondary-school civic education courses for white students have been widely cited. The major source of their conclusion concerning the modest impact of teachers is drawn from an analysis partialing out teacher attitudes and examining the relationship between parents and students, and then partialing out parent attitudes and examining the relationship between teachers and students. Parent-student similarity remained strong even after teacher attitudes were partialed out. The authors note that an atmosphere of fairness in a school seems to foster political trust and that peer groups are influential primarily through close friendship ties. More complex models may be necessary to settle the question of school influence with greater certainty. To place these findings in a comparative framework, it is interesting that in a regression analysis for predicting cognitive and attitudinal outcomes of civic education, other investigators found school effects to be less potent in the United States than in nine Western democratic countries (see Judith Torney, A. N. Oppenheim, and Russell Farnen, *Civic Education in Ten Countries: An Empirical Study*).

Jennings and Niemi conclude that black high-school seniors show accelerated politicization in many kinds of activities. Sex differences in student attitudes were less striking than expected; the authors commented on the important role played by mothers in the family's political socialization process. The section on sex differences concludes with a statement concerning the possible decline in feminine parental influence if mothers spend more time away from home – based on the dubious assumption that it is amount of time at home which is the important mediating variable, rather than, for example, presentation of a politically active model.

The section which compares the data from seniors with adults surveyed in SRC election studies is interpreted by the authors as evidence of socialization and developmental effects after graduation from secondary school. Some of the differences spotlighted are intriguing. Concepts of and orientations toward political parties are shown to undergo considerable change during young adulthood. The comparison of the salience of various levels of government for adults and seniors suffers from a confusing presentation of a three-way-choice using an inappropriate mean indicating level of trust (Figure 10.7). Life-cycle differences observed in conception of the good citizen role may reflect that parents were responding in terms of responsibilities of parenthood (e.g., concern for personal responsibility in their offspring) rather than as adults in general might respond. Socialization processes are difficult to pin down during adulthood given the additional complications of cohort and generation effects. The analysis here raises many useful questions about socialization after high school.

The book must be read with a few methodological cautions. Comparisons between correlation coefficients are made without reference to statistical significance or confidence bounds. One correlation coefficient may be significantly larger than zero and another coefficient may not be significantly larger than zero, yet the two coefficients may not be significantly different from each other. Many of the differences between correlations reported here could very well be the result of sampling fluctuations or differences in variability and not of true differences in the strength of relationship in the populations. For example, conclusions are drawn about differences in strength of association or "transmission rate" based on correlations ranging from .18 to .34 (p. 235), from .29 to .36 (p. 80), from .01 to .14 (p. 147). The number of respondents used in computing these correlations is not given, and the reader is left to ponder whether some of the differences may result from chance fluctuations or some artifact of the distributions.

There are some other difficulties as well. Efficacy is defined as the "self-perceived ability of citizens to cope with the political system and to have some impact on its performance." Yet the only two items used for both parents and students concern the recognition that there are ways other than voting by which the individual can influence the system and a perception that the government is not complicated. In a later chapter the authors refer to a group of students which is relatively more educated than their parents although the only data available concern students' educational aspirations not achievements.

Despite these difficulties, no shelf of books on political socialization or list of important citations will be complete without this volume. Parts of it will justifiably become classic and will stimulate further more focused studies. The competition is still open for the prize for a landmark study which weaves it all together.

JUDITH V. TORNEY

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Polish-American Politics in Chicago. By Edward R. Kantowicz. (Chicago: University of Chicago Press, 1975. Pp. 260. $12.95.)

One of the happy results of the current revival of interest in ethnic diversity in the United States is that a generation of young scholars has begun the arduous task of writing the research monographs that are indispensable for any theoretical understanding of immigration and ethnic diversity in the United States. Such young scholars very frequently study their own ethnic group, substituting careful, dispassionate (though scarcely uninvolved) research for the noisy militancy of a previous generation.

Edward Kantowicz's fine history of Polish-American politics in Chicago is an example of this new research commitment. It is more than just a history of Polish politics in Chicago; it is in fact a history of the whole Polish community in Chicago, of its great economic and social success combined with its very limited success, or even failure, in the political arena. For all their obvious importance in urban America, and for all the crude and stereotypical jokes about them, the Poles are a little-known ethnic group – until recently, less well understood by the university faculties who are their neighbors than are many African tribes.

Professor Kantowicz traces Polish political history in Chicago through its early concern with the politics of the old country, through the emergence of the Polish liaison with the Democratic party, on to the temptation of a Republican affiliation through "Big Bill" Thompson, and then, finally, a definitive commitment to the Democrats in the late 1920s. Detailed analyses of election returns in heavily Polish wards provide the principal evidence for the ebb and flow of the Polish vote and its final overwhelming commitment to the Democrats – a commitment which did not, unfortunately from the Polish viewpoint, win political power commensurate with Polish numbers.

Poles indeed became aldermen, ward committeemen, state representatives, and even United States Congressmen, but the Irish still ran Chicago, and the two Polish candidates for mayor, Waner and Adamowski, were soundly beaten – with Waner losing even in his own heavily Polish ward. While some Polish Americans did achieve success and recognition in politics, political careers did not turn out to represent a major channel of upward mobility for Poles. Kantowicz suggests that many Poles, realizing that this was the case, turned to other areas.

Why has Chicago never had a Polish mayor? Kantowicz hints that the Poles lacked the Irish skill to move from solidarity politics to coalition politics.

> For newly emergent groups, solidarity politics is probably a necessary first step; but it reaches a point of diminishing returns. Only through coalition politics, broker politics, the politics of painful compronise and careful bridge-building can a new group be successful at the highest levels in pluralist, polyglot America. Polish-American politicians were dedicated to the Polonia ideal of strength in unity. But in a pluralist society, politics must go beyond the internal unity of one group. The role of politics is to unify all groups in a society, to manage conflicts between groups (p. 218).

But eschewing further exploration of "the murky depths of national character," Kantowicz also observes that, "but certainly Polonia's difficult numerical position in Chicago, neither a small, hungry minority nor a clear, dominant majority, has had much to do with its failure to elect a Chicago mayor" (p. 219). So it is very likely, then, that the Poles will not have a "turn" as mayor of Chicago, and that the first non-Irish mayor will be black. Political power is not absolutely essential either for respectability or for success in American society, and yet there is some cause for concern if a particular group seems to be excluded from the final rewards of the political system. What is amazing about the Poles of Chicago is that they have remained loyal to the Democratic party locally and, for the most part, nationally, even though the party has given them less recognition than it might. Furthermore, it would appear from national survey data that Poles continue to vote heavily Democratic in national elections.

Kantowicz has ably summarized the fascinating story of the development of Polish politics in Chicago. Perhaps he, or some other scholar, will now go on to the next step – the study of the intellectual, cultural, and artistic development of the Poles – and try to explain the fascinating phenomenon that although unrewarded and often scapegoated, the Poles nonetheless continue to be Democrats and to vote overwhelmingly in favor of such liberal candidates as the two Adlai Stevensons.

ANDREW M. GREELEY

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Indispensable Enemies: The Politics of Misrule in America. By Walter Karp. (Baltimore, Md.: Penguin Books, Inc., 1974. Pp. 324. $2.95, paper.)

Some in the profession will feel that this work by a journalist does not merit review in the *APSR*, arguing that it is a diatribe. Certainly it is polemical. The book will receive vastly different receptions from teachers of parties.

Briefly Karp's thesis is that American politics are explained by a deliberate lack of party competition and conscious collusions by the "party oligarchs" which "spring up automatically between the party hierarchies." "Because the politics of special privilege is a policy of active, maximum corruption, it can only be carried out by means of two-party collusion. . . . It takes two collusive party syndicates to manage the inveterate abuse of power which the usurpation of power requires" (p. 162).

Party organizations endeavor to block reform in order to maintain themselves in power. They are powerful enough to choose officials for high office who are willing to serve their ends, and the two parties act in concert to defeat candidates who threaten either one. Karp asserts that New York Democrats were determined to defeat their candidate for governor, Arthur Goldberg, as the "National Democratic Syndicate" decided to knock off George McGovern (p. 184). Almost every recent president is made to look bad. It is avowed, for example, that Franklin Roosevelt deliberately proposed enlarging the Supreme Court to thwart reform (p. 111)! The Democratic party oligarchs are powerful enough to make the labor unions do their bidding such as supporting Jim Crow in the South and the Vietnam War.

The author admits that readers will find many of his contentions "incredible" and charge that he is advancing conspiratorial theories. He acknowledges both but insists that his interpretations are correct. His diagnosis resembles some made by those on the extreme left but he does not accept their prescriptions. Rather simplistically he sees the near-monopoly of bipartisan collusion broken up by local self-government and republican education for all. These two principles ". . . are not a panacea for our ills; they are the *sine qua non* for curing them" (p. 298). Jefferson and Tocqueville are Karp's heroes.

The theses of this work challenge much prevailing opinion that party organizations are weak, do not generate issues, or impose serious controls and restrictions on the party's public officeholders. Karp sees the opposite as the true picture. For example, he persistently asserts that "party organizations do wield great political power, for they control most of the nation's elected officials" (p. 152). I feel that there is insufficient hard factual evidence for a great many of Karp's contentions even though he writes persuasively and presents, in some cases, strong circumstantial evidence. Seldom are the party oligarchs identified. There is an implication that party elites can and do dominate the corporate and other elites rather than the reverse. The reader is not given a clear picture of how party collaborators manipulate so successfully. Moreover, Karp does not tell whether the "boss control" is facilitated by clandestine meetings of the organizational leaders themselves or by their emissaries, or whether it results from an uncanny, visceral understanding, or simply symbiosis. I found some statements factually incorrect – for example, Karp's assertion that Democrats in Washington are "largely restricted to Seattle."

The book feeds the fires of cynicism. Yet it contains many truths and examples showing that private power is "hidden from and unaccountable to the citizenry." Areas of two-party collusion other than gerrymandering have not received the attention they deserve from the profession. The tension between the organization leaders and the government party, while generally known, will also be better appreciated by the emphasis provided by the author. But instructors would be well advised to use the volume for purposes of challenge rather than gospel. Can the scores of instances cited be documented as cause and effect? Do our studies of party competition – or lack of it – reinforce or refute Karp? Can his formula for "restoration of self-government" be accomplished by local but not "municipal government" and education? The book most certainly will provide many students with topics for critiques of these and many other questions.

HUGH A. BONE

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The Politics of School Decentralization. By George R. LaNoue and Bruce L. R. Smith. (Lexington, Mass.: Lexington Books, 1973. Pp. 256. $10.00.)

The dilemmas of conducting policy analysis from the perspective of political science are made apparent by reading *The Politics of School Decentralization.* Professors LaNoue and Smith criticize behavioralism a la Dolbeare but answer the new (or revitalized) questions with the old methods of behavioralism. Ironically, they fail to advance the discipline of political science toward the new political science and policy analysis, but they succeed in conducting an excellent version of the old political science they are trying to supplant.

The *Politics of School Decentralization* "belongs to the new tradition – or rather, the re-emergence of an older tradition – that makes attention to public policy matters a central concern of research. . . . There is less interest in who governs than in what difference it makes who governs, whose interests are served, and how well policies meet the 'needs' of the governed" (p. 5). While the focus of the volume is on the conversion process, "the process by which demands for governmental action become or fail to become translated into public policy," the authors hope that the volume stimulates "more systematic research on ways to measure the effectiveness of governmental outputs and thus to judge the performance of urban government" (pp. 5–6). My reading of the book indicates that the latter issue, the evaluation of governmental output and effectiveness, is the key concern of the authors, but they are limited to studying the conversion process by their disciplinary backgrounds.

The bulk of the book comprises case studies of school decentralization in the St. Louis, Los Angeles, Washington, Detroit, and New York school systems. The studies are reminiscent of the Gittell and Hollander and Crain volumes of a few years ago. They are generally well written, strike a good balance between comprehensiveness and succinctness, and are well documented. Those interested in the topic of school decentralization will find them a valuable resource. The authors are most stimulating in their discussion of the conflicts and contradictions tied to decentralization; decentralization vs. metropolitanism, decentralization vs. integration, decentralization vs. reformism and change. The case studies offer evidence that decentralization is not a panacea or the neat answer to an urban educational puzzle; urban educational problems are ill-defined, are viewed differently by those holding various value positions, and are related to a myriad of other urban problems. The value of the case studies is limited by the conflict between the authors' goals and their capabilities which results in an inadequate specification of the link between studying the conversion process and drawing conclusions about governmental output and effectiveness.

Specifically, in addition to stating their general goals, LaNoue and Smith state research questions in several other places. On page 4 they claim to seek to "clarify such broad questions as: How responsive are governmental institutions? Is public policy sensitive (or overly sensitive) to the wishes of minorities? What mixture of professional and lay judgment leads to what kind of policy outcomes, and whose interests are mainly served?" On page 8 they raise several questions, culminating with: "How far can the values of openness, representativeness, and the drawing in of a wide range of society's creative energies be incorporated into the administrative process without incurring unacceptable costs in the loss of discipline, orderliness, and professionalism?" Finally on pages 22 and 23, the authors ask questions like, "What is the impact of decentralization on school personnel practices? What is the effect of decentralization on providing services and educational achievement? Who are the decentralization activists?"

The authors not only mix conversion, output, and effectiveness questions, they fail to establish the relationships among these questions. In fact, the volume lacks a systematic approach to a well-delineated set of questions throughout. For example, if the authors are interested in evaluating the political effects of decentralization, how do they define or distinguish among concepts like responsiveness, minority group influence, and professional accountability?

LaNoue and Smith's attempt at drawing policy conclusions from their study is disappointing. They conclude that "decentralization is a better alternative [to the end of increasing educational options] than a voucher or tax credit program" with little justification (p. 238). They conclude that the decentralization movement has not been "revolutionary" even in those cities where the movement developed the furthest. Inexplicably, the authors first conclude that decentralization has inspired little change, and then conclude that decentralization is superior to more radical alternatives like tuition voucher plans. Not only do the authors leave their data far behind in reaching this conclusion, they fail to address the more basic question that their analysis raises: can school systems be reformed?

All of this is to say that LaNoue and Smith have tried to add on the trappings of policy analysis without abandoning the case study method and process focus of political science. They offer us no general conclusions about the relation of governmental structure to governmental effectiveness. Meeting their goals calls for a new approach to political policy analysis including the systematizing and operationalizing of concepts like responsiveness and accountability. By analyzing the authors' attempt to use traditional political science to answer new questions of policy analysis, the reader is forced to confront a critical question facing the discipline of political science: can success at normative policy analysis be accomplished within the discipline of political science, or, alternatively, must the discipline be radically altered or abandoned to achieve normative goals?

JEFFREY A. RAFFEL

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Politics, Medicine and Social Science. By David Mechanic. (New York: John Wiley & Sons, 1974. Pp. 306. $12.95.)

This is a book of advocacy, not a report of new research. In an occasional way, it proposes research that would be helpful. It is beguilingly frank about things we do not know, but would like to know. A theme appears in some of the essays here collected, but is not brought to coherent analysis, as is the common fate of tomes so constructed. The writing is informed by extensive contact with the patterns of change in health service organization in this and other countries, by familiarity with field research, and by good judgment. The author has a measure of common sense not always found in this policy area.

Though Mechanic does not spell out the problem in categories of political ideas, he recognizes clearly that the central issue is the emerging demand for equality of condition, not just equality before the law or careers open to talent. In medical care, such equality would require full health services for all as a matter of right, not a commodity to be purchased if and to the extent the individual wants it and can afford it. Dislocated by this challenge is the role of the doctor. To a student of political ideas, the issue is particularly poignant. On the one hand, our hedonist, sensate, secular society places almost paramount value on preserving life, health, and the ever-youthful body. On the other hand, the doctor has been one of the last remaining, and for long generations the most honored prototype of an older liberalism, which saw the free man as autonomous, self-directed, responsible to himself and to his learned profession, asking himself the "right question," the good of others. Though the old liberal myth is now little regarded, it has a grip on our imaginations. Extending and guaranteeing services does lead to malingering, unconscionable costs, often enough with the doctor's connivance. The doctor does get supervised by laymen, disciplined by bureaucrats, loses his autonomy. Medical care does become impersonal, often hurried. Equality of condition is never reality, however socialist the socialism. Mechanic, a sociologist, only indirectly recognizes this underlying conflict of political myths that makes so difficult the re-organization of medical care, but he is sensitive to it.

The author would approach the study of medical service reform "by enlarging the social and intellectual climate within which policy-making discussions take place and by affecting the types of information considered relevant to social policy decision" (p. 53). He would have social science "serve as a form of community representation by proxy, attempting to put forth in the most reliable and objective way the likely repercussions of one or another policy on varying groups in the community." On the other hand, we are told an "honest social science can enhance the democratic process and retard the dominance of special interest perspectives . . . bring into consideration the needs of affected groups outside the main stream of government decision making." But he warns of "the political difficulties of such a role . . . and the danger that social science (which has its own needs and vantage points) will itself become a special interest or serve the interests that presently prevail" (p. 56). So we are embarked on the endless quest for the "public interest" and warned against the "interests that presently prevail." The social scientist stakes a claim to moral superiority, and slips from the self-assigned role of objectivity. Most policy analysis gets involved in something of this confusion of perspectives. This volume is not an exception.

The author fills the center of his book with variegated but illuminating observations. He notes a convergence in patterns of organization, specialization, institutionalized treatment, geographic maldistribution, fragmentation of care, across most of the nations of the Western world. He elaborates opinion survey material to debunk the doctor's claim to special hearing, finding that doctors oppose change mostly in proportion to their political conservatism, which derives from their status, training and class origin. Institutional, salaried, and per-capita paid doctors are less attentive to the patient's life situation than are private general practitioners. He worries intelligently about the ordinary courts spelling out requirements of treatment under new doctrines of rights. There is a nice chapter 16, reporting with refreshing humility that we do not know much about teaching health behavior or changing wayward personal behaviors rooted in social life. Mechanic is almost unique in observing that there is "no particular reason why health should be the highest value or the most central social goal" (p. 269). His good judgment is particularly marked when he doubts a dramatic increase in efficiency of service in the promised land, a salient error in political argument in this country today.

One misses in his essays much recognition of the role doctors have played in reorganizing services, particularly group practice. He is not explicit about the cleavages within the profession, though he avoids stereotypes about a monolithic, greedy, preternaturally skilled pressure group. He opposes categorical grants, by disease or patient type, which is politically correct for his program preference. It is almost certainly sound medically as well, the atomic bomb and trip to the moon notwithstanding.

The final policy prescription is for a national health service, by defined population area, with fixed budget, leaving to local medical administration the setting of priorities within the budget limit. This is an exceptionally idealistic model. It seems an unlikely design to emerge from present policy struggles. His proposed administration would be subject to guidelines set by professional consensus, with a channel for "consumers' expression of feelings concerning improper and unsympathetic care and to provide feedback to professional personnel" (p. 288). By suddenly ending the book with this ex cathedra little bombshell, with no wiring diagram, we are removed from the world of conflict, selfishness, inertia, economic waste, and administrative Juggernaut. We cannot really quarrel with the author's program preference.

OLIVER GARCEAU

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CORE: A Study in the Civil Rights Movement, 1942–1968. By August Meier and Elliot Rudwick. (New York: Oxford University Press, 1973. Pp. 563. $15.00.)

Meier and Rudwick's history of the Congress of Racial Equality (CORE) provides an opportunity to review the great sweep of the civil rights movement as experienced by people trying to shape events and, inevitably, shaped by them.

This book offers a rich, exhaustive account of the formation and transformation of this organization. CORE began in 1942 as a small group of seminarians dedicated to racial equality and to the tactics and principles of Ghandian nonviolence. The authors trace at length the founders' early struggles to develop and sustain action-oriented chapters throughout the nation. Indeed, CORE was ready to provide a vehicle for the civil rights movement in 1960 – almost twenty years after it was founded – only because of the extraordinary dedication, tenacity, and vision of a very small group of people over the previous two decades.

CORE achieved national recognition when it organized the inspiring Freedom Ride of 1961. Professors Meier and Rudwick describe the organization's efforts to nurture and capitalize on various episodes of southern black protest, culminating in the voter registration and political mobilization campaigns of 1963–64 in Mississippi and Louisiana. They further treat the efforts of CORE affiliates to initiate links with northern ghetto communities. Meier and Rudwick's history concludes with CORE's ultimate transformation from an interracial membership organization with dozens of local chapters to a much smaller group dedicated to black self-help and separate development.

This massive study is preeminently the history of an organization. Based on files and extensive interviews, it alternately focuses on the activities of the national staff and those of the affiliated chapters in an attempt to capture the complexity of a national organization dependent on program implementation at the local level. The chief limitation of the organizational focus is that the authors cannot fully treat the environmental context in which the possibilities for action were structured. CORE's great expansion in the early 1960s did not result so much from changes in organizational character or strategy as from the readiness for action of masses of people stimulated by the student sit-ins. The leaders of civil rights organizations were heroically riding a whirlwind over which they had little control. Yet this lack of control, so critical to understanding the civil rights movement, is obscured by a narrative focusing on the organization.

Indeed, the leadership of CORE, as reported by Meier and Rudwick, often appears to have chased the Movement around, continually seeking opportunities to obtain publicity and establish an organizational base. The authors allude to the sea-changes in public attitudes that sweep over CORE during the period of the study. But only in their discussion of the transition and decline of the organization during the period of "black power" of the later 1960s do they effectively provide insight, *through the medium of an organizational study,* into the dynamics of national civil rights developments.

Meier and Rudwick are most interesting in their account of CORE's internal processes, largely because such perspectives are rarely available to students of social movements in such detail. A reading of this book provides confirmation of leadership dilemmas such as the following:

(1) National CORE leaders were continually concerned with gaining credit and publicity for the organization while providing the appearance of unity and cooperation with other civil rights groups. In part this was because of the fund-raising implications of gaining credit for program success, and in part because (particularly in the early 1960s) they had an ideological commitment to proliferating nonviolence as a tactic. This concern, also characteristic of the other civil rights organizations to be sure, resulted in considerable backbiting and bitter feelings between organizations.

(2) The apparent contradiction between developing membership-based, action-oriented political organizations, and stable bureaucratic practices, is well illustrated by the CORE experience. CORE constantly operated with great tension between its program on the one hand, and its organizational capability on the other. The organization was almost never on a firm financial footing. The more it tried and was called upon to do, the more precarious became its position, as additional efforts expanded much faster than income.

(3) Over and over again, local CORE chapters discovered that their ability to sustain membership for mass action was very limited, although the need to project the impression of cohesive local chapters remained high. Moreover, difficulties in maintaining members' commitments seemed to occur equally because affiliates experienced success and new ways to direct members' energies were not immediately clear, and because they experienced failure and members became discouraged.

To review the history of CORE is again to be instructed by and puzzle over the drift from mass mobilization for desegregation to political action in the name of separate ethnic and racial development. Rudwick and Meier's conscientious history of the organization reminds us of just how fragile the enterprise of the civil rights movement was, and how precarious its legacy is.

MICHAEL LIPSKY

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Blueprint for Health: A Multinational Portrait of the Costs and Administration of Medical Care in the Public Interest. By D. Stark Murray. (New York: Schocken Books, 1974. Pp. 220. $7.50.)

This slim volume is another in the growing number of books and monographs offering prescriptions for the ailing health care systems of the Western world. The focus of this particular effort is on the need for a significantly strengthened scheme for health care in Great Britain, but using principles of organization and financing which have applicability in other settings. The specific solutions advanced by Dr. Murray are not new and represent fundamentally reformulations of concepts which health people have used to support changes in the health care delivery system in the United States and elsewhere.

The basic features of the revised health service advocated by Dr. Murray, a physician with substantial experience in the British Health Service, are outlined in the final three chapters of the book. Comprehensiveness and universality are the two central criteria which Dr. Murray believes should govern the development of any health care system. His concern with comprehensiveness is shared by most observers of the health care systems in the world. His position is simply that individuals served by the system should have access to the total spectrum of services, ranging from prevention to rehabilitation in contrast to the consideration that most schemes today give primary attention to the financing and provision of curative services. The second of these criteria – universality – means that the system must cover everyone, not just those who, for one reason or another, have access to institutions and health care personnel.

To accomplish these twin objectives, Dr. Murray stresses the need for an integration of primary care efforts with other services through the development of health centers in which teams of primary and specialized health workers would function. These centers, some of which already exist in rudimentary form, would constitute the base of a system in which there would be two additional levels. Immediately above the centers would be a series of regional planning bodies with coordinative responsibilities to insure a minimum of overlap and duplication of effort among the local health centers. At the apex would be a central group charged with policy planning as well as with financing the overall system.

Throughout the discussion of the proposed plan, Dr. Murray also emphasizes the need to develop appropriate governance mechanisms for the system. His particular concern is that the consumers, as well as the providers, participate in the control over services and their delivery. Thus, he outlines a series of ways by which this objective might be achieved, ranging from elected health center committees, to worker committees, to various councils at each level of the organizational arrangement.

To finance this new mode of health care delivery, Dr. Murray argues for a shift from insurance and service fees to general taxation. It is his contention that neither comprehensiveness nor universality of services can be adequately financed except through general tax revenues. Insurance and service fees create barriers to accessibility and secondly, create limits on the services, especially preventive services.

These specific characteristics of an appropriate health care system are based on the earlier sections of the book in which Dr. Murray criticizes existing practices in various Western nations. This overview of the general problem is followed by a series of chapters in which he focuses on specific elements of the National Health Service in Great Britain, giving attention to both the improvements which have occurred since its establishment and the shortcomings which have emerged. His analysis proceeds from an assumption that comprehensive and universal health care will ultimately require a full socialization of the system. This posture is perhaps the basic reason he feels that general taxation rather than insurance or other means, must be used to support health care.

Also implicit in Dr. Murray's approach is the belief that health care is an integral and basic element in any social welfare system. He recognizes that significant political and social changes must precede this development. His principal argument is that basic improvements in the provision of health care will not occur until the debate about approaches is framed in these terms. Unless this is done, we will continue to develop partial responses to the needs which, in both the long and short run, will be costly and inadequate.

The book provides a good overview of many of the central issues involved in the development of health care systems. Its primary value is to the general scholar in the field and not the specialist in health policy.

JOHN H. ROMANI

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Criminal Justice in Middle America. By David W. Neubauer. (Morristown, N.J.: General Learning Press, 1974. $4.95, paper.)

In recent years, political scientists and sociologists have been making a serious contribution to public discussion about American criminal justice. They have been doing so by pointing out that criminal justice institutions are to be understood not only as legal phenomena but as political actors as well – as agents whose partisan and organizational dimensions are similar to those of other, more explicitly political bodies and whose actions are to be assessed in terms of their political effects. The best of these social scientists have not claimed that the legal character of these institutions is irrelevant to their behavior; their argument has simply been that legal standards can also be understood in nonlegal terms, as an element of the social and organizational norms that govern criminal justice administration. And one interesting effect of this meta-legal perspective has been to call into question some of the suggestions that are currently made, in the name of enlightened law enforcement, for the reform of the criminal justice system. It seems that fair and rational legal administration can have problematic political effects.

Professor Neubauer's book is useful not because it asks new questions about this general theme or gives new answers, but because it provides an example of the prevalence of this tension between legal reform and other values that one might wish a criminal justice system to serve. The book sets out to describe an actual court system in relatively advantageous circumstances. First of all, the system is "reformed" in more than one sense of the word. For one thing, the jurisdiction in which it operates – Illinois – has recently seen important changes in its criminal law and procedures: decriminalization of certain vice offenses, reduction of statutory penalties, simplification in the definitions of many crimes, and the elimination of the private bail bondsman system. For another, the recruitment and career advancement system for most court personnel is relatively immune from the imperatives of party politics.

"Prairie City," the community in which the court operates, is neither a metropolis nor a hamlet; it escapes both the pressures of overloaded courts and the inconsistency that a too-small caseload can foster. The community is relatively homogeneous: the population is 95 per cent native-born and 80 per cent Protestant. The major source of heterogeneity of values and behavior in the city is its 11 per cent nonwhite population, but plentiful job opportunities and the absence of any history of overt job discrimination have helped the place escape any major racial conflagration or overwhelming racial tension. Finally, while overall crime levels in "Prairie City" are not notably low, the community does not see the crime problem as an overriding crisis. Vice crimes, a perennial source of ethical and administrative problems for courts and police, are relatively rare.

So it is not surprising that the court system in middle-sized "Prairie City" does not exhibit all the pathologies to be found in the courts of larger urban centers. The local bar is not rigidly stratified into those who are a part of the criminal system and those who are not, and this in turn means that the standards of the whole bar are brought to bear on all court personnel. Prosecutors feel relatively immune from popular pressures, whether to enforce dubious laws or to support the local police and *their* definition of what constitutes a prosecutable crime; the prosecutors are able to control the charging process, and to exercise that control without violating their responsibilities as advocates or as officers of the court. Defense counselors, for their part, are conscious enough of professional scrutiny to make the accommodations necessary to the system's functioning without adopting a cynical attitude towards their clients or abandoning their capacity for criticism. The system's judges are not notably solicitous of defendants' rights or lenient in their sentencing attitudes; but their effective power is limited by the extent to which, throughout the adjudication process, they accept the judgments of the other court personnel. Plea bargaining in "Prairie City" involves relatively little alteration of actual charges, and the process pays more attention than might be expected to the question of actual guilt or innocence. Sentencing is fairly consistent, and the disparity between the fate of the poor and that of the nonpoor is less than in other places that have been written about.

Neubauer by no means contends that "Prairie City" is without problems. For one thing, consistency of treatment is, not surprisingly, bought at the expense of consideration of individual circumstances. For another, Neubauer argues that the system both overemphasizes and underemphasizes cultural differences, in equally egregious ways: it is reluctant to afford the law's equal protection to lower-class persons involved in domestic disputes, while it prosecutes aggravated battery cases with a vigor that ignores the differences in the meaning of violence to different people. These two criticisms, of course, call on the "Prairie City" court system to adopt partially contradictory virtues; but the arguments do suggest that the dilemmas of heterogeneity have not bypassed "Prairie City" entirely. Most important, finally, is the ultimate result of this efficient, consistent, largely nonarbitrary system: "Prairie City" sends a lot of people to prison. It has an average of 1.768 people in prison per thousand population, compared with a figure of .482 for downstate Illinois as a whole.

This is not a previously unobserved connection; consistent administration of justice is often empirically, if not logically, associated with relatively harsh administration of justice. (See Martin Levin, "Urban Politics and Judicial Behavior," *The Journal of Legal Studies,* 1972, 1, 193–225.) This is not to say that the connection can yet be called a universal one, or that future reforms in behalf of consistency and efficiency need necessarily bring this kind of results. But Neubauer's book does join that body of literature emphasizing that in the area of criminal justice, we can not expect that reformers are going to be able to reconcile all the goals they have espoused.

SUZANNE R. WEAVER

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Equality and Efficiency: The Big Tradeoff. By Arthur M. Okun. (Washington, D.C.: The Brookings Institution, 1975. Pp. 124. $6.95.)

Reading *Equality and Efficiency: The Big Tradeoff,* by Arthur Okun of the Brookings Institution, is like having a leisurely conversation with an old and wise friend on the major moral messages behind current discussions of public policies. His principles are proper – he prefers equality to efficiency except when it costs too much. In other words, Mr. Okun is a moderate and reasonable man. Therefore it is certain that his principles will appear peculiar if not perverse to those who think a tradeoff is equivalent to a sellout.

A personal book invites a private response. To my taste Okun is short on principles but long on policy. He lacks singlemindedness. I am dubious about his principles – he thinks capitalism is strong on efficiency but weak on ethics, and I believe the reverse; he faults markets for being based on greed, while I prefer the private self-assertion of exchange to the collective force of envy. The assertion of the common interest in everything will leave it operative in nothing. I prefer to divert rapacity rather than convert all human desire until the sin becomes the excuse for the sinful.

But when it comes to policy as against philosophy, to a realm in which a combination of insights is often superior to a single monomania, Okun is a winner. Among many possible illustrations of his perspicacity – from government competition with private oil interests to subsiding wage rates for the working poor to an appreciation of the real gains of the social programs of the '60s to "a redirection of federal expenditures toward income-equalization programs and away from other outlays" (p. 105) – let us focus on the distribution of income. Okun begins with some facts that place limits on distribution under democratic conditions. If no compensation would be paid to nationalize all corporate wealth, $150 billion could be realized at a price: "It would annihilate the constitutional rights of the affluent and would clobber the welfare of small savers as well as universities, philanthropic organizations and the beneficiaries of pension funds" (p. 54). If fair compensation were paid, the returns from nationalization would be surprisingly small – about one per cent of GNP, or $13 billion at 1973 prices. Compensation of executives could, of course, be limited. But if the top were put at $60,000 per year, the pay of a Cabinet member, "the total amount saved would be only a fraction of one per cent of the national wage bill" (p. 54). As Okun says " 'fair' expropriation has less potential than tax reform" (p. 53).

Unlike Rawls, Okun would not almost always give priority to equality, and unlike Friedman he would not emphasize efficiency. "My answer," Okun says, "isn't always neat . . . I compromise." He would give equality heavy weight but would rely on "the democratic political process to select reasonable weights on specific issues as they arise" (pp. 93–94). These are worthy principles for policy as a meliorative process, but I disagree with the way in which Okun arrives at them.

To me it seems that Okun is unnecessarily concerned about how his pragmatism squares with Rawls's egalitarianism. John Harsanyi has decisively (until someone convinces me otherwise) refuted John Rawls' egalitarian position by showing that it often leads to the same results as classical utilitarian doctrine and, where it does not, clearly leads to undesirable moral results. Why should society pay an infinite amount to slightly improve the few worst off, as Rawls would require, when it could immensely improve the many slightly better off at less expense? Trading off, as Okun would have it is, after all, a part of utilitarianism.

It is one thing to trade efficiency for equality when life has decreed that we cannot maximize simultaneously on both dimensions; it is quite another to withdraw realms of activity wholesale from the need to trade at all. Okun is worried about conflicts between the domain of rights versus the domain of the market. Some things, after all, are sacred and ought not to be considered in terms, as was once said, of vulgar trade. If, instead of money, however, we speak of its analytical equivalent, that is, of opportunity costs, extending the religious realm is equivalent to saying that economic calculations are undesirable. My feeling, which Okun might disapprove of, is that if "rights" and "merit wants" and other such concepts are used to take increasingly larger chunks out of the province of markets, then I for one shall have much less need for economics, and considerably less interest in economists. Then I shall talk only (or largely or mostly) to moral philosophers (with possibly a captive cost accountant around to calculate sums). If, for instance, gas with which to drive cars belongs in the realm of rights rather than the mandate of the market, it would be difficult to find interesting problems for which the expertise of economists – there being little left to the economy – would still be appropriate.

Okun's brand of bourgeoise reformism (mine too!) is easily unmasked by such statements as, "To me, the purpose of heavier taxation at the top of the income and wealth scale is not to bring down the affluent but to raise up the deprived" (p. 105). Once, in those lengthy campus conversations about justice and repression that were a staple of the 'sixties, I asked a group of students which of these situations they would prefer: (a) the increase in income of the rich is redistributed to the poor or (b) all the wealth of the rich is destroyed. Most students, being reformist, preferred (a) because some are better off and none are worse off but a few, being radical, preferred (b) because formal equality would be greater, despite the fact that some people would be worse off and none better off. They would do well to join Arthur Okun's floating seminar, and so would we all.

AARON WILDAVSKY

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The Morrison Era: Reform Politics in New Orleans. By Joseph B. Parker. (Gretna, La.: Pelican Publishing Company, 1974. Pp. 154. $7.95.)

Most political scientists under the age of thirty-five and many beyond that age are likely to regard this book as an anachronism. This is not solely because the subject is historical, since the events of the Morrison era are recent enough to be considered contemporary for purposes of political analysis. But the pattern of politics described here seems more akin to the pre-World War I Age of Reform than to post-World War II urban politics. For the story turns on the career of deLesseps S. "Chep" Morrison, who was elected to four successive terms as Mayor of New Orleans in the role of a reformer battling the evil forces of an old-fashioned city machine.

Furthermore, the book is unpretentious (and therefore out of keeping with the dominant mode of the discipline) in at least two, and possibly, three ways. It does not purport either to refute or to validate any grand theory of urban political power, and it is not self-consciously concerned with research design or methodological gimmickry. The possible third evidence of near-Victorian modesty is an almost total absence of social science jargon; and I use the modifier "possible" because I can never be sure whether the deadly prose of so many of our professional brethren is used to demonstrate that we are dealing with an established body of knowledge, or is the result of a simple inability to think and write in the native language. Whatever the case, Mr. Parker's lack of sensitivity to the mores of the political science fraternity will almost certainly mean that the book will be considered hopelessly outmoded as a merely descriptive, rather than an explanatory or, even more aspiringly, a "causal theory" effort.

Quite apart from its Dionysian and gastronomic attractions, New Orleans has had an intrinsic appeal to the afficionado of politics as it is practiced in the provincial arenas. Because of its early development, ethnic composition and function as a major port, New Orleans is the only city in the South with the dubious distinction of having developed a full-fledged urban machine paralleling those in the North during the nineteenth century. The pattern of ward organization, patronage, service as a welfare surrogate, control over vendible blocs of votes, and boss-oriented leadership characteristic of this phenomenon generally also typified the Regular Democratic organization for upwards of sixty years. Periodically, too, the organization was beset with temporary incursions by reform groups who lacked staying power; but in the 1930s Huey Long managed to break the local hold of the machine and reconstitute it for his own larger purposes.

Morrison, who at the age of thirty-three had just returned from the war with the rank of Colonel, was a last minute choice of the then current reform group to oppose Mayor Robert S. Maestri in the 1946 election. Not only did Morrison surprise both the machine and his own supporters by winning, but he organized so effectively and expanded his support so broadly that he eventually wrote the epitaph for the Old Regulars by dominating New Orleans politics for the subsequent sixteen years. Although he was able to extend his base of support throughout the French Catholic geographic triangle in the southern part of Louisiana, he was unsuccessful in three gubernatorial races because he barely made a dent in the vote of the predominant Anglo-Saxon Protestant culture of the rest of the state.

The major interpretative emphasis in Professor Parker's review of the era is that, once Morrison was elected, his efforts at reform were subordinated to organizational politics designed to perpetuate and extend his political power. As support for this argument the author points out the extent to which the Morrison organization – The Crescent City Democratic Association – continued the pattern of ward-based control through local leaders, some of whom were co-opted Old Regulars. He also stresses the extent to which Morrison personally dominated the organization, relied on political professionalism rather than ideological commitment to augment his electoral strength, and concentrated more on favors and adjustments of minor local problems than on solutions to major social and economic issues.

Some of these assessments are undoubtedly correct but, as some of his own data suggest, the matter was more complicated than Professor Parker indicates. In the first place, there is little in Morrison's background to indicate that he was ever a programmatic reformer. But Morrison was a structural reformer as well as something of an urban rehabilitationist, yet his overhaul of the Municipal Charter and Civil Service reform are mentioned only in passing, and urban renewal under his administration is not treated at all. Furthermore, Professor Parker's tables on the distribution of Morrison's votes among various socioeconomic strata show a steadily increasing appeal to a wide variety of groups, including blacks and working classes, which implies that many voters perceived Morrison as a more satisfactory urban policy-maker than any of the alternatives open to them. Thus Morrison's reforms in New Orleans may have appeared to contemporary locals as being more extensive than they seem to the retrospective observer whose point of reference is not the Age of Reform but the policy aspirations of a later breed of urban utopians. Even in this context, it is probably the case that Morrison's structural reforms (including expanded opportunities for popular participation) were necessary precursors of the more substantive reforms the city now seems to be undergoing.

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Curing the Mischiefs of Faction: Party Reform in America. By Austin Ranney. (Berkeley: University of California Press, 1975. Pp. xi, 218. $8.50, cloth; $3.25, paper.)

This book, an outgrowth of the Jefferson Memorial Lectures delivered by Professor Austin Ranney at the University of California, Berkeley, in February and March, 1973, is a thoughtful and timely examination of the theory and practice of the various efforts that have been made to reform political parties in the United States from the 1790s to the first half of 1974. "Party reform" Ranney defines as the "adoption of party rules and public laws intended to alter the structure and behavior of an established party or party system" (p. 12). In addressing himself to this topic, Professor Ranney speaks as both a scholar and a practitioner. From 1969 to 1972, he served as a member of the Democratic party's Commission on Party Structure and Delegate Selection (the "McGovern-Fraser commission") which produced some of the most far-reaching rule changes in that party's history.

Professor Ranney identifies three major waves of party reform efforts since the initial establishment of the American party system in the years following the adoption of the Constitution. The first was the Jacksonian reforms of 1820–1840, which ended the making of presidential nominations by the Jeffersonian Republicans' congressional caucus, and led to the use of national presidential nominating conventions. The second was the Progressive reforms of 1890–1920, which featured the introduction of the direct primary and the adoption by most states of elaborate legal codes to regulate the state parties' internal affairs.

The third reform wave has run from about 1952 to the present day. This has seen the national committees and conventions assert that they – not their state and local affiliates – have the ultimate control over membership in national party organs. And it has also seen efforts by both parties to make their procedures for selecting national convention delegates more open and their conventions more representative. Writing in early 1974, Ranney felt that this latest reform wave had still not spent its course.

It is Ranney's thesis that these great reform efforts have occurred in part because Americans are profoundly ambivalent in their attitudes toward political parties. We tinker with the rules because we don't want to grapple with the fundamental alternatives of either abolishing parties or allowing them to act as they will.

Ranney holds that James Madison, among many others, seems to typify this American combination of antiparty thought and partisan action. In 1788 he published his Tenth Federalist paper, with its famous argument that the first objective of republican government must be to find "methods of curing the mischiefs of faction" by the fragmentation and dispersion of governmental power, thus making it impossible for a single party or faction to obtain a monopoly of power and impose its will everywhere. That was Madison the theorist. Yet less than two years later, Madison the politician joined Thomas Jefferson in a determined effort to resist the foreign and domestic policies of Alexander Hamilton. To do so, it was necessary to unify the anti-Hamiltonians in Congress. Thus Madison, the critic of faction, became one of the chief architects of the new Republican party.

There follow three illuminating discussions of central questions concerning the proper status of parties about which Americans have never fully agreed. In a chapter on "Putting the Parties in their Place," Ranney provides an interesting analysis of the development of nominations by legislative and congressional caucuses, and the subsequent shift from the caucus to the convention. He also explores the issue of whether parties shall be treated in law as private associations or public agencies, legal regulation of American state and local parties, and the current conflict over the legal status of the national parties.

In a chapter on "Making the Parties Behave," Ranney discusses how to make the parties representative of their members. In the selection of delegates to national party conventions, he sees two competing conceptions of fairness which the Democratic party, in particular, has been grappling with in recent years. One he calls "preferential proportional representation," the notion that each state delegation should include supporters of the various candidates in the same proportions as they exist among the state's Democrats. The other is "proportional *demographic* representation." This is the notion that a convention, or other representative body, should be a sample or a map-to-scale of the population it represents. This led to the famous "quota systems" for women, young people, and minority ethnic groups in the 1972 Democratic Convention (quota systems which Ranney, as a commission member, eventually opposed).

In this section Ranney also discusses the introduction of the direct primary, starting with the nation's first mandatory and comprehensive direct primary law, passed in Wisconsin in 1903. That law was enacted with the vigorous support of Robert M. La Follette, who had lost bids for the Republican nomination for Governor in the party's state conventions in 1896 and 1898. In its implications for party structure and party processes. Ranney considers the direct primary "the most radical of all the party reforms" (p. 121).

A third major discussion is entitled "Putting the Right People in Charge." Once again basic issues over which Americans have been divided for over a century are carefully dissected. Who are the party members whom conventions and other party organs are supposed to represent? Is it the party regulars? Enthusiasts for particular issues or candidates? Self-designated "members" of a party? All voters who feel they would like to vote in a particular party's primary? Or any and all voters? A second theme is the issue of national versus local control in the organization of the American national parties. Ranney calls the years from 1824 to 1956 an era of decline for organs of the national parties. But he also sees interesting and potentially important signs of a revival of national control from 1956 to the present.

Ranney concludes by stating explicitly his own value preferences on a number of these recurrent and often hotly debated issues of party reform. He feels that all devoted party followers – of whatever ideological persuasion – should ask of any proposed reform rule, "Is it fair?" and "Is it moral?" But they should also ask, "Will it help or hurt our party's chances to win elections?" In part because of this second standard, he favors "preferential representation," but not "demographic representation" (p. 197).

Considered as a whole, Professor Ranney's book is an important and thought-provoking contribution to the literature on political parties. There is interesting new material on the development of party institutions and the attitudes of opinion leaders toward political parties in the United States. At the same time, the book is as much a thoughtful synthesis and evaluation of research findings by others on political parties as a study based on the generation of new empirical data. As a consequence, it raises intriguing new questions for further empirical analysis. There is more, for example, to be done on the development of nominating institutions in the late 18th and the 19th centuries. What were the factors that caused their development? What were their consequences? And why did the new institutional arrangements have the political effects that they did?

The causes and consequences of the adoption of the direct primary also appear to be a particularly promising field for further investigation. Ranney stresses the development of the direct primary in the North – particularly the push given it in 1903 by the La Follette progressives in Wisconsin, where the convention system seemed to function as an impediment to the elevation of progressive candidates to power. But as the work of political historians such as Albert D. Kirwan on Mississippi and L. M. Holland on Georgia suggests, there were experiments with the direct primary in southern states *before* it was widely adopted in the North. And a major factor in the South – in addition to hard-nosed calculations as to who could win better with a new electoral device – may have been a desire to escape from one-partyism by providing an arena for meaningful political struggles in the primary.

Professor Ranney's point about the basic ambivalence of American attitudes toward political parties is well taken. It could, as he implies, easily be broadened to an even more fundamental ambivalence toward political power itself. And as events as recent as 1973 and 1974 make clear, perhaps that dispersion of governmental power which Madison felt is the only sure restraint on its abuse is buttressed most effectively only when one party does *not* control both the executive and legislative branches of the government. What, for example, would the outcome of the inquest into the domestic activities of the Nixon administration have been, if Mr. Hutchinson rather than Mr. Rodino had been Chairman of the House Committee on the Judiciary? Or if a Republican Senator backed by a Republican majority, rather than Senator Sam Ervin, had been Chairman of the Senate Select Committee on Presidential Campaign Activities?

Finally, this admirable and stimulating book has a special flavor because, as already noted, its author not only is a scholar but also participated actively in the forging of the Democratic reforms of 1969–1972. One senses that Ranney, like many a politico who observes subsequent events after the enactment of reforms, may occasionally have asked himself: What have we done? At the end of his book, he wisely admonishes the reader to beware of the unanticipated consequences of sweeping political reforms. "No one concerned with party reform has been able to forecast the consequences of particular changes with much accuracy and consistency" (p. 208).

And why have would-be reformers "encountered so many surprises, pleasant and unpleasant" from their reforms? The answer, Ranney suggests, is that "party rules and procedures constitute only one set of factors operating in a vast maelstrom of other triggering and contagion factors" (p. 208). Now that the 1976 primary and general election campaigns have taken place under new ground rules, those words may take on even greater relevance.

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Policy Analysis in the Federal Aviation Administration. By Steven E. Rhoads. (Lexington, Mass.: Lexington Books, 1974. Pp. 160. $13.00.)

This is *not* a book about policy analysis in the Federal Aviation Administration (FAA). Rather it is a discussion of some contemporary national aviation issues: charges, airport control towers, and other matters relating to the use of the airways. Approximately fifteen pages before the end, the author finally gets around to a brief consideration of the role of analysis in FAA, but even then he fails to animate his study with the stuff of public policy: real actors, organizations, and politics. Throughout the book, FAA is a lifeless bystander, not an active policy maker.

The problem is more than a misleading title for there still is no adequate study of the application of policy analysis (or PPB) by a federal agency. Professor Rhoads comes closer than others, but he does not penetrate the administrative curtain to examine the place of analysis in government decision making. Nor does he make effective use of the relevant material and evidence such as FAA budgets, congressional hearings, or policy actions. There is an occasional reference – barely more – to legislative proposals, but little presentation of details. The reader is not even informed of the place of FAA in the Department of Transportation or of the role of the Office of Aviation Policy and Plans in FAA.

This lack is unfortunate because throughout the book Rhoads offers judicious and succinct commentary on the validity of various analytic approaches. The tone is balanced and reasoned, a welcome relief from the polemics of the PPB and policy analysis literature. Rhoads takes the position that analysts should be "principally concerned with policy rationality" rather than with political feasibility and he therefore restrains himself from exploring the political world of FAA. What emerges is an analysis of analysis, not of policy.

After a brief introduction to aviation issues and a six-page description of various legislative proposals of the Johnson and Nixon Administrations, Rhoads presents the "analytic perspective," a rigid commitment to economic efficiency, consumer sovereignty, and a cost-effectiveness test for all proposed FAA activities. The bulk of the book is an examination of how analysts (mostly outside FAA) evaluate the costs and benefits of FAA investments. Rhoads constructs a "debate" between analytic proponents of user charges and interest group opponents. The debate is fueled by the author's repeated injection of "analysts would respond," "most analysts think," "analysts often ask," and similar substitutes for a full-blown exposition of analytic practice. The author intervenes constantly in the "debate," passing judgment on the would-be claims of the sides – always sensible and fair, but never allowing the reader to gauge the battle by himself.

The most interesting (and lengthiest) portions of the book relate to family disputes among economists over the valuation of time and life. Of particular relevance is the dispute between those (such as Fromm) who would use a straightforward earnings test to evaluate FAA safety activities and those (such as Schelling and Taylor) who seek a broader, politically sensitive measure of value.

The book contains much that relates to the utility and limitations of economic analysis for government organizations. The abortive PPB experience of the 1960s showed that the most serious problem is *using*, not *doing*, analysis. Perhaps Professor Rhoads will carry his study a big step further by reporting what happened to analysis when it met a government organization. As Elizabeth Drew suggested some years ago, this is the key to understanding the fate of policy analysis in FAA and elsewhere.

ALLEN SCHICK

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Lessons from America: An Exploration. Edited by Richard Rose. (New York: John Wiley & Sons, 1974. Pp. 308. $14.95.)

Overseas Americans, for at least a quarter century, have been engaged in the task of transmitting "Lessons from America" to an initially receptive, now increasingly resistant underdeveloped world. Occasionally they may have asked themselves the two key questions with which this collection of a dozen essays is concerned: "to what extent is the American experience unique," and "to what extent does it provide a model for the rest of the world?"

The present undertaking reflects the effort, "deceptively simple" in the words of editor Richard Rose, by a group of scholars, nine Britons and two Americans, to assess the relevance of the American experience for the more developed world. Specifically, the question to which the editor directs the work is whether America is "inevitable or inimitable?" The range of the responses is broad, covering political science, history, economics, sociology, advertising, broadcasting and even architecture and design. The contributions have been assembled under two headings, "Myth and History" and "Lessons Drawn," although not all of the essays fit neatly under either rubric.

One of the most delightful (if unusual) approaches to "Myth and History" is provided by Reyner Banham, Professor of the History of Architecture at University College London. In his urbane and witty "European and American Design," Banham surveys the design field from skyscrapers to household gadgets, noting along the way the superbly functional purity of the frontier Bowie knife and sharing LeCorbusier's admiration for those "gifted savages" – American engineers.

Noting accurately that "the neat engineering solution is an integral product of U.S. culture" (p. 82), Professor Banham rather neglects the flood of trivial contrivances (from electric toothbrushes to mechanical carving knives) this approach has also loosed. But it is not necessary to limit to design his conclusion that, to the careful analyst "America becomes less a prophecy, less of a model, and more of a compendium of examples against which to compare our European experiences, experiments and ambitions" (p. 91).

Moving along to the "Lessons Drawn," editor Richard Rose, Professor of Politics at the University of Strathclyde, Glasgow, provides one of the most valuable – a lesson sophisticated in its understanding, not just of institutions, but of the people behind them. He is equally wise about the alleged "superiority" and possible transferability of institutions:

> The existence of differences in government does not mean that one model must be superior to the other. But it does provide a basis for assessing their contrasting assumptions and consequences. The conclusions are unlikely to lead to the transfer of institutions across the Atlantic, for the machinery of government is very sensitive to national influences. Comparison leads to greater understanding of the political culture that inhibits institutional transfer (p. 132).

Professor Rose is similarly persuasive in his analysis of what he sees as: (1) the tendency in the U.S. to rely on the courts to protect individual rights as against the greater reliance in the U.K. on the "good sense of the citizenry"; (2) the greater "electoral volatility" of American voters as compared with the greater party cohesion of their British counterparts; (3) American party weaknesses, "the American party system is unique [in] that other countries do not wish to copy it" (p. 143), in contrast to the British parties with their "undivided responsibility"; (4) European fears of anarchy versus American fears of centralization; and (5) the British belief in the "probity of their government" against the American "Wild West" approach.

In contrast, the essay by Tom Hooson, "Exporting the Pursuit of Happiness," comes out more American than the Americans, more Madison than the Avenue on which British-born Hooson received his training before becoming director of European operations for Benton & Bowles International, overseas branch of the American public relations firm. Hooson presents an impassioned, if less than persuasive, defense of American advertising practices, concluding that "quality is the key to success" in the competition for the American consumer dollar. His prime example? "Premium petrol!"

John Whale goes almost to the other extreme in his article, "Public Broadcasting Policy." Discussing the impact of the American TV model on the British, he pointedly and justifiably concludes: "Whether that particular lesson was a useful one is a matter of doubt" (p. 192). On the other hand, he does neglect the sometimes positive aspects of American commercial radio – exemplified at its best by the continuing Metropolitan Opera series and the earlier New York Philharmonic broadcasts.

Yet another negative "lesson" is that drawn by Hugh Heclo in "The Welfare State: The Costs of American Self-Sufficiency." American Heclo, presently a research associate at the Brookings Institution, concludes that in the case of welfare, *Americans* could and should learn from *Europeans*. He provides an effective analysis of the contrasting approach to relief in Britain and the U.S. – the active seeking out of eligibles in England as against the reliance on a "work ethic" to justify penurious and begrudging aid in America. The latter, he further notes, is compounded by a Federal system of grants "to create mutual exchanges of money without creating joint commitment to social purposes" (p. 278). Concludes Heclo:

> Models can serve as warnings as well as inspire emulation. The American experience with the welfare state seems to serve best as a cautionary tale. Rather than facilitating a balanced competition between collectivist and individualist conceptions of human freedom, the American ethic automatically prejudges the issue. The modern American Dream is not a social vision; it is not an expression of what people are to each other, but what they are to have for themselves (p. 280).

In summary, *Lessons from America,* as do many such collections, leaves the reader somewhat dissatisfied with the variable quality of its articles and with the limited depth inherent in its treatment. On the other hand, contributors like Banham, Heclo and Rose have demonstrated a sound approach for judging the desirability as well as the inevitability of the American model. And, if Banham is correct, as we suspect, about the value of a compendium of American experience for such purposes, this initial effort has served its function well. Indeed, it strongly suggests further inquiries along similar lines. Certainly there are many other areas where additional lessons, negative as well as positive, might be drawn by equally perceptive explorers. As it stands, Richard Rose has assembled, in *Lessons from America,* a worthwhile and challenging primer, not only for those in other lands, but for those in America seeking a better understanding of themselves through a searching assessment of the applicability of their own experience and their own institutions to other times and places.

KARL M. SCHMIDT

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Citizenship Between Elections: An Inquiry Into the Mobilizable American. By James N. Rosenau. (New York: The Free Press, 1974. Pp. xxxii, 526. $14.95.)

One of the original expositors of the concept of the attentive public here gives us two books in one – a general analysis of attentiveness, and a case study of the Americans for Democratic Action (ADA) which applies the concept. In the first he gives us the fruits of his considered thoughts on citizenship between elections, presenting in these four chapters the most sophisticated analysis of political attentiveness that I have seen. The sections describing the American attentive public are full of information and most valuable, but I was more interested in what appears to be a change in Professor Rosenau's view of the size of this attentive public. In *Public Opinion and Foreign Policy* (1961) he stated that this public represented 10 per cent of the population at its upper limit (p. 40). Yet in the book under review, the attentive seem to represent between 15 and 27 per cent (pp. 26–28) – or a much broader monitoring public than Rosenau has used before. The author also adds to the literature by distinguishing between attentives and the "mobilizables" who may be called upon actually to give time, energy, money, or other forms of active support. This second group is estimated to be much smaller – perhaps less than one per cent of the population (pp. 30, 33) – but it is even more critical, since these are the people that leaders can request to actually perform political acts for specific ends.

Rosenau also strongly emphasizes his belief that the attentive public has been growing over time. As proof of this he presents data which show the growing numbers of letters to the editor of *The New York Times,* the editor of the *New York Post,* Senator Philip Hart, and the White House from the 1950s to the early 1970s (p. 72). Yet the rate of mail on national and international issues to the White House – the most reliable source of data and the most important politically – did not change (pp. 80–81), and writing to newspapers and political authorities would seem (using Rosenau's criteria) to be mobilization rather than attention. As far as those paying attention is concerned, SRC data – cited by Rosenau only up to 1964 (p. 65) – show a very mixed pattern up to 1972, with some increases and some decreases but no discernible direction over time. Thus, I must agree with John Mueller, who is cited in this regard, and disagree with Rosenau: political attentiveness probably has not increased over time; but the mobilizables have increased slightly and have become more active.

The growth thesis is not critical to Rosenau's excellent analysis of attention and mobilization as central aspects of democratic citizenship, however. His definition of support as specific support and the consequent ignoring of diffuse support (p. 99) could be argued with, but the author's purpose is clearly to study the policy rather than more basic political processes. And for this his model is excellent – especially in its insistence that citizen participation between elections springs from a multiplicity of sources (chapter 4).

The remainder of the book is more difficult to evaluate. Although the author warns us that he realizes his "treatment of the empirical materials is likely to cause methodological purists to wince" (p. xxvii) – and even though I am very sympathetic to breaking methodological rules if reasons can be given – his use of a single group as the data base for all of the remaining generalizations about attentiveness and mobilizables is disturbing. This is especially so because the Americans for Democratic Action is an ideological rather than a functional group, and Key and Truman tell us that the former are atypical of pressure groups.

One example, of several, which suggests that the ideology of the particular group (the ADA) and time period chosen (1963–1964) for analysis are more crucial than the fact that the group consists of attentives and mobilizables is a finding that the attentives and especially mobilizables look to the president when they write letters to influence policy. Although this is called one of the "central characteristics" of mobilizables (p. 216), the facts that the two cases studied (the test ban treaty in 1963, and the civil rights legislation of 1964) were both presidential priorities, that both presidents were Democratic and liberal, that both were very aware of the ADA as an important reference group and that the ADA itself was very strongly president-oriented at the time certainly had a much greater effect on ADA members than on mobilizables or attentives generally. Even Rosenau's reservation that the findings might only apply to ideological groups generally (p. 195) seems doubtful, for an American Conservative Union counter-campaign at that time certainly would have been aimed at Congress.

Even so, there are many suggestive findings in this very well-analyzed book. The one on letter writing, indeed, seems itself valid when limited to ideological groups who are represented by a president who is favorably disposed to them. Other interesting findings show that mobilizables differ from attentives in the following ways: by religion, party identification, and income, but not by age, sex, race, region, education, or occupation; by being more active in writing letters to political authorities and in working in voluntary associations but not in writing letters to the media; and by being more interested and knowledgeable about the issue on which they were mobilized but not on issues generally.

As a case study of the ADA, then, the book is important. It is much more than a case study, however – although perhaps somewhat less general than the book itself suggests – because it presents many suggestive and imaginative hypotheses and at least eliminates some as not worthy of further research. The first part of the book especially rises above the details of a case study to give us an excellent description of the American attentive and mobilizable publics, their place in American politics and their critical role in democratic government.

DONALD J. DEVINE

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Program Budgeting for Planners: A Case Study of Appalachia with Projections through 1985. By Warren Rovetch and John J. Gaskie. (New York: Praeger Publishers, 1974. Pp. xiii, 136. $15.00.)

Scholars and administrators may have proclaimed the death and passing of Planning-Programming-Budgeting (PPBS) in the federal government, but Professors Rovetch and Gaskie apparently are unimpressed with the reported demise. They have produced a slim volume which attests to the tenacity of the concepts of program budgeting. On balance a somewhat disappointing book, this work, however, should not be dismissed perfunctorily by those pondering the dynamics of incremental versus program budgeting. In six chapters the authors chronicle the design of a program budget system for the Appalachian Regional Commission (ARC). The perspective throughout is that of the outside planning consultant – the authors having been the principal consultants involved. The logically consistent presentation commences with a conceptual chapter outlining a procedure for program budget development. (Define goals, define steps to achieve goals with timetables, determine costs for all steps, define yardsticks to measure performance.) The remaining chapters, occasionally repetitive, describe the formulation of the ARC programs following these steps. Primary attention is given to goals, programs, and costs with disappointingly little being devoted to yardsticks for performance measurement – probably the most crucial and theoretically significant aspect of program budgeting.

The exposition lacks much of the systems terminology of recent books on PPBS. Key terms include "performance information" rather than "output or impact data" and "socioeconomic conditions" rather than "need and demand indicators." The book does not suffer by sacrificing terminology, however, because its most redeeming quality is its success in getting beyond terminology to demonstrate the use of empirical theory and statistical data in program design and selection. In chapter 2, for example, the authors begin by describing (albeit too briefly) the process of goal selection and follow with analyses in two program areas – health and education. These analyses, while not comprehensive, show the consultants' use of field literature in selecting relevant theory and data to support program recommendations. Other analyses demonstrate techniques for use in areas where high quality data is lacking. Citations to source documents in selected policy areas are presented throughout. Given this focus, the case study should be of considerable interest to planners, administrators, and budget analysts facing operational problems in designing program budgets.

Redeeming qualities notwithstanding, political scientists and academic public administrators probably will be disappointed in this book for its errors of omission. The authors, having stated that budgets must be formulated "in a world that is political as well as economic" (p. 7), proceed to ignore the political context of their enterprise. Beyond this omission, there is a disturbing lack of theory throughout the book. Three paragraphs attacking "incrementalism" (p. 6), plus scattered comments in the concluding chapter provide what little theoretical perspective the book has to offer. Few political scientists would disapprove of attempts to "rationalize" traditional budgeting procedures; nevertheless, few will be satisfied with a glib dismissal of the functional aspects of incremental decision making so ably documented by Wildavsky, Lindblom, and others. Because the book lacks a theoretical perspective, the authors have missed the opportunity to make a significant contribution to the general literature on budgeting.

Costs to the authors in terms of missed opportunities and insights are seen throughout. In a section entitled "Local Input," the authors concede a failure by local officials in the Appalachian subregions to identify subregional programs. This shortcoming is ascribed to their failure to understand the program budgeting process – a failure in "timing and execution rather than in concept" (p. 30). The reader is skeptical. Noting the experience with PPBS in other jurisdictions, one wonders whether the failure is due at least partially to elitism in the decision-making process itself. A candid discussion of this possibility could have been most enlightening. Similarly, the authors concede that housing goals were selected in a manner inconsistent with the rational program budget model. Anticipating that need figures generated by the budget model would be excessive, ARC apparently forced the consultant to forego the normal analysis and propose a more "reasonable" figure. Thus political considerations apparently forced compromises in housing which call to mind the satisficing versus maximizing discussions of Herbert A. Simon. Rather than superficially treating these aspects of the decision-making process as aberrations to the rational procedure, the study could have been deepened by explaining them in terms of intergovernmental relations, organizational and political theory.

By failing to present governmental budgeting within the theoretical context of the larger public-policy-making process, the authors have missed the opportunity to transform a narrow case study into a work of more general interest.

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The Politics of Defense Analysis. By Ralph Sanders. (New York: Dunellen, 1974. Pp. 361. $15.00.)

The furor over Robert McNamara's introduction of systems analysis into the Defense Department's decision-making process has subsided, the clashes it aroused with the Services and Congress have died away, and now, though shorn of its privileged status, it is an accepted element in our determination of the requirements for national security. The time would therefore seem to be ripe for a searching look at the political implications of defense analysis (systems analysis applied to defense problems). Ralph Sanders's book is a brave attempt at such a look.

As Sanders (himself a defense analyst, once on McNamara's staff) notes, we need to distinguish between the techniques of systems analysis and the Office of Systems Analysis (eventually headed by an Assistant Secretary) established by McNamara. Definitions of systems analysis abound, and Sanders mentions a number of them. Some have described systems analysis as no more than the application of the scientific method to particular problems, or quantified or enlightened common sense aided by modern analytical methods. James R. Schlesinger has called it "a mechanism for sharpening intuitions of the decision-maker." Sanders's preference is to represent it as a "cluster of analytical approaches appropriate for the middle point in the spectrum of problems" (p. 13), spectrum meaning here the degree to which problems involve "technical or nontechnical factors." In practice, as he notes, it has come to mean much more than analytical approaches; it has been employed as a guide to action, an instrument for increasing the rational quality of decisions, a device for treating uncertainty as explicitly as possible, and an investigatory tool.

Surely no one could quarrel with such beneficial aims, yet systems analysis as practiced in the Office of the Secretary of Defense certainly led to serious political conflicts, several of which Sanders discusses. In particular, it aroused great opposition from the military and Congress. The shift in influence from military officers to civilian analysts, and clashes in personalities and rigid styles played their part, as Sanders points out. Military men tend to cover as many risks as possible and to place high value on experience; civilian analysts focus more on resource constraints that impede covering so many risks, and approach problems with abstract methods that seem unreal to many military men.

Congressional response to systems analysis in defense policy followed a similar pattern. A good many congressmen, especially supporters of the military, objected to systems analysis' getting into what they called a military role, e.g., responsibilities of the Joint Chiefs of Staff (JCS). They felt the real issues were being ignored, that the Services were being forced to waste tremendous amounts of time in replying to requests from Systems Analysis, and that Secretary McNamara paid too much attention to his analysts and too little to military advisors. Sanders himself notes that various devices incorporated by systems analysis gave OSD analysts the lead in defense planning (over the Services) and had the effect of transferring the strategic planning function from JCS to Systems Analysis. Congressmen, and some military commentators, came down particularly hard on the use of systems analysis for what they claimed was obstruction of congressional prerogatives in defense matters, abetting the centralization of power in the hands of the Secretary of Defense (as against the JCS and the Services), and for discouraging innovations in weapons development (MIRV was a notable exception) by studying problems to death.

After the departure of Secretary McNamara, a trend toward decentralization set in, and the office of Systems Analysis lost much of its influence in defense matters. According to Sanders, it was confined largely to a review function, and under Elliot Richardson, Systems Analysis was redesignated as the Office of Program Analysis and Evaluation, no longer to be headed by an assistant secretary. It is not likely, says Sanders, that in the foreseeable future OSD analysts will regain the "privileged status" they enjoyed under McNamara. Despite its decline, the influence of systems analysis remains. The Services have built up their own analytical capabilities and installed analysts at higher levels in their own organizations. The Planning, Programming, and Budgeting System (PPBS) has evoked special approval and is now employed in a number of civilian departments. Sanders feels that systems analysis won enough general endorsement to remain an integral part of our defense planning and management structure.

The "politics" of systems analysis or defense analysis would therefore seem to have died away. Its connection with the centralization of the Department of Defense was what made it so controversial, and its influence waned in the face of problems, such as Vietnam, which involved political and social factors not readily susceptible to quantitative analysis.

Sanders's book is loosely organized and tends to mix up systems analysis with discussions on how the management of national security policy as a whole has changed since 1961. Truisms abound, and there are long, extraneous portions, particularly on the Safeguard ABM hearings and on the subsequent investigation by the Operations Research Society of the testimony in these hearings. The index is especially poor; the "Whiz Kids" are referred to in the text on page 49 yet the index's first reference to them is for page 148; and Secretary Richardson, although mentioned in the text, fails to show up in the index at all.

In summary, Sanders presents a generally sympathetic, discursive account of defense analysis since the beginning of the McNamara period. As Sanders observes in his own summing up, systems analysis, although now treated with less reverence, ". . . has joined practical politics, law, administration, economics, science, and military affairs as one more ingredient to consider in public business" (p. 322). It is this fact which will make his book, despite its considerable limitations, an additional source for students of the American government and how it deals with national security problems.

HAROLD L. HITCHENS

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The American Judicial Process: Models and Approaches. By Charles Sheldon. (New York: Dodd-Mead & Co., 1974. Pp. 244. $5.95.)

Should Copernicus have sought common ground with Ptolemy? Einstein with Newton? "Behavioralists" with "Traditionalists"? Are political scientists, like "Buridan's ass," unable to choose between two bundles of hay? Such questions come to mind when one reads Charles Sheldon's final chapter in *The American Judicial Process: Models and Approaches.*

In calling for eclecticism in our study of the judicial process, Professor Sheldon retains some common distinctions. A behavioralist is a "scholar who researches the judicial process from the perspective of decision making, micro-groups, role, macro-groups, impact and systems models. . ." (p. 228). The traditionalist, on the other hand, studies doctrine and normative questions, and makes value judgments.

While the traditionalist looks to the past, and the behavioralist to the present, the postbehavioralist is future-oriented, judgmental, and reformist in outlook. He demands that we judge what we organize, analyze, predict, and explain. He urges that it is better, following Easton, "to be vague than nonrelevantly precise" (p. 229). The primary question the author sees in all this is not which approach shall prevail, but "how can all three work together to help understand and improve our judicial system and the society of which it is an important part?" (p. 229).

The postbehavioral revolution, in the author's view, can provide a meeting ground for all scholars of the judicial process through a suggested division of labor. The behavioralist must cooperate more with his colleagues – i.e., build on what others have done, share his knowledge with traditionalists, and suggest empirical ramifications of past and present for the future, bringing forth alternatives with all their implications. The traditionalist is to make his contribution by applying the models of the behavioralist to the past and by making normative judgments about the judicial process. He is also to suggest to us what is unacceptable and why. For, says Sheldon, if "judges are to allocate values in society, these allocations must be 'correct' and the values ought to be 'good' " (p. 232). The traditionalist, he thinks, possesses specialized training and experience for such an investigation of values. In each chapter, the author provides a discussion of a single model – decision making, micro-group, etc. – a critique of the model, and a survey of research studies which employ the particular approach.

My first conclusion is that this is a much-needed book by a broadminded author. Unlike others who have sought to survey the work in judicial process, Sheldon does not appear wedded to any of the competing perspectives on the subject. He is knowledgeable about an impressively wide spectrum of studies dealing with the judiciary. Indeed, I believe Sheldon's volume is the broadest survey of research on judicial processes currently in print. And it will have particular value for the student who wishes to know in general terms what has been done, who has done it, and what main concepts have organized the field. For this alone, the bottom line shows a fat profit for which the discipline is in the author's debt.

At the same time, this book, like all books, has its limitations. The author – in keeping with a "let every plant prosper" philosophy – seems to imply that all are flowers and none are weeds. Holding that each model has its unique and important contribution to make, he does not always keep his promise to identify that contribution. The device of the systematic critique provides an excellent opportunity for evaluation, and in some cases (notably the discussion of systems, role, and impact models), Professor Sheldon provides effective criticisms at a conceptual level. But when he moves to consider the work applying particular models, his critical faculties appear to weaken. In most cases he seems to accept too much and reject too little. The reader, therefore, will not leave the book with a very clear idea of the strengths and weaknesses of the research studies examined. I concede that the author's purpose may not have encompassed the goal I imply here. Be that as it may, a continuing need in the subfield is the identification of superior and inferior work and the juxtaposing of concepts and theories so that the poorer ones may be rejected and the better ones retained.

Professor Sheldon's normative stance, to say the least, is stimulating. Some readers will undoubtedly disagree with his assignment of research roles – and particularly with his view that the traditionalist is peculiarly equipped to serve as the conscience of the subfield. The author does not emphasize a crucial distinction here: a student of values is not necessarily equipped to determine in some superior fashion the values to be pursued by the judicial system. As for Easton's approved quip about relevance and ambiguity, the best that can be said is that it requires further discussion or qualification. Its validity, clearly, depends on many things. It assumes agreement regarding relevance and fails to distinguish present from future. Since we frequently cannot know whose determination of relevance is the appropriate determination for society, perhaps precision in our work should not be callously disregarded.

A division of labor, similarly, is not a panacea for the differences in our perspectives. Good research *should* drive out bad. Validated theory *should* triumph over theory found inconsistent with relevant data. Since such statements represent an epistemological posture, they are open to argument. But, surely, few would suggest that Copernicus should have sought common ground with Ptolemy – that either compromise or coexistence is preferred when "good" theory faces "bad." Behavioralists and traditionalists do have common ground, and there is a sense in which each has something to contribute. Yet, political scientists are not faced with the problem of Buridan's ass. The "hay" content of proposed additions to our knowledge is a variable. In evaluating such proposals, we should not hesitate to exercise our ability to choose between clover and straw. If we continue (unhappily in my view) to insist on the use of the behavioralist/traditionalist dichotomy, we should savor the contributions of each approach but reject the mistakes, the illegitimate claims, and the false leads, of which both are sometimes guilty. A division of labor will not suffice if what we seek is knowledge rather than "conflict avoidance."

S. SIDNEY ULMER

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International Relations and the Future of Ocean Space. Edited by Robert G. Wirsing. Studies in International Affairs No. 10. (Columbia: University of South Carolina Press, 1974. Pp. xi, 146. $5.95.)

The four essays contained in this volume were presented at a symposium convened in April 1972 by the University of South Carolina Institute of International Studies.

The purpose of Professor Knight's paper on "Special Domestic Interests and United States' Ocean Policy" was to identify and comment upon the role of "special domestic interests" (industries, institutions or other interest groups or lobbies concerned with the use of the oceans) in the formulation of United States' oceans policy. This is an essay which will interest not only the American observer, for, of course, every democratic state faces the same problem of how to consult and take account of the views of the many groups which have an interest in marine affairs.

One of the main elements in the American effort to ensure that the formation of government policy should be open to public debate and informed criticism by special interest groups was the establishment by the State

Department in 1972 of the Advisory Committee on the Law of the Sea, the main function of which was to advise the Chairman of the Government's Inter-Agency Law of the Sea Task Force. Although Professor Knight feels that the interests of the Department of Defense and the petroleum industry are better served than those of other lobbies, it certainly appears to a British reviewer that this machinery provides a model which might well be copied to advantage by other governments like that of the United Kingdom, whose policies in this field seem too often to be regarded as secrets of statecraft pending their formal presentation in international forums.

Dr. Friedheim, as his contribution to this volume, asks a question which must have been on the lips of many a delegate during the past few years: "A Law of the Sea Conference – Who Needs It?" Though somewhat dated now, this is still a thoughtful analysis of some of the defects of the United Nations as a suitable forum for the consideration of law-of-the-sea issues. It may still be necessary, if the Third United Nations Conference on the Law of the Sea fails, to ask, with Dr. Friedheim, where do we go from here? The author offers sound advice on the available alternatives for seeking solutions to ocean use problems by means other than a universal UN-sponsored law-making conference.

Professor Alexander's contribution is concerned with "New Approaches to Control of Ocean Resources." Given the breadth and complexity of the subject, it is not surprising that Professor Alexander could do little more than scratch the surface in a mere 15-page essay. It is nevertheless a useful summary of some of the principal problems for newcomers to this subject.

The final paper by Mr. Hull and Dr. Koers complements their earlier paper on an "Introduction to a Convention on the International Environment Protection Agency" (Law of the Sea Institute, University of Rhode Island, 1972). The object of their collaboration in this subsequent paper was to combine Mr. Hull's wide-ranging knowledge of marine affairs with Dr. Koer's legal expertise to ensure that the designers of "A Regime for World Ocean Pollution Control" were supplied with a sound understanding of the environmental, societal, political, economic, and financial facts on which workable legal arrangements must be based. This is an ambitious undertaking which could hardly be accomplished in thirty-three pages. The result is, however, a stimulating and original contribution to the literature on marine pollution.

The four essays are complemented by five documentary appendices.

E. D. BROWN

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Education, Unemployment, and Economic Growth. By Alan L. Sorkin. (Lexington, Mass.: Lexington Books, 1974. Pp. 186. $12.50.)

Numerous technical volumes in the fields of both labor and manpower economics have been published in recent years. These are often unintelligible to nonspecialists or even to specialists with less than quantitative skills. Alan Sorkin's study, while highly scientific and factual, is not a technical book and is intelligible and useful for anyone interested in the dynamics of the labor market and the politics of current manpower development programs.

The book contains eight chapters in which Professor Sorkin explains developmentally the anatomy of employment and unemployment in the American labor force. Chapter 1 discusses basic labor force concepts, offers criticisms and suggestions for broadening the data base (e.g., job vacancy statistics) and carries out in-depth studies of particular employment phenomena. The second chapter, while focusing on the job structure of the U.S. economy, offers a coherent analysis of changes in the labor force participation rates.

The next three chapters are concerned with the nature and composition of postwar unemployment. While carefully reviewing a number of major studies regarding the causes of excessive unemployment during the late 1950s and early 1960s, the author observes that "from 1950 to 1972 the relative unemployment rates of the less educated have not increased in comparison with their better educated counterparts" (p. 97). Moreover, using census data he argues that structural unemployment did not worsen from 1957 to 1972, and attributes the high jobless rate to nonstructural factors such as inadequate demand.

Chapter 6 focuses on the interrelationships among unemployment, economic growth, and poverty. Sorkin shows that the Phillips curve, which relates the percentage of unemployment to changes in either wages or price, has shifted upward in recent years, indicating that low rates of unemployment can be achieved only with more rapid inflation than previously. He fails to notice, however, that perhaps relative poverty and unemployment are on the increase in view of limited world resources and worldwide inflation.

The remaining chapters present an evaluation of various significant federal manpower programs for the disadvantaged as well as a consideration of labor force and employment projections for the year 1980.

It seems however, that Sorkin, like many other scholars, is not concerned with the limitation to economic growth or with problems associated with the new era of declining affluence and rapid inflation. Apparently the ethnocentric perspective of the U.S. scholars prevents them from delving deep into the realities of issues and controversies with regard to the relationships among capitalism, exploitation, poverty, and economic growth throughout the world. Perhaps, then, this narrow perspective is the fault of the community of scholars and not the author. Therefore, Sorkin's book is worth reading not only for those students and members of the general public with an interest in labor problems, but also those policy makers in business and government.

FARHAT GHAEM MAGHAMI

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One Last Chance: The Democratic Party, 1974–76. By John G. Stewart. (New York: Praeger Publishers, 1974. Pp. 208. $8.00, cloth; $3.50, paper.)

Early in 1968 I wrote an article for a popular magazine anticipating the likelihood of a realigning election. A few weeks after the article appeared, I received a note from a distinguished professor emeritus of political science arguing that such speculations were unnecessary since history had proved that realigning elections occur every thirty-six years. With all due respect, the issue remains unresolved. Plainly something of significance is happening in American politics. What it is, is not so clear. Indeed the elections of 1968 and 1972 make somewhat suspect the whole realigning-deviating-maintaining classification schema.

These last two elections have also produced a good shelf-and-a-half of interesting and readable books purporting to explain where our party system is going, why, and how. But neither the self-styled psephologists, nor the journalists, nor the active party professionals – like Mr. Stewart – have been able to develop anything approaching a theory to explain what it is that is going on. Stewart's book, like others of the genre, is fun to read, and it provides a useful set of references to recent poll materials, party documents, and campaign anecdotes. His chapter on the origins and effects of reforms in the Democratic National Convention, for example, is a first-rate and up-to-date survey. But *One Last Chance* is not as comprehensive as a text; it offers no new theories of any significance and no new data. Its significance, if it has any, is as a small piece of the intellectual history of our times.

And it is, by and large, a disappointing history. Clearly, we have more data about the American electorate than ever before. Yet we seem to know less. The half-life of recent books in the field is remarkably short (Whatever became of Scammon and Wattenberg's "social issue" and their housewife in Dayton?), and there are few tasks more frustrating than that of trying to prepare syllabi for courses in party politics.

Despite the book's title, and despite Mr. Stewart's background on the staffs of Hubert Humphrey and the Democratic National Committee, this is not simply a party tract. As the majority party in the United States, the problems of the Democrats are in many ways the problems of the party system as a whole. "This book," Stewart ambitiously begins, "suggests how the Democratic Party can provide the political leadership that America will need for the . . . closing decades of this century" (p. vii). To do so, he tells us, it is not enough for the Democrats to retain control of Congress, nor simply to remain in command of overwhelming pluralities in the Gallup poll. "Presidential power, for all its majesty, must be leavened by the clash of interests and values found on Capitol Hill and across the country" (p. 35), but, in the final analysis, it is the "Big Enchilada" that counts.

How, then, can the Democrats recapture the presidency? Stewart's answer, unfortunately, is not very clear. "The party's future cannot be seen as a choice between the advocates of the New Politics . . . or those who counsel a return to the familiar demography of the New Deal. The constituency that is potentially available to the Democrats in the 1970s is broader and more diverse than many persons imagined" (p. 140). Indeed it excludes almost no one. And the way to bring this massive constituency together is to "force a new issue: Which of the two major parties is better able to achieve a new identity between the activities of government and the needs of the people? Which of the new parties is better able to communicate this new identity in terms that the people can understand?" (p. 152).

Thus the issue for the Democrats, the

problem for our times, boils down to problems of imagery and technique. "This issue involves two principle imperatives . . . first, to restore the popular belief that government . . . can function competently and fairly; second, to convince people that government cares about what happens to average citizens" (p. 159). In showing such care, however, the government must avoid the pitfall of designing "appeals that are tied directly to the specific demands of discrete social and economic groups" (p. 161).

Here we find both *The End of Ideology* and *The End of Liberalism* pushed to their extremes. The Democrats must display responsiveness through the mastery of technique, without in any specific ways being responsive. They must prove that they can govern without coercing.

Perhaps this is what the present period of realignment is all about. Perhaps the manifest symbol of the arrival of the technological society is that our politics are being transformed into a debate over technique. But it is difficult to see what role this leaves for the Republicans. Will they be consigned to become less efficient? less responsive? less caring? less competent? Will the issues of the 1970s pit Roman Hruska's championship of mediocrity against a Democratic standard of excellence? Or are there perhaps some deeper currents that Stewart and others have not yet reached?

The problem with the literature on realigning elections, and with most books written during periods of realignment, is that it has, with a few notable exceptions, avoided deeper questions of political significance. The unanswered question is not whether realignments recur with the regularity of comets, nor whether Catholics, Jews, or one-eyed jacks shift this way or that; but rather what these shifts signify in terms of who gets what, when, and how. Stewart, unfortunately, is in good company when he ignores these deeper questions.

EDWARD SCHNEIER

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Chicano Revolt in a Texas Town. By John Staples Shockley. (Notre Dame, Ind.: University of Notre Dame Press, 1974. Pp. 314. $9.95, cloth; $3.95, paper.)

Crystal City, Texas, was the scene of a political upheaval of no small import in the 1960s. After decades of accepted Anglo rule, the majority Mexican-American population was aroused to electoral revolt and related confrontations. First, in the 1963 school board and city elections, Chicanos of known sympathy for the low-income racial majority won political office. The history of this headline-making turn of events in Crystal City is captured in painstaking detail by Professor Shockley.

Without imprisoning himself at the outset in any particular analytical framework, the author simply attempts to reconstruct chronologically a complex drama. He gives attention to a wide range of social, economic, and political factors which seem to explain, at face value, the course of the Crystal City story. At the end of each of the seven chapters is a summary analysis pointing up what Shockley considers to be crucial variables. As happens in so many narrative case studies, one is led inevitably, as is the author, to the conclusion that the Crystal City revolt was unique. No such confluence of social, economic, and political factors is likely to duplicate itself elsewhere in small-city America.

Perfect replicability aside, however, Shockley provides much ground for generalization about revolt, the birth of new political organizations, the economic clout of incumbents, the administrative capabilities of revolutionaries, the pathology of mass mobilization, the importance of outside assistance, the impact of a changed educational system, and the like.

Students of the Crane Brinton theory of revolution on a grand scale will recognize, for instance, many parallels in the Crystal City episode. The economy of the city and the surrounding Winter Garden Area of South Texas were stable and growing. The ruling Anglos were caught completely by surprise by the 1963 electoral upset. Inept leadership by the victorious, but equally surprised, rebels produced several years of chaos, counter-revolutionary successes, and retrenchment. Finally, a charismatic local leader emerged in the person of highly educated Jose Angel Gutiérrez to solidify the gains of the revolt and father the birth of La Raza Unida Party. Only the absence of significant violence during the upheaval separates Crystal City from the mainstream of genuine social revolutions.

Much subtle economic violence is evident, nevertheless, in the Crystal City revolt, as Professor Shockley discovered while interviewing the victims. Until its demise, the poll tax systematically deprived Mexican-Americans of suffrage. Chicanos elected to the city council in 1963 found themselves out of a job or in other economic trouble. The lame-duck Anglo council took care to keep the major employer in the area, the Del Monte Corporation, off the property tax rolls through an annexing ordinance exempting them for seven years. The economic vulnerability of the Chicano majority prompts amazement that their revolt could sustain itself at all. Shockley does not suggest that the Chicano seizure of political power turned the economic violence around or produced great hardships for the Anglos in general.

Anglo sensibilities were measurably shaken, though, by what the Chicanos were able to accomplish in capturing the school board. A militant new emphasis was given to the teaching of Spanish and the cultural heritage of Mexican-Americans. Anglo teachers were gradually replaced by Chicanos. The Anglo students were made acutely aware of their own minority status within the local school system. The drama of events surrounding this cultural metamorphosis shows Shockley at his best as a political historian.

Students of local politics and public administration will be especially drawn to the author's account of the first year of rule of La Raza Unida Party. After the 1969 elections, the Party was in a position to control both the school board and the city council. (As a result of the November, 1974 elections, they have since gained control of the county government as well.) Changes in personnel in the schools and the city are carefully noted by Shockley. The use made of federal grant monies, the dynamic role of Gutiérrez, and the building of a broad community organization pivoting around La Raza are key elements in the administrative side of the revolt.

Professor Shockley obviously went to great lengths to interview as many of the political participants as he could reach. Also, he utilized oral history interview materials produced by the Baylor University Program for Oral History. Most of the sixty-plus interviewees are listed in the substantial bibliography. The author considers himself a neutral observer, and extensive documentation of the text bears this out.

Perhaps this concern for objectivity furthers Shockley's inclination to down-play the typicality of Crystal City. Thwarted revolts in Uvalde, home of Texas Governor Dolph Briscoe, and other nearby towns with sizable Chicano populations are cited as evidence that Crystal City is an exceptional case. While La Raza Unida has received deserved statewide and national attention, it has been turned back so far in attempts to control local politics in the Southwest. Meanwhile, Gutiérrez has expanded his own base in Zavala County (since Shockley completed his manuscript) by taking office as county judge in January, 1975.

In the final chapter on Chicano control and the future, Shockley sheds his role as political historian. His extensive analysis of Chicano gains and Anglo failures allows him to point to the future with an accuracy of detail that cannot be disputed. He errs in only one important regard: he does not foresee Anglo corruption being replaced by Chicano corruption in local office. Such charges have grown serious since Shockley's prognosis.

Students of minority group politics will find this case study helpful in analyzing future developments among the Chicanos in Texas and the United States.

IRVING O. DAWSON

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Revenue Sharing: Legal and Policy Analysis. By Otto G. Stolze. (New York: Praeger Publishers, 1974. Pp. 303. $17.50.)

This book is a highly detailed – almost paragraph-by-paragraph – analysis of the State and Local Fiscal Assistance Act of 1972, of the rules and regulations promulgated by the Treasury Department for its implementation, and of a major court decision interpreting one of its provisions. While it lacks the drama of a major case study of the legislative process, it, nevertheless, reveals clearly the aspirations of the major interests which sought its passage and the numerous compromises painfully and tediously negotiated by White House, Treasury, and House, Senate, and Conference Committees.

On the one side, Professor Stolze tells us, were those in Congress and the Executive Branch who regarded revenue sharing as a device to enhance local responsibility, reduce federal control, and aid the ailing treasuries of state house, county seat, and city hall. These forces were critical of the detailed federal controls associated with categorical grants-in-aid through which the federal government shared its treasure and, particularly of the assumed distrust of state and local elected officials which federal practice implied. Mayors, governors, state legislators, city councilmen, and county commissioners rallied to the theme of local control and to the hope of eased tax burdens. On the other side were those who, for abundant reasons, distrusted state and local authorities when it came to aiding the unfortunate, rescuing the central city, and allowing all to share in the benefits without discrimination. Somewhere in the center were those who felt that under any circumstance the federal government had to ensure that allocated funds were not spent frivolously, nor without adequate reporting, auditing, and public understanding. The author shows clearly the resultant compromises, inconsistencies, and inadvertencies which resulted from those diverse pressures. The effort to minimize reporting to the

U.S. Treasury resulted in requiring virtually unworkable provisions for reporting to local constituents. The effort to avoid using the "windfall" for redecorating the mayor's office resulted in restricting the use to "ordinary and necessary maintenance, operation, and capital expenditures" – a phrase defying definition. The effort to identify and then account for the federal money resulted in the requirement of a special trust fund and the encouragement everywhere to substitute federal funds for local funds by "displacement" – making it impossible to state accurately what the federal funds were really used for.

The author recounts at length the struggle over the allocation formulae, the difficulties of definition of legitimate objects of expenditures, the efforts to prevent abandonment of antidiscrimination rules and prevailing-wage requirements of most existing federal grant programs, and the problems associated with attempting to insure "maintenance of tax effort" by the recipient units.

Revenue sharing in the form of this act does not redistribute resources in the U.S. It was not intended to, says the author. While it does seem to favor the central city over the suburbs, it does not promote much governmental consolidation.

Disenchantment with this approach began to set in with the threats of and the practice of impoundment by the Nixon administration forcing spending pressure back upon local tax resources and by several special revenue sharing measures which simply bundled up existing categorical aids in new packages adding no new money.

This book identifies virtually every controversy, explains clearly the attempted resolution and sets out the guidelines for evaluation when we have had more experience. It should be an invaluable guide to how to do better next time.

WILLIAM H. YOUNG

University of Wisconsin, Madison

How to Get Things Changed: A Handbook for Tackling Community Problems. By Bert Strauss and Mary E. Stowe. (New York: Doubleday & Co., Inc., 1974. Pp. 319. $8.95.)

Basically, this book is a "how to do it" manual for gaining maximum citizen input and involvement in identifying and searching for common solutions to community problems. Its authors divide the book into three sections covering what is done to involve citizens in issues of local concern, why it works, and how it is done. The first section deals with the formation and accomplishments of the Committee of 55, a local citizen "think-tank" and action group in Loudoun County, Virginia. This ongoing group has tackled such diverse problems as drug abuse, unemployment, and environmental issues, and utilized such methods as job fairs, educational television, and planning conferences involving all segments of the community. In the second section the authors explain their successful group-centered, nondirective approach initiated and sustained through the skill of a facilitator who guides without dominating the discussion, eliciting ideas from the group in an attempt to maximize its potential in jointly arriving at solutions and the means by which they can be implemented. Emphasis in this section is on techniques skillfully applied to maximizing local input in the solution of contentious problems, with the underlying assumption that eliciting the untapped potential of citizens gives them a sense of participation and involvement in the issues which affect their daily lives. Hence, they will presumably exhibit greater responsibility in seeking and working for ultimate solutions to local problems. In the third section, the authors explain how they broaden the base of participation in setting up planning groups, how they train facilitators, resource persons, reporters, and discussants, and how they organize, conduct, and evaluate planning conferences. The book is graphically illustrated with outlines, charts, step-by-step approaches, and check-lists, and is amply interspersed with examples of group interaction which include rare failures as well as varied accomplishments. The book also has a useful appendix which contains a potpourri of training guidelines, questionnaires, goals, sample conference programs and conference evaluations, and even includes such diverse topics as suggestions for cutting in on a long-winded speaker.

This practical volume's focus on interactional situations is understandable in light of the background of its coauthors: Bert Strauss has served as a management consultant to various groups, and Mary E. Stowe is a civic leader and former reporter. The value of the book lies not in the formulation of new methods to mobilize citizens in the processes of decision making and implementation, but rather in the collection and application of established group techniques and skills, such as role playing, and the use of discussion facilitators and organizing skills in mobilizing citizens in the search for solutions to festering community ills. Since the authors take nothing for granted in demonstrating how citizens are organized for participation, the book at times is redundant and too detailed; on balance, however, it is an interesting and informative book which can easily be read in several hours. While the book will undoubtedly be somewhat helpful to the academician in pinpointing ways to encourage and provide channels for effective citizen participation, it should be of even greater assistance to local community leaders who are actually trying to disseminate information and enhance communication and interaction both among local citizens, and between these citizens and policy-making bodies.

SHIRLEY E. OSTHOLM

York College, CUNY

The Supreme Court and "Political Questions": A Study in Judicial Evasion. By Philippa Strum. (University, Alabama: University of Alabama Press, 1974. Pp. 188. $7.50.)

Philippa Strum aptly titled this book when she added to its main title (The Supreme Court and "Political Questions") the words, *A Study in Judicial Evasion*. The author carefully, methodically, and often with meticulous detail, "dissects" case after case to show judicial evasion in deciding pressing social issues. The Supreme Court of the United States avoids decision making by reliance upon the "political question" technique. The reader might conclude from merely reading the book's title, and certainly after reading most of the chapters, that the author takes a critical and disapproving view of the Court's evasive technique. But this is not the case. She states in the concluding chapter that, "The political question device is justifiable because it preserves the Court by enabling it to withdraw from unequal contests and this permits it to eliminate the inevitably harmful effects of such combat" (p. 142).

The reader would have been better prepared for the author's surprising justification of and support for the Court's evasive scheme if the introductory chapter had contained some allusion to this point. It also would have been helpful if she had stated in the introduction what questions the study sought to resolve and the investigative methodology to be used.

To determine whether the author adequately supports the book's central proposition presupposes that the reader has a detailed knowledge of the numerous cases analyzed and discussed. The burden of proof rests, then, upon the persuasive argument and mass of information proffered by Professor Strum. The author does make a cogent and interesting brief for the assertion that the Supreme Court's use of the "political question" is dependent not only upon legal guidelines and its view of public policy, but upon the Court's estimation of two additional factors: "executive enforcement of judicial decisions, and the existence of a societal consensus" (p. 4).

Professor Strum convincingly shows that the Court is not neutral. Its decisions are political and involve questions of power. Why has the Court labelled only some questions "political?" The answer is the Court has determined that in these cases either its decision would not be enforced by the Executive Branch and it would, therefore, lose its prestige and esteem – indispensable to its power – or, if enforced, such enforcement would negate "an existing or emerging American 'truth' " (p. 4).

The author applies this thesis to four categories of cases. They are cases which involve the "republican form of government" clause in Article IV, Section 4 of the United States Constitution; reapportionment; laws and elections; the Executive. Much of the book is devoted to the Court's use of the Guaranty Clause as the constitutional phrase upon which to justify its determination of the "political question." This clause as discussed historically and as placed into its political environment, is utilized by the Court as a "necessary concomitant to separation of powers" (p. 35). Professor Strum rejects the claim that the Court uses "political questions" to avoid confrontation between the national government and state governments.

The supporting data on the first factor in the Court's employment of the "political question" are strong and convincing. That factor, as previously stated, is the problem of executive enforcement of the Court's decree. Supporting data for the second factor (enforcement would result in a negation of an American "truth") are not at all clear – possibly because no one can tell what constitutes "American Truth." Further, the author changes the statement of the thesis from the Court's search for "societal consensus" to the statement that enforcement would result in a negation of an American "truth." It is debatable whether lack of consensus is the same as negation of a truth.

The book repeatedly addresses itself to the proposition that the Court looks for social consensus on a policy question. When such consensus is present, the Court does not resort to judicial evasion via the "political question." The voting rights cases and the reapportionment cases illustrate this view. Presence of consensus makes executive enforcement an almost certainty.

Lack of clarity on this second factor weakens the conclusion: "When it [the Court] declares the presence of a political question, the Court tacitly admits that it cannot find, and therefore cannot ratify, a social consensus which does not violate basic American beliefs" (p. 142).

For reasons of cohesiveness and more persuasive argument, the question of "The Utility of the Political Question" as discussed in the concluding chapter, should have been raised earlier. The view that judicial evasion via "political questions" is justifiable becomes a lonely assertion at the end of the book, unsupported by the data cited within. It is a valid, poignant question to raise.

Notwithstanding the above, the book is a valuable addition to the literature. It is informative and clearly written, though sometimes it is a bit tedious. It is easy to understand, well researched and copiously documented.

LOIS B. MORELAND

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The Politics of Representation: The Democratic Convention of 1972. By Denis G. Sullivan, Jeffrey L. Pressman, Benjamin I. Page, and John J. Lyons. (New York: St. Martin's Press, 1974. Pp. vii, 152. $9.95, cloth; $3.95, paper.)

This book, produced by three political scientists and a student assistant then at Dartmouth College, is the result of a carefully planned study of the Democratic convention of 1972, with special attention to the problem of representation, or, in other words, the touchy problem of delegate selection and its relation to certain features of convention organization and operation. The method was somewhat similar, though on a much smaller scale and with a more limited purpose, to that of the American Political Science Association's five-volume study of the 1952 conventions; that is, a group of about twenty-five students, under the supervision of Denis Sullivan and colleagues, was sent to Miami, where they sat in on caucuses, state delegation meetings, and floor proceedings, and interviewed a random sample of 234 delegates on the basis of a previously prepared questionnaire (which is included in the book as Appendix B). The results of these observations and interviews, together with a CBS poll of all the delegates, were written up in five chapters, each one apparently individually prepared but drawn together by the common purpose, presumably under Sullivan's guidance and supervision, and without any appreciable distinction in style or quality.

The first chapter, by Sullivan himself, suggests four "dilemmas" in convention representation: (1) who is entitled to participate directly in the nominating process – actually the problem of party membership, which I examined twenty-five years ago but which persists without solution to this day; (2) how to represent outside interests (labor, women, blacks, elderly, youth, southerners), and yet maintain a modicum of party loyalty; (3) whether the emphasis should be on issues, which are divisive, or on party survival, requiring compromise; (4) how to achieve a "balance between issue purity and organizational power," or the conflict between the idealist, willing to sacrifice power for integrity, and the professional politician, willing to sacrifice integrity for power.

These dilemmas are examined in more detail in the succeeding chapters, with tables and figures to support the text. There are no clear solutions or conclusions, but the dilemmas or problems before the convention and the reactions of the delegates are pretty well stated. The authors find, for example, that the "outside" or special interests so carefully provided for by the McGovern-Fraser reforms were not much concerned about party loyalty, some of the delegates even insisting they were not Democrats at all. With respect to quotas, the authors argue that the issue is not the presence or absence of quotas, but which groups should be recognized, and how formally any quota requirement should be stated. There is a good account of the various caucuses (Black, Women's, Youth, Latin, Senior Citizens, Jewish) that met before and during the convention, with the observation that the most effective were, after all, the more traditional delegation caucuses, But I found most interesting, instructive, and realistic the chapter dealing with the party platform, often sneered at as insignificant window dressing. The authors describe in detail the drafting of the 1972 platform – the battles and compromises in its preparation between the issue purists and the professionals, the attitude of the delegates (they don't read the platform, but 71 per cent think it important), and its final adoption ("the Democratic convention adopted an entire preprinted draft platform, amending only two sentences"). There is some implication in the account that the hearings held to ensure citizen participation in the 1972 platform represented a "novel effort," although such hearings and other procedures were begun about fifty years ago and have been increasingly used since, but probably not as effectively as did the Democrats under Richard E. Neustadt.

The project was funded by the Ford Foundation and Dartmouth College, and is probably worth what it cost, for the book is, on the whole, a calm and detached analysis of the principal problems before that turbulent Democratic convention, and a useful addition to convention literature. There are three appendices – an explanation of the sampling procedure, the full text of the questionnaire, a chronology of convention events – and a good index.

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Patterns of Racial Discrimination. Edited by George M. von Furstenberg, Bennett Harrison, and Ann R. Horowitz. (Lexington, Mass.: Lexington Books, D.C. Heath and Co., 1974. *Vol. I: Housing,* pp. 220; *Vol. II: Employment and Income,* pp. 254. $32.00, set.)

Housing the Urban Poor: A Critical Evaluation of Federal Housing Policy. By Arthur P. Solomon. (Cambridge, Mass.: The MIT Press, 1974. Pp. 227. $12.00.)

With enviable precision, John Kain has calculated that 24,622 more blacks would have been employed in Chicago in 1956, and 9,113 more in Detroit in 1952, if they had not been excluded from the suburbs by residential segregation (pp. 8, 23–4). No wonder economists have been welcome in policy-making circles. The policy implications of Kain's analysis are perfectly clear – or would be perfectly clear were it not that the form of his regression equation, the quality of his data, and the interpretation of his findings are all disputed by other economists.

In *Patterns of Racial Discrimination,* edited by George M. von Furstenberg, Bennett Harrison, and Ann R. Horowitz, economists engage in refreshingly lively debates: a common format pits an eminent protagonist against his or her critics. The papers in this two-volume set, all previously unpublished, were originally presented at an OEO-funded conference on racial discrimination in housing and employment held at Indiana University on May 11, 1973, and later revised in the light of the other participants' criticisms and contributions. Since some summarize the literature and others present original data, they are useful to anyone interested in the current state of debate over certain public policy issues.

The first of two major issues examined in Volume I, devoted to racial discrimination in housing, is the impact of residential segregation on employment. As noted above, John Kain reiterates his view that the decentralization of urban jobs (especially those in manufacturing) combined with racial discrimination in suburban housing has increased black unemployment. He argues that opening suburban housing to central-city blacks is a necessary condition for increasing their job opportunities and improving their representation in suburban workplaces.

Bennett Harrison, leading spokesman for Kain's critics, summarizes a large number of studies that he believes support the conclusion that "racial discrimination in employment seems to be spacially uniquitous and should be dealt with directly – through affirmative action programs, legal sanctions, and probably quotas – rather than indirectly (as Kain proposes) through suburban open housing" (p. 22). He argues that although many jobs are being decentralized, many others (70 per cent requiring low skill levels, according to one study) are being created in central cities, but are going to suburban commuters. Thus, he recommends redistributing these jobs rather than dispersing the black central-city population. Duran Bell, Jr.'s and Peter Hutchinson's findings tend to support Harrison's conclusions: Bell finds increased employment discrimination against black women residing in the suburbs compared to those residing in the nonpoor neighborhoods of central cities; and Hutchinson finds that blacks, especially black women and youth and those with higher educational attainment, residing in suburban poverty areas are worse off than their counterparts in central-city poverty areas.

The second issue examined in Volume I is the impact of residential segregation on home values and housing consumption. Richard Muth and his critics argue the merits of equilibrium and disequilibrium theories of racial housing markets: whether the market alone or the market inhibited by racial discrimination and inelasticities in the supply of housing determines how much blacks pay. A defender of equilibrium theories, Richard Muth argues that residential segregation need not be caused by racial discrimination. He examines a number of studies and concludes: "Blacks clearly are more poorly housed on the average than whites are, a fact that I would attribute primarily to their lower incomes. While labor market discrimination and other forms of discrimination may thus contribute to the poorer housing of blacks, it does not appear to me that housing market discrimination does so to an appreciable degree" (p. 117). Disequilibrium theorist John Quigley disagrees, however, finding evidence of "systematic discrimination against black households in the housing market" (p. 122). And, on the basis of a survey of San Francisco Bay Area housing markets, Mahlon R. Straszheim concludes that "increases in black family incomes in the context of a continued segregated market of the type prevailing in 1965 will not solve the housing problems of blacks" (p. 163). But George M. von Furstenberg and R. Jeffrey Grant test the explanatory value of equilibrium and disequilibrium theories in the Pittsburgh housing market and find "moderate support for the equilibrium theory" (p. 176).

Victoria Lapham explores the problems associated with determining whether blacks pay more for housing. One of the most difficult is developing measures of neighborhood as well as housing quality so that it can be ascertained whether blacks pay more than whites for units of comparable quality. Edgar O. Olsen tests the hypotheses that poor and blacks pay more using New York City data and finds no support for them. Peter Mieszkowski summarizes the issues raised by the relationship between residential segregation and discrimination.

Volume II is devoted to employment and income. The first half examines some theories accounting for racial discrimination in employment and their policy implications. Focusing upon why population groups differ in productivity and why individuals of the same productivity receive different wages, Joseph E. Stiglitz reviews various theories accounting for income differentials between whites and blacks. Richard B. Freeman compares individual and collective behavior models of labor market discrimination. Barbara R. Bergmann describes a simple microsimulation model of the labor market which may give insight into relationships among black workers, white workers, and employers. Jerolyn R. Lyle discusses problems associated with the federal enforcement of civil rights laws and regulations. In a more theoretical section, Marcus Alexis argues that noneconomic factors should play a role in the analytic treatment of racial discrimination in employment; and Saul Pleeter examines the impact of uncertainty on the discriminatory behavior of employers. In separate articles, Barry R. Chiswick and Duran Bell, Jr., explore aspects of the employee discrimination hypothesis; and Bradley R. Schiller offers critical comments.

To the noneconomists, the second half of Volume II, examining trends in the differences between black and white incomes, may be the most interesting. Data are presented which show that the ratio of black to white incomes increased slightly in the period between 1964 and 1971 (and declined in 1972). With considerable caution, the inference is drawn that, even allowing for the favorable effects of a tight labor market, federal poverty and civil rights programs may have contributed to the increase. In one article, Ann R. Horowitz surveys the literature on black-white income differentials since 1947, and in another, she examines certain differences in white-nonwhite family income for the 1954–1971 period. Using both census and Social Security Administration data, Wayne Vroom analyzes changes in the relative income of nonwhites by sex, age, region, and position in the distribution of income for the 1948–1971 period.

Whereas the economists in the *Patterns of Racial Discrimination* volumes include policy recommendations among their conclusions, fitting policy recommendations to what the data appear to support, Arthur P. Solomon (also an economist) in *Housing the Urban Poor: A Critical Evaluation of Federal Housing Policy* begins with a listing of housing policy objectives and compares different methods of achieving them. Whereas the authors in the *Discrimination* volumes can draw only the most tentative conclusions about the impact of different federal programs, Solomon compares the effectiveness of specific federal programs along a number of dimensions.

Solomon sets out to prove that housing programs have relied excessively "on the subsidization of new construction, as opposed to the improved utilization of the existing housing stock through conservation, rehabilitation, or direct consumer subsidies" (p. 2). To clarify the issues, following Henry Aaron, he distinguishes between the "too little housing" problem and the "bad housing" problem, and argues that federal housing policies have been much more successful in dealing with the first than the second. National policy has mistakenly attempted to solve the "bad housing" problem by subsidizing the construction of new units instead of trying to reverse the process of housing and neighborhood disinvestment within the inner cities. "Unfortunately, even tragically, the isolated construction of individual housing projects has absolutely no impact on the wholesale deterioration of entire neighborhoods" (p. 7).

Proponents of the construction strategy have wrongly concluded that housing for low-income groups is in short supply, Solomon contends, whereas "it is the loss of housing demand associated with an absolute decline in population, as well as more concentrated poverty – rather than inadequate supply of housing stock – which is at the heart of the old central cities' housing problems" (p. 11). Thus, he advocates housing policies that promise to restore housing demand in central cities and permit the upgrading of residential services, including the quality of existing structures, neighborhood environments, and municipal services.

Assuming that resources available for housing are scarce, he examines the relative effectiveness of six existing housing programs (three subsidizing new construction and three using existing housing stock) in terms of the number of inadequately housed families moved into decent shelter for any given expenditure of public funds; the extent and duration of the U.S. Treasury's commitment which they entail; their impact upon vertical equity (the distribution of benefits among income classes) and horizontal equity (the distribution of benefits among those in comparable situations); their impact upon the creation of employment opportunities; their impact upon revenue collected from local property taxes; and their impact upon the urban environment. Each index of effectiveness is considered in a separate chapter; and tables summarize the data used to assess the costs and benefits of each approach. In the last chapter, Solomon sums up his findings and advocates shifting federal housing policy from production subsidies to consumer subsidies in the form of housing allowances or income maintenance.

Political scientists curious about how economists approach an understanding of poverty and racism should examine these volumes. They will learn that a need exists for institutional analyses as well as statistical manipulations of aggregate data.

JUDITH V. MAY

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Management Science in Federal Agencies. By Michael J. White. (Lexington, Mass.: D.C. Heath and Company, 1975. Pp. 111. $12.50.)

There is no question that in our age there is a good deal of commotion over the manner in which our federal agencies function. To many it seems that we live in an age of moronic decision making. Professor White characterizes current administrative and decision-making practices "as being completely subjective, irrational, and superannuated" (p. 2), and for that reason, he asserts that management scientists can make decision making more objective and more systematic by employing scientific methodologies, principally mathematics and statistics. In his words, "management science is also a base from which a policy science might be constructed" (p. 1).

Professor White, then, like many of his colleagues in the profession, laments the crisis of identity which causes public administrators to question their subject matter and methods. In an effort to grapple with this problem, White ambitiously examines the phenomenon of adopting management science activities in government agencies – viewing the concept of the adoption process as a critical variable to the idea of diffusion – to illustrate the difficulties attending the diffusion of sociotechnical innovations.

Initially basing his comparative field investigation of forty-six government agencies over a five-year span on the descriptive Radnor-Rubenstein phase model, Professor White constructs with considerable methodological elegance his own complex variation of the adoption-diffusion paradigm. One of his conclusions is that in any attempt to infuse sociotechnical innovations into complex organizations through revolutionary processes – where the innovators act aggressively on the adopting agency – the effort will encounter skepticism, inertia, and opposition, illustrating once again the power of bureaucratic preconceptions and the structural barriers to innovation. After all, the introduction of innovation means big changes in the plans, operating procedures, and responsibilities of people in these organizations. Indeed, perhaps the most compelling reason for the demise of PPBS as a sociotechnical innovation was that the budgeters, unimpressed by what they viewed as meretricious claims of analytical rigor, refused to allow the PPB'ers in; the PPB'ers did not know how to break down the resistance, notwithstanding the enthusiastic support on behalf of program budgeting by top level management.

In treating the development and the various patterns of adoption of management science in federal agencies, White's doctoral study scrupulously documents the complex experiences and diversity of the data collection and analytic methods he employed to study the diffusion process accompanying sociotechnical innovation. Although White's systematic analysis is admirable, he is on somewhat more tenuous ground when he disparages those who have criticized management science. To be sure, any skeptical social scientist who delves into the application of the management sciences will discover numerous abuses committed primarily by those who, mesmerized by the aura of the scientific method, forget that its techniques are merely tools; and while one may exert energy and effort in the search for hard data, there remains at the same time the obligation to guard against the tyranny of technique which may be exercised at the expense of wisdom and good sense. Indeed, it is a commonly shared view that the phrase "scientific method" has in recent years been frequently used to invoke authority whose features have been worn away by the touch of many untutored hands.

If Professor White asserts that management science should form a basis for participation in public policy making with its concerns with unpredictable, atypical, and mostly unquantifiable phenomena, then it behooves him and the proponents of the system to be skeptical of the methods employed and the data collected and to be ever mindful that the units of analysis addressed should not be restricted only to such goals as "improved standards of living" with their precise units of measurement, but similarly to the more ambiguous social policy of improving the "quality of life."

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Freedom for Sale: A National Study of Pretrial Release. By Paul B. Wice. (Lexington, Mass.: Lexington Books, 1974. Pp. 212. $14.00.)

Freedom for Sale purports to be both a description of bail administration in the U.S. and an evaluation of bail reform efforts. It fails on both counts.

Professor Wice attempts to study the administration of bail in thirty-six cities with bail reform programs and an equal number without such programs. He spent about a week conducting personal interviews and observing courts and jails in each of eleven of these cities. The bulk of the study, however, is based on the questionnaires sent to informants in the seventy-two cities. These respondents *estimated* the length of time a defendant is held from arrest to trial, the influential factors in the judge's bail decision, bail forfeiture rates, and the percentage of eligible defendants released on bail.

Standing alone, such survey data have limited validity in studying anything so complex and chaotic as local courts. Wice's failure to corroborate his findings by integrating his different data sources makes matters even worse. Two of many examples: On the basis of his observations of Chicago and Los Angeles, Wice argues that active interest groups are important in achieving effective bail reform. His other data show, however, that these two cities have relatively *ineffective* bail reform programs. The other example, which more basically questions Wice's approach, is that the multiple measure-validity problem destroys his use of release rates, a crucial measure in determining a city's commitment to bail reform.

Wice defines release rates as the ratio of the number of defendants released on their own recognizance (ROR) to the total number recommended for ROR. The problem is that different data sources suggest very different rates. The release rate in the cities Wice visited was 11.5 per cent, but this conceals a disparity. Among the visited cities, those that kept official statistics on release rates had a rate that was twice as high as the remaining observed cities. But the release rate for all seventy-two cities, a rate based on informants' estimates, is 21 per cent of *all* arraigned defendants, not just of those recommended for ROR. To make matters worse, in appendix E, the rate for the District of Columbia is given as 66 per cent, though earlier Wice reports it as 31.4 per cent.

There are a raft of careless mistakes. Wice's misinterpretations of Table 2.2 exaggerate the importance of the seriousness of the charge as a pretrial release criterion. Tables 4.2, 4.3, 4.5, and 4.7 should be tabulated across rows rather than down the columns. Retabulation shows that other findings are exaggerated or should be questioned in light of other difficulties. An example of the latter is Table 4.3, where only 28 per cent of the cities have bail forfeiture rates below the national average. On the basis of Wice's data, the percentage should be much closer to half.

With this shaky foundation it is not surprising that Wice can offer few evaluative insights about bail reform. Despite his contention, he explodes no shibboleths. He could, however, go a long way toward creating some. Wice accepts hook, line, and sinker the bail-related programs that are euphemistically called "pretrial diversion." They are based on the assumption that delays in court will continue and that reformers must set up "rehabilitative" programs for those who are in jail because they cannot make bail or are ineligible for ROR. The ultimate is Project Crossroads, designed not simply for those who are forced to remain in jail but for young adults *who are eligible for ROR*. Instead of simply being released to await trial, these recipients of Crossroads' largess are released in the hands of counselors who advise them about employment and other problems. If all goes well before trial, the counselor recommends that charges be dropped.

Here we have the possible emergence of a depressingly common pattern of criminal justice reform. The state creates a problem and then attempts to deal with the problem without confronting it. It does so to the accompaniment of a rhetoric of rehabilitation that masks a great potential for arbitrary social control. These programs allow the state to intervene in the lives of individuals who are not yet guilty of anything. In short, they legitimate pretrial detention or a "rehabilitative" facsimile.

By now we should be suspicious of such programs' effectiveness, even if we approve of their goals. We know that there is an inherent tension between rehabilitation and coercion. We also know that one way that persons caught in this tension handle it is by faking change, manipulating the setting so that it looks as if rehabilitation has taken place.

There is no guarantee that this will happen; but Wice ought to display a whit of cynicism, especially since he offers virtually no evidence that such "diversion" succeeds even on its own terms, and since he does not even discuss the possibility that the programs are an affront to individual rights.

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Democrats and Progressives: The 1948 Presidential Election as a Test of Postwar Liberalism. By Allen Yarnell. (Berkeley: University of California Press, 1974. Pp. xiii, 155. $8.95.)

Professor Yarnell's central assumption is that a "conventional wisdom" exists concerning the impact of third parties on American party politics. He perceives this established view to be one which holds that third parties have a positive, i.e., liberal, influence on the behavior of either one or both of the two major parties and, therefore, a progressive impact on the outcome of presidential elections. He tests the validity of this "pluralistic notion" of politics by an examination of the 1948 presidential election. Yarnell's central question is: "Did Henry A. Wallace and his followers have a substantial impact on the Democrats?" (p. ix). His answer is that the Progressive party did *not* shift the Democratic party to the left during the campaign challenge and that the 1948 election was *not* a victory for liberalism. The party of Henry Wallace either had no impact or had a negative, i.e., conservative, impact on both the campaign strategy of the Democratic party as well as the election outcome. The left-wing "shift theory" of the "conventional wisdom" is, therefore, either wholly inaccurate or accurate in the wrong direction for the Progressive party's impact on the 1948 election. The general lesson which Yarnell would have us draw from his specific study is this: American party politics is not as pluralistic and flexible as we have been taught.

Yarnell grounds his thesis in a November 1947 memorandum written by Clark Clifford, then special counsel to President Harry Truman, in which the Democratic party election strategy is detailed. Thus "if the conventional wisdom regarding the impact of third parties . . . holds true, it should be possible to discern a shift within the Democratic party during the period when the Wallace Progressives challenged it" (p. 62). Yarnell's point is that the Democrats did not shift from their previously established foreign and domestic policy positions as a result of the Wallace challenge in 1948. The Republican party remained the focus of concern.

A policy of peaceful coexistence and detente with the Soviet Union rather than the Truman doctrine of containment and hostility was critical for Wallace. But the Democrats and liberal interest groups either ignored the issues raised by Wallace or smeared him as a "fellow traveler." The first postwar presidential election institutionalized the red-baiting of opponents and the McCarthy era was legitimated by "liberals" whose main interest was their own personal advancement. The domestic issues were labor policy and civil rights. Yarnell admits that a shift did occur in the domestic policy of the Democratic party. But the Progressives cannot receive the credit. Truman's veto of the Taft-Hartley bill in 1947 put the shaky New Deal Coalition back together again. This action meant that Wallace was virtually defeated before he even started. The fear of black defections to the Republican party rather than the arguments of the Progressives spurred the Democrats to respond to the plight of blacks. Moreover these domestic shifts were more rhetorical than substantive and they conserved a victory for the old order established by Franklin Roosevelt in 1932. In short, the Progressive party did not have a progressive impact on either the election or the outcome. The conventional wisdom concerning the impact of third parties on American party politics is unsound.

Yarnell's presentation of the darker side of prominent liberal spokesmen and the power plays among leading liberal groups is provocative. His description of the extensive presence of political intrigue and personality politics rather than an open and honest confrontation of issues is persuasive. His argument that liberals might really be conservatives in disguise and that people who raise critical questions are seen by those in power as part of the problem rather than as part of the solution also has much merit. But the argument that the Democratic approach to labor policy and to civil rights was more rhetorical than substantive is one which fails to give credit to the genuine regard for the plight of workers and blacks in 1948 as well as the presence of decency in the American tradition. To claim that the victory of 1948 was illiberal because the victory conserved the New Deal is a strained view of liberalism and a strange interpretation of political history. The reader also has to wait until the penultimate chapter to learn that for postwar liberalism "the central issue was whether or not non-Communists and Communist party members could work together in liberal organizations in the United States" (p. 87). How this brief treatment of the "critical issue" is related to the "conventional wisdom" is left unclear.

Yarnell has effectively demonstrated that low politics is an established part of the American scene. But his central assumption about the "conventional wisdom" of the "pluralistic notion" of politics is more his own contrivance than established reality. That "normal politics" occurred in 1948 and that liberals have a harder time dealing with the question of communism than do conservatives might be a novel insight and a shocking revelation to Yarnell personally and more evidence that there might be a tragic flaw in the American system. But such a demonstration does not undermine the pluralistic view. Demonstrating the pragmatic side of American politics and the basic consensus between conservatives and liberals, particularly in foreign affairs, has been a regular feature of the established literature. A particular interpretation of the effect of third parties is not central to the view that America is a pluralistic democracy.

The established literature carefully hedges rather than naively pontificates about the impact of third parties. Yarnell does not explain why elections in general and the 1948 election in particular contain empirical evidence for testing the conventional wisdom. To be sure, the 1948 election is mentioned in the literature as an example of how an established party can adopt a main plank of a third party and thus secure electoral victory. But there is no inevitability of absorption suggested. Moreover the literature focuses on the area where Yarnell's argument is weakest: the impact of the Progressives on the labor and civil rights positions of the Democrats. Neither does Yarnell explain how it is possible to discuss the 1948 election without considering the Dixiecrats. The existing literature does not ignore the Dixiecrats nor suggest that the impact of third parties is exclusively progressive. It depends on the political circumstances of the time. The literature points out that historical, institutional, and social factors have limited the national impact of third parties and that the way to measure the impact or shift is in terms of social and electoral behavior decades later. That the views of the Wallace Progressives had little impact on the election of 1948 but considerable impact on the Democratic party in 1972 is wholly consistent with the "pluralistic notion" of politics. Yarnell has, unintentionally, validated the major thrust of the established literature.

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Ben Gurion, State Builder: Principles and Pragmatism, 1948–1963. By Avraham Avi-hai. (Jerusalem: Keter Publishing House and New York: John Wiley & Sons, Inc., 1974. Pp. 354. $12.50.)

No understanding of modern Israel, especially in its early years, is possible without an awareness of the central role of David Ben Gurion. Avraham Avi-hai, formerly an official in the Israeli prime minister's office and now a professor, in this book examines three interrelated theses. First, "that effective national leadership stemmed from the pragmatic solution of major state problems and crises in accordance with the fundamental aims and principles of the leader" (p. 2). Here Avi-hai measures leadership by dual criteria: the perpetuation of the leader in his roles (prime minister, minister of defense, and party leader), and the advances made by the leader toward achieving his objectives. The second thesis is "that the state leader, particularly in a post-revolutionary situation, tends to maximize the power of state institutions and to minimize that of rival frameworks in order to achieve his aims of state-building" (p. 3). And the third is that "political systems impose limitations on the power of the individual leader. Conflicts between the leader and the system . . . lead to an eventual loss of will on the part of the leader and the loss of support on the part of his followers" (p. 4).

Avi-hai's methodology is simple but effective. He has culled Ben Gurion's voluminous speeches and articles, isolated his statements of principle and compared these with the end political result. Basically the book is divided into four parts. The first part consists of three chapters on the setting for state-building. A brief outline of Ben Gurion's biography is cast against the forces that affected Jews in Eastern Europe and the ferment in the Zionist movement. Ben Gurion's "World of Ideas" (the Bible, messianism, universalism and particularism, will – i.e., the power of the spirit – and national identity) are discussed, and the author gives a cursory sketch of Israel's political background and workings. Parts two and three treat Ben Gurion's attempts to maximize state power. Part two deals with four important domestic issues: the labor movement, the unique position of the religious parties and the educational systems, the defense establishment, and the policy toward the Arab minority in Israel. Part three discusses three critical areas of external relations: conduct toward neighboring Arab states, the acquisition of weapons, and conflicts with the Zionist establishment. In the fourth part, Avi-hai highlights the waning of Ben Gurion's pragmatism and charisma against the background of internal clashes in the political system. He concludes with a retrospective chapter in which he summarizes his thesis, reflects on Ben Gurion's charisma, and perfunctorily compares him with other leaders. The book is buttressed with copious relevant footnotes, citing English and Hebrew sources, biographical notes, and a useful glossary.

Avi-hai covers a wide range of basic themes in the major areas of Israeli public policy. His thesis is successfully presented and developed. Ben Gurion's basic ideology is analyzed carefully and judiciously, although the force of Ben Gurion's personality never emerges. The author is at his best when he is discussing external affairs; in discussing the complexities of Israeli internal affairs, he tends to gloss over the subtleties of the party system. Although the book was copyrighted in 1974 and touches on events through 1973, the chart of Knesset elections includes only the Sixth Knesset election of 1965 and ignores the elections of 1969. Perhaps it is because of his thematic organization, but the author repeats himself and has the disconcerting tendency of breaking the flow of the narrative by frequently referring the reader to other parts of the book. Originally written as a Ph.D. dissertation, the book is fundamentally of interest to specialists who will gladly welcome it to the large, and too frequently shoddy, literature on Israel. This is a good, substantial book.

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Guide to Hungarian Studies. By Elemer Bako. 2 Volumes. (Stanford, Calif.: Hoover Institution Press, Bibliographical Series 52, 1974. Pp. 1218. $35.00.)

Conceived as a source of information and a research tool for students of Hungarian history, this two-volume work – a hybrid of bibliography and reference – attempts to portray Hungary, its history, its people and its achievement as seen by foreigners and by Hungarians themselves. Of the nearly 5,000 entries, listing scholarly works, journals, pamphlets, reviews, reports, maps, including audio-visual aids, many come from German, English, French, and Latin sources.

This extensive and diverse material is organized into twenty subject-chapters, such as the "history of the Hungarian book" (I); "government and politics" (IX); "economics" (XI); "literature" (XIV). The two concluding chapters, "Hungarians Abroad" (XIX) and "Hungary and the United States" (XX), give a poignant documentary of the diaspora of modern time – the scattering of Hungarians around the four corners of the globe.

Each chapter is further subdivided according to whether the subject is treated in (a) special reference works; (b) journals and monographic series; (c) monographs; and (d) articles. The subdivision entries appear in the form as those in standard library cards, catalogs, and other bibliographical aids.

As with other bibliographical reference works, the *Guide* confronted its author with Hamlet-like questions: "To select or not to select." Professor Bako is forthright about his selections. "Some titles listed here might appear of secondary importance and not worthy of inclusion, but in such cases the criterion for selection was either its availability in North American libraries or the fact that I have had the opportunity to use it, or that its usefulness and timeliness have persisted" (p. xii). Given this threefold, highly personal rationale for selection, nonetheless one is compelled to question the relevance and informative value of some entries: e.g., that in 1945 "400 sound films were made in Hungary." The preceding entry informs us that in 1942 "thousands of acres in Eastern Hungary were destroyed by floods." One is left to conjecture the connection between floods and the movie industry.

There are also some erroneous assertions. For instance, we are told that in 1524 the "great scholar and reformer Erasmus developed an extensive correspondence with several prominent Hungarians" (p. 116). Erasmus was not a reformist; he recoiled from Luther's teachings. In 1521 Erasmus wrote: "I shall not oppose if they roast or boil Luther. The fall of one man is a small matter, but I am very much concerned with the public tranquility." In view of Erasmus's attitude to Luther, Mr. Bako should have told us who the "prominent Hungarians" were that Erasmus corresponded with. Were they humanists like himself, or sympathizers with Luther whom he disliked?

Even in the excellent chapter XIX, which contains one of the best bibliographical sources on "Hungarians Abroad" this writer has seen, by listing virtually all Hungarian language publications, from the Brazilian Presbyterian Church's denominational journal (p. 831) to the scholarly works published by Hungarian authors at Bolyai University in Rumania (p. 834), there are spurious entries like "Soviet Geography," published by the American Geographical Society. One is left to conjecture what links may exist between Soviet geographical literature and "Hungarians Abroad."

Such flaws in judgment and attempts at "padding" are minor, however, when judged against the scope and depth of the undertaking. To cite only one example of the latter, the author lists the work by Sandor Farkas (Utazas Eszak-Amerikaban – *Journey in North America* – published in Kolozsvar (Cluj) in 1834). Farkas was a contemporary of Tocqueville, and his insightful analysis of American democracy, still untranslated, not only complements that of Tocqueville but gains timely significance in light of the recent bicentennial.

The author's stilted style in the introductory chapters, chronicling Hungary's sociopolitical and cultural development from 896 A.D. to the present (pp. 3–147) makes demands on the reader's patience. Nonetheless, this guide deserves to be on the shelf of every library, and within the reach of everyone interested in Hungary.

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Village Life in South India. By Alan Beals. (Chicago: Aldine Publishing Company, 1974. Pp. 199. $7.50, cloth; $2.95, paper.)

The study under review carries a subtitle, "Cultural Design and Environmental Variation," raising a suspicion that the academic study of the rural life of developing regions is at last being itself adapted "ecologically" to the desperately ecological obsessions of the North American academic system. Yet such a view would be superficial and misleading. This is because Professor Beals does, in fact, construct skillfully a seemingly simple description and analysis of the village life, society, and culture of Mysore (now Karnataka State), which establish the usefulness of the ecological strategy in such studies. He demonstrates how land, men, and animals are interlocked to produce enduring structures and patterns.

While the author's assertions about the ecological wisdom and adaptability of the South Indian village may command broad validity for precolonial times, he seems to assume too easily their validity for postindependence years. In fact the weakest part of this work is its political aspect. Beals fails to give adequate attention to the politicizing forces and factors in the environment of the village. In offering a somewhat sketchy ecological version of "modernization" in the three Karnataka villages he has studied at first hand, Beals fails to relate ecology to some of the crucial evidence that he himself has dug up with regard to the growth of a rural class system and its inequalities. Instead of breaking down his ecological study into a complex analysis of a differential cost-benefit structure for unequal groups in the rural environment, Beals simply takes refuge in an ecological mysticism, invoking the oldest of the myths about village India – its alleged harmony. His last words are an indication of this, and they are "... the strands of the web are held together by the principles of South Indian world view – by the image of harmony, unity and right action that is Dharma" (p. 183). The reality of village life presented by Beals himself effectively contradicts this simplistic conclusion.

I am tempted to describe the Indian village community as a populistic community characterized by the following five features: (1) it is a peasant community which entails localized exchange relations and is served by the ideology of localism; (2) it displays a pluralism based on horizontal integration rather than hierarchic integration, emphasizing diversity, mutuality, and complementarity; (3) elite ideology is absent but not necessarily the elite as a structural component; (4) real but perpetually challenged and limited links exist with the world beyond the "populistic community"; and (5) modernization is an on-going process arising partly, but importantly, from the adaptive capacity of the existing institutional complex, a capacity which permits incorporation of "things modern" simultaneously with preservation of "things traditional" embodied in the core-culture of the community.

It is this concept of "populistic community" in India which challenges Beals's overworked concept, "the South Indian worldview" or "civilization." I have found that what happens is not a mere adaptation of a higher civilization to local needs, but a more complicated process in which so-called higher civilization itself continues to evolve through a set of historical interactions between the periphery, the intermediate regime and the center.

The great merit of this work is that it is lucid and forthright in its comments, and has been clearly inspired by a genuine concern for the people studied. Its ecological propositions are original and deserve to be debated widely.

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Economic Development in East-Central Europe in the 19th and 20th Centuries. By Ivan Berend and György Ránki. (New York: Columbia University Press, 1974. Pp. xiii, 402. $18.00.)

A major inadequacy in the academic training of most political scientists who have passed through our graduate programs during the past few decades has been the lack of attention to history. Even those scholars trained primarily as area specialists have tended to leave our graduate schools with little more than a brief exposure to the history of the geographic areas of their specialization. In addition, in most instances neither area specialists nor generalists in political science have more than a passing acquaintance with the significance of economic factors for the political process.

Economic Development in East-Central Europe in the 19th and 20th Centuries provides a partial remedy for this general lack of historical and economic background – at least for one area. The authors, Ivan T. Berend and György Ránki, are two well-known and respected Hungarian economic historians. The book, originally published in Hungarian in 1969, has been revised for the U.S. edition.

In virtually any undergraduate course on the politics of East-Central Europe students are informed that when the Communist regimes came to power after World War II most of the area was severely underdeveloped economically in comparison with Western Europe. Seldom, however, are students given any analysis of the causes of this backwardness, which continues to plague much of Eastern Europe almost thirty years later. Berend and Ránki set themselves the task of searching out the "main tendencies, the common features, and the special peculiarities of economic growth in order to delineate an East-Central European economic model" (p. viii). The core of their argument is that most of East-Central and Southeastern Europe was prevented – in large part by political factors such as external political domination – from entering the capitalist stage of development in both agriculture and industry until well into the nineteenth century. By this time Western Europe had already outstripped the economic growth of the East.

When capitalist development finally did come to East-Central and Southeastern Europe it followed patterns that differed significantly from those that had developed in the West. Agriculture was expanded largely to fill the needs of the export market to Western Europe. Foreign capital played an extremely important role in the industrialization of East-Central Europe and, in the Balkans in particular, capital investments were tied to efforts to influence the foreign policies of the recipient countries – e.g., Bulgaria, Rumania, and Serbia.

Berend and Ránki note that two relatively different economic growth patterns developed in the area commonly referred to in the U.S. as Eastern Europe. In the Western areas of the Austro-Hungarian empire – present-day Austria and portions of Czechoslovakia, Poland, Hungary, and northern Yugoslavia – industrialization had made a major impact on virtually all branches of the economy by 1900. In addition to coming much later than it had in the West, industrialization depended more heavily on foreign investments and permeated society less completely than in the West. In the Balkans the process of industrialization had only just begun by the turn of the twentieth century and had made almost no impact on the traditional sectors of the economy.

The authors point out that World War I destroyed the economic regions that had developed in the area during the previous century and disrupted existing trading patterns. Fantastic inflation rates in most of the new states, huge debts, and reparations payments hindered the economic rehabilitation of many of the states. But inflation also stimulated capital investments and industrial expansion. By the mid-'twenties increased protective tariffs resulted in a decrease of trade and the artificially stimulated growth in capital investments slowed appreciably. With the onset of the depression in the 1930s, export prices and markets were drastically cut and economic stagnation occurred.

The rise of the Nazi Germany and the tying of the economies of some of the East-European countries to the German economy provided a new, if short-lived, stimulus for economic growth. With the outbreak of World War II most of Eastern Europe was exploited to meet the demands of the German war economy. After the war, economic reconstruction and development occurred according to entirely different patterns with the introduction of Communist economic policies based on those that had evolved in the Soviet Union.

Berend and Ránki have produced an important and stimulating analysis of some of the causes for the failure of East European economic growth to keep pace with that of Western Europe after the seventeenth century. The study is based largely on a judicious use of archival materials. Although probably a disproportionate amount of the analysis focuses on the territory of the old Austro-Hungarian Empire, those portions dealing with the Balkans and non-Austrian Poland are also well documented.

Messrs. Berend and Ránki are to be commended both for their effort to encompass the general economic history of pre-World War II East-Central Europe within the confines of a single volume and, even more, for successfully accomplishing the task that they set themselves. *Economic Development in East Central Europe* is an important book, not only for the specialist interested primarily in Eastern Europe, but also for the student of comparative political and economic development.

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Building an Austrian Nation: The Political Integration of a Western State. By William T. Bluhm. (New Haven, Conn.: Yale University Press, 1973. Pp. 265. $9.75.)

Professor Bluhm's book deals with the phenomenon of a Western state that is still grappling with problems of political integration in the twentieth century. Why are the Austrians so late in developing a "national consciousness"? Why are they well on the way to developing one now?

When the Habsburg monarchy broke apart in 1918, the German-speaking inhabitants of the remnant of the multinational empire decided to join Germany. The idea that the Austrians were part of the German people in a cultural sense played an important role in the decision, although there were also economic and political reasons for it. The Allies objected to the "Anschluss," and the Austrians had to constitute themselves a separate state, although they doubted its viability. This "state that nobody wanted," beset by economic crises and torn by internal strife, was a poor focal point for the creation of an Austrian national consciousness. In addition, the political and intellectual elites were unable to perform the task. Defining "nation" mainly in cultural terms, they were themselves ambivalent on the question whether Austrians were Germans or a separate nation. The Anschluss of 1938 seemed to answer the question for them.

The decision of the Allies in World War II to reconstitute Austria after Germany's defeat provided the possibility for a new start. Pragmatic leaders defused social and ideological conflicts by portioning out political power among the two main contending camps in a "Grand Coalition," underpinned by a new spirit of "contractarianism" (p. 63). They led the country to economic recovery and political independence based on permanent neutrality. They also promoted a national consciousness, which was actually a compromise between the positions of various groups. The most sensitive question – whether Austrians are a separate nation in a cultural sense – was repressed. The idea that they are, at least, a "political" nation was propagated. The people, disenchanted by their experiences with Germany after the Anschluss, feeling the need for solidarity in the face of the long-drawn-out four-power occupation, and satisfied with the state's economic performance, were ready to accept that idea.

This is the story that Professor Bluhm tells in great and often engrossing detail, based largely on the writings of politicians and intellectuals and on open-ended elite interviews. Although the book deals with an aspect of political culture, it fails to investigate the role of political socialization and, especially, of the schools in the creation of a national consciousness; only one chapter taps the national consciousness in the population at large by means of polls, and there the interpretation of some of the data is open to question; conservative viewpoints are relatively overemphasized; and while the integration of the Croatian minority in Burgenland is discussed, the more problematical integration of the Slovenian minority in Carinthia is barely mentioned.

There are also some factual errors. Thus, "National Council" and "National Library" did not have "the national idea incorporated into their names" after 1945 (p. 55), but had been known as such since 1918; Bruno Kreisky did not emigrate before the Anschluss (p. 118), but was imprisoned by the Gestapo; and "Action 20" was not the economic program of the People's Party of 1966 (p. 121), but a braintrust that was to bring a "symbiosis of politics and science" to bear on the discussion of a broad range of issues.

Still, as a somewhat impressionistic interpretation of its subject, the book has considerable merit. It is written with a good deal of empathy, it abounds in interesting vignettes of Austrian politicians and literati, and the author's personal observations provide often valuable insights. Professor Bluhm claims that the book also tests a number of hypotheses advanced in the social science literature about nation building and political development. But the short introduction, the references to the literature interspersed here and there, and the last four pages of the concluding chapter hardly justify that claim. His analogy between nation-building in the Third World and in Austria after 1918 (p. 1) neglects the crucial fact that the peoples of the new states had demanded and obtained a grant of independence from their colonial masters, while the Austrians had been the ruling group in the Habsburg empire. In addition, Professor Bluhm's analogy is supported tenuously by a few quotations, taken out of context without any elaboration or qualification to make them fit the case at hand. Thus the quotations from Amitai Etzioni's *Political Unification* (pp. 8 and 254) deal with the question whether successful political unification requires cultural homogeneity of the participating units. This question, so relevant to the integration of the multinational Habsburg empire, has little relevance for the Austrian republics of 1918 or 1945. For, in spite of Professor Bluhm's contention (pp. 3 and 4), the cultural and linguistic differences between Salzburgers and Vorarlbergers are hardly the equivalent of the differences between Ibo and Yoruba. Finally, as the book itself demonstrates, the problem of political integration in the Austrian republic was not so much one of unifying disparate cultural subunits as one of establishing a consensus – or at least a *modus vivendi* – among rival social groups with antagonistic ideologies. For these reasons, the comparisons with nation building in the Third World are of questionable validity.

The comparative framework rests rather lightly on the main body of the book. If we disregard it, what remains is – aside from the reservations mentioned earlier – an interesting and useful addition to the scant English language literature on the politics of postwar Austria.

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Radical Politics in South Asia. Edited by Paul R. Brass and Marcus F. Franda. (Cambridge, Mass.: The MIT Press, 1973. Pp. 449. $15.00.)

Radical parties and movements in South Asia have a long history, and they have proliferated in the new nations that emerged in 1947 and 1948. Even though they have had surprisingly little success on the national level, they have had a great impact on political thinking and orientation; and, in general, they have had an influence far out of keeping with their limited political successes.

The present volume has the distinction of being "the first book-length comparative study of regional radical movements in the major centers of radical political strength in the South Asian countries of India, Bangladesh, and Ceylon" (p. 3). It is indeed surprising that Indian and foreign scholars have paid so little attention to this subject. Perhaps this is due mainly to the dual considerations that the subject is a sensitive one, and that reliable information is hard to obtain.

The study is deliberately limited in several important respects. In the first place, it concentrates on "the indigenous roots and aspects of radical politics rather than . . . the international aspects" (p. 4). This approach is very useful, especially since most studies of Communist parties in South Asia have given perhaps undue attention to the international linkages of these parties; but any balanced analysis of communism in South Asia must consider international ties as well as domestic roots. Secondly, the study quite properly emphasizes the strong regional character and bases of radical politics. For this regional approach to be comprehensive, however, consideration would have to be given to radical politics in other parts of South Asia, including all of the other states of India and West Pakistan (now Pakistan). Third, the focus is on radical political parties, the most conspicuous agencies of radicalism in South Asia. Much less attention is given to mass movements, including the recent more revolutionary movements, which may in fact prove to be the most significant and most deep-rooted aspects of South Asian radicalism.

The introductory chapter by Paul Brass, which occupies more than one third of the entire volume, presents these major themes and approaches. It also discusses such important topics as the types of leadership, the locus of decision-making authority, and the support bases of Communist and Socialist parties, as well as the bewildering phenomena of factionalism and party splits, usually centering less on ideological issues than on "the strategy and tactics to be followed toward the parties, authorities, and classes in power" (p. 32) and "the choice of appropriate coalition partners" (p. 46). Professor Brass is probably correct in predicting that "the ultimate impact" of the parties of the radical Left in South Asia "is likely to be only gradualist and reformist despite all the commotion" (p. 116). The same prediction cannot safely be made of the more revolutionary and less identifiable movements which have surfaced to some extent in South Asia in recent years.

Communists have been active in Kerala for many years, and generally they have fared better than the Congress Party. Professor Hardgrave presents a summary of the origin and fortunes of the CPI in Kerala, with particular emphasis on its strength in trade unions and in certain caste and communal associations, the regional diversity of its support, its alternating strategies "reflecting the twists and turns of its ideological line between 'revisionism and dogmatism' " (pp. 144–5), the rivalry between the CPI and CPM since the split in 1964, the formation of a United Front government, headed by a CPI chief minister, after the 1970 elections, which the Congress eventually joined – "the first time in India that the Congress had shared power with the Communists" (p. 179) – and the continuing mass support of the CPM, in spite of extreme factionalism and relative isolation. Professor Hardgrave makes only passing mention of the 1957 elections in Kerala, which brought the Communists to power – an unprecedented event in any political subdivision of any non-Communist country. The impact of the Communists on the politics and society of Kerala has obviously been greater than their electoral fortunes would suggest. In fact, as Professor Hardgrave points out, "the strength of the Communist movement in Kerala and the competing interaction between the CPI and the CPM have served to radicalize the state within a parliamentary framework" (p. 180). And there are other kinds of radical politics – Professor Hardgrave mentions six of them, of a generally Naxalite complexion (i.e., pro-Peking terrorist groups) – which have suffered serious reverses but may surface more conspicuously in the future.

Even though they seem to have been largely destroyed, as a result of their own factionalism, their resort to purposeless violence, and the vigorous and ruthless Congress-led counter-attacks, the Naxalites in West Bengal have spearheaded some of the most widely publicized of all radical movements in South Asia. Surprisingly, Professor Franda, author of an excellent study of *Radical Politics in West Bengal*, makes only a few passing references to this kind of radicalism. He does, however, provide a general survey of the course of Bengali extremism, led by the predominantly Hindi *bhadralok*, "a group which maintained their dominant position for a number of centuries before the British invasion" (p. 187) and has provided much of the leadership for the CPM, the most successful of all the non-Congress parties in West Bengal. The CPM has been sharply divided over the strategies it should pursue. It has at times been dominant in state governments, and it has retained wide mass support. More recently, the CPM has alienated substantial portions of the electorate by its policies toward both the Congress and the Naxalites, and it suffered a major electoral set-back in the 1972 elections. It seems to be faced with the possibility of another split, with mounting pressures on its members "to either defect from the party or to become more militantly attached to a non-electoral political style" (p. 219).

No one has better described the distinctive characteristics of the Communist movement in Andhra Pradesh than Mohan Ram, a well-known Marxist journalist. This movement has "found itself increasingly at odds with the rest of the party and with the Cominform" (p. 282). The famous Telengana movement (1946–1951) was one of the most dramatic episodes in the flare-up of Communist violence in several Asian states during the early postwar years. At that time the Andhra CPI invoked Mao Tse-tung's "New Democracy" and stimulated "perhaps the first recorded debate about the legitimacy of Mao Tse-tung's teachings and their place in Marxism-Leninism" (p. 282). After a period of "electoral communism," featured by substantial electoral successes in 1952 and electoral disaster in 1955, and after the split in the CPI in 1964, the main group of the Andhra Communists became increasingly alienated first from the CPI, then from the CPM, and finally from the CPML. Today the "Maoist stream" of Indian communism "comprises the CPML and the RCC (Revolutionary Communist Committee) in Andhra Pradesh, as well as a number of groups outside Andhra Pradesh that are presently supporting the RCC" (p. 318). Mohan Ram concludes that "In Andhra Pradesh the Maoist movement has immense potentialities" (p. 320) and that "The Andhra Maoist movement will retain its distinctive regional character, regardless of its national and international moorings" (p. 321).

In Bihar the Radical Socialists have been stronger than the Communists, and have been "the symbol of militantly radical politics" (p. 394). The SSP and the CPI "have represented two distinct kinds of approaches to radical politics in north India" (p. 331). They have "been institutionalized forces in the politics of Bihar, but nothing in the district patterns (which Professor Brass analyzes in considerable detail) suggests that their potential for growth in the immediate future is great" (p. 383). One wonders why a chapter on Bihar – the second longest in the book – was included in this volume. Perhaps it was decided to include some discussion of radical politics in at least one north Indian state. If so, Bihar was a logical choice. Professor Brass includes some interesting comments on alleged Naxalite activities in Bihar, but of course he wrote this chapter long before the emergence of another kind of radical movement in the state, associated with students and other young dissidents and blessed by Jayaprakash Narayan.

Radical politics in Pakistan were concentrated in East Pakistan. For years the East Pakistan Communist Party led a shadowy existence, working mainly through the Awami League and then through Maulana Bhashani's National Awami Party. In 1966 the EPCP split into several CPs – one pro-Moscow and several pro-Peking. The pro-Moscow CP is now working openly, and seems to be the only radical group of some importance in the open arenas of political life. "The appeal of this group among the masses was considerably heightened by their active participation in the liberation war and by the valuable support of the Soviet Union during the Bangladesh liberation struggle" (p. 276). Professor Tulakder Maniruzzaman of Rajshani University, in one of the most informative sections of his chapter, identifies five Maoist factions, and explains the apparent differences between them in some detail. During the 1971 crisis all of these factions "lost most of their cadres and also much of their goodwill among the people because of China's support for the West Pakistan military regime" (p. 276). At the moment they are weak and disorganized; but if conditions in Bangladesh continue to deteriorate, they may become much more active.

Ceylon is the only country where Trotskyist groups have achieved and retained a significant political role. Many Marxist groups have existed for years – by 1968, according to Kearney, there were no less than six – but the main division has been between the LSSP, the leading Trotskyist party, and the pro-Moscow CP. Both parties draw their support mostly from the southwest part of Ceylon, among the low-country Buddhist Sinhalese. They have both fought and cooperated with each other. Their representatives have played an active part in the Ceylon Parliament, and in the United Front government that assumed power in 1970. "Over the past two decades, the major Ceylonese Marxist parties appear to have become progressively more domesticated and pragmatic" (p. 439); but "The recent appearance of a new radicalism rooted in different social strata and revealing a markedly different perspective and style confounded an otherwise orderly picture of a highly sophisticated, intellectually inclined, increasingly domesticated Ceylonese Left" (p. 401). Since the surprise uprising of the young "Che Guevarists," led by the JVP, in April, 1971, things have not been the same in Ceylon (now Sri Lanka).

It is impossible to predict what the new radicalism that has emerged in India and Bangladesh, as well as in Sri Lanka, portends for the future; but if conditions in these countries deteriorate and if frustration grows – and there are all too many signs that this will indeed be "the wave of the future" – it is quite possible that the new radicalism in South Asia may turn out to be more important than the kinds of organized and thus far more significant "radical politics" that are described in this excellent volume.

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Politics and Development in Rural Mexico: A Study of Socio-Economic Modernization. By Manuel L. Carlos. (New York: Praeger Publishers, 1974. Pp. 144. $13.50.)

In Manuel Carlos's case study of *ejido* politics in the Rio Fuerte valley, the reader who is prepared to accept a heavy serving of jargon will be rewarded with an interesting analysis of local *ejido* government in a northwestern Mexican irrigation district. Having selected thirty-four representative *ejidos* throughout the region, Carlos employed the methods of participant observation, open-ended interviewing, the key-informant technique, and the key informant-centered survey as the principal data collection methods. This material, together with information gleaned from newspapers and government documents is used to construct a formal model of the political processes of the Rio Fuerte *ejidos*. Equipped with this model, Professor Carlos seeks to demonstrate "that the *ejido* communities have modern, associationally based, and competitive political systems that in many ways resemble those of fully developed societies" (p. 2). Furthermore, Carlos attempts to show that the Rio Fuerte *ejidos* have a pluralistic elite and differentiated political group structure and that these elites, groups, and political oppositions follow constitutional norms in the conduct of their political lives. For the author, "these political traits that the communities have acquired represent the most important outcome of the region's and its communities' modernization" (p. 2).

While we may not share the author's basic assumption that political modernization is an achievement more impressive or more important than economic modernization, we are indebted to Professor Carlos for clearly pointing out the link between political and economic modernization in the case of the Rio Fuerte *ejidos*. For, although one often wonders how so obvious a point could be overlooked, much of the literature on *ejido* development fails to draw a connection between the resource base available to a community of land recipients and the subsequent productivity of that *ejido* as an economic unit. Professor Carlos not only examines this relationship, but he is also concerned with understanding how the economic situation of the *ejido* tends to condition the social and political life of the community and determine the variety and type of political associations, the degree of competition, and other political characteristics that will take shape in each *ejido*.

In providing evidence of this relationship, Carlos strikes a blow at what he identifies as "the Banfield-Foster theory of political incapacity among rural villagers" (p. 55). The data gathered by Carlos show voluntary associations dedicated to community improvement in all thirty-four *ejidos* and, while in wealthier *ejidos* the variety and frequency of these voluntary activities are greater, "*ejidos* are poor for economic reasons, not because they have fewer interest groups, or a set of attitudes inhibiting the formation of collective good-oriented associations" (p. 68). Furthermore, Carlos's detailed study of opposition factions shows us that peasant behavior characterized by the Banfield-Foster school as resistance to community improvement prompted by an "image of limited good," may be better understood as the resistance of some peasants to community goals established by a rival group of peasants (p. 92).

Professor Carlos succeeds in drawing a picture of highly "modern" political entities, and he offers some revealing data on the relative levels of political development of the *ejidos* under study. He is not so successful, however, in providing us with the promised comparative analysis, nor in explaining why the Rio Fuerte *ejidos* have highly articulated political structures where others have not, and how these communities have managed to avoid strong-man rule, while in other areas of Mexico *ejidos* languish under the oppressive control of *caciques*. It's a safe guess that any comparison between the Rio Fuerte *ejidos* and those of Mexico's central valley would probably be a study in gross political contrasts. But in terms of specific comparative data supplied, Carlos does not even give us a sense of how the communities in his study stack up against the *ejidos* in other newly opened irrigation districts in northwestern and northern Mexico.

The factor Carlos claims has shaped the Rio Fuerte *ejidos* and, above all else, made them the modern political units that they are, is the relationship of these communities to a number of government agencies which have had a hand in opening up and developing this new agricultural region. Yet, although Carlos insists from the start that his study concerns the role of these agencies in the transformation of the region (p. 1), we get neither a detailed nor a particularly critical analysis of the relationship between the *ejido* communities and the federal apparatus concerned with regional development. For the most part Carlos confines himself to describing the tasks assigned by law to the various agencies, and in light of the rosy picture he paints, one can't help wishing that the Ejidal Bank and related government bureaucracies would serve peasants in other regions of Mexico with the same efficient dedication that Carlos's account has them exhibiting in the Rio Fuerte.

This failure to provide comparative data or to examine critically the relationship between the *ejidos* and the government is related to a general lack of material which would help to place the internal politics of the Rio Fuerte *ejidos* in the larger context of agrarian struggle in Mexico. It would, for example, be helpful to know how, why, and under what political pressures land was distributed in the Valley, the Commission for the Rio Fuerte was established in 1951, and massive government investment has continued to pour into the region. Unfortunately, the author does not provide this perspective on the land and labor struggles of the past, nor does he indicate the current lines along which agrarian conflict is drawn. Carlos does not explain, for example, that the Rio Fuerte is a region afflicted by *neo-latifundismo* (illegal land concentration typically in the hands of wealthy politicians and industrialists). And while we are told that the government agencies in the Rio Fuerte serve competing constituencies (the *ejidos* and the private commercial estates), we are not given any concrete idea how the *ejidatarios* come out in the shuffle. No data are supplied, for example, which would permit us to compare government investment in the region's *ejidos* with federal expenditure on the Rio Fuerte's private sector. There is no record of pesos of credit or units of irrigation water provided for *ejidos* vs. similar inputs made available to private agriculture. Thus, despite Carlos's assertion that it is crucial to understand the special relationship between the *ejido* and the government development agency, this relationship is not illuminated by the data provided. And lacking a broader comparative framework, at the end of the book we still do not know if the Rio Fuerte *ejido*, with its interest groups, elites, loyal opposition, and the rest is any more successful in pesos and centavo terms at winning concessions from the government, than a *cacique*-ridden politically "backward" *ejido* in Tlaxcala.

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Law and Justice: The Legal System in China, 2400 B.C.–1960 A.D. By Philip M. Chen. (New York: Dunellen Publishing Co., 1974. Pp. 234. $10.00.)

This is a brief book on a complex subject covering a long period of history. The author attempts to analyze law and justice in China between 2400 B.C. and 1960 A.D. – exactly 4,360 years, in less than 240 pages. The result is disappointing because of the author's glaring omission of data on Chinese law, his insensitivity to the assumptions underlying the discussion, and his apparent lack of understanding about modern approaches and their importance.

The book is divided into seven chapters. After an introduction emphasizing certain salient features of traditional Chinese law, two chapters are devoted to the ideology and role of law in imperial China. Beginning with chapter 4, Professor Chen spends some 150 pages on contemporary law up to 1960. The final chapter presents a conclusion. It is hard in all this to find a theme, since all the book tries to prove is that there is historical continuity between imperial and modern Chinese law, that law is not, and has never been, important in China, and that the Chinese Communist legal system is different from other systems in the world because it is Chinese.

Many points in the book raise questions. Chen starts with the view that "law occupies a position of crucial significance [in Chinese society]" (p. 1), but then he develops a contradictory theme that law is not important in China. A close examination of this contradiction and others is revealing. It appears that Chen's conclusion has derived from one of three arguments. The first line of argument is: because law was not important in traditional China, it is not important today. Obviously, this argument ignores the complexity of modern social problems which demand new concepts and mechanisms for solution. It assumes that what characterized China during imperial times must continue to do so today. The second line of argument is: because the current Chinese government, just as its imperial predecessor, is "a government of men, not by law." Assuming (and this is fallacious) that the Chinese Communists are not subject to legal restrictions, it is still a far cry from saying that the Chinese government does not operate under the guidance of regulations and rules. Indeed, the reader may have serious doubts about the dichotomy between "government by law" and "government by men," a concept which is no longer a useful analytical tool in the field of judicial behavior.

A third line of argument is: because the Communist Party has not promulgated legal codes, law is not important in China. But this argument is contradicted by the author's own contentions that, even with elaborate law codes, imperial China still did not emphasize law, and that in Communist China mediations guided by regulations have been a vital means for social control.

More serious is the book's lack of methodological sophistication. "My approach," Chen claims, "is highly eclectic . . . I have combined various functionalist approaches with more traditional kinds of analysis" (p. x). There is no trace of the use of "functionalist" approaches. And lack of methodology results in the presentation of unexamined assumptions and ambiguous concepts. In sum, the book breaks no new ground in research – there seems to be not a single quotation which has not appeared in other studies on Chinese law. It is high time that students of Chinese law start bringing the subject into the mainstreams of modern social sciences.

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Urban Policy and Political Conflict in Africa: A Study of the Ivory Coast. By Michael A. Cohen. (Chicago: The University of Chicago Press, 1974. Pp. 262. $14.75.)

Political scientists studying urban questions in Africa today face a sharp dilemma: political structures are increasingly the focus for the allocation of valued resources, but the "politics of allocation" are either hidden from view or surface too sporadically for systematic investigation. One way out of this dilemma is to shift our interests from such traditional concerns as decision making, official ideology, and the structure and function of political parties, to the substance and impact of government policy. Since most African countries are administrative states, such an approach has the advantage of concentrating on the crucial relationship between administration and its socioeconomic environment. Michael Cohen's study is one of the first monographs by a political scientist to focus on policy as a variable.

Based on fieldwork in the Ivory Coast in 1969–70, the analysis moves around two major themes of urban policy. The first is the extent of inequality in the burgeoning Ivory Coast economy. Much of this inequality, the author seems to argue, can be traced to urban policy decisions or patterns. In a carefully documented chapter, Professor Cohen shows how administrative allocation of urban building plots, the system of public housing, the structure of secondary education, employment and incomes policy, and the distribution of social services all tend to reinforce the already sharp distinction between the political-administrative elite and the rest of the population. While public policy thus tends to reflect "zero-sum" authority relations in the Ivory Coast, it has also brought in its wake various kinds of organized protest which must be managed by government.

The expression of this protest is the second theme of the book. An earlier stage of urbanization in the Ivory Coast (as elsewhere in Africa) led to the development of ethnic associations which were incorporated, in turn, by the nationalist party as its main local organizational units. Cohen feels that as long as this local party structure persists, it is incapable of articulating the neighborhood, occupational, and economic interests that have sharpened in response to the class character of government urban policy. The main evidence for this argument is the period of political tension in 1969 (arising out of both student unrest and general resentment against foreign workers) which brought on a lengthy series of meetings between Houphouet-Boigny, the president, and the major *couches sociales* of the nation. During this "Dialogue," which essentially bypassed the party, a number of specific demands were made relating to the key urban policy areas of land allocation, salaries, housing and rents, and unemployment by spokesmen for a whole variety of interest and occupational groups. The government's limited response to these demands was to make a number of cosmetic changes in policy (which, Cohen shows, still tended to benefit the administrative class disproportionately) and to co-opt some younger, more educated elements into administrative posts and higher party organs. The manner in which Houphouet-Boigny defused discontent at this time reflects his long-standing concern with limiting the forms and levels of political participation.

At the book's conclusion, the two themes of substance and procedure are brought together. If social injustice is the issue, argues Cohen, it can be softened only if institutional reform takes place and if more redistributive criteria are brought to bear on the evaluation of development programs. The most important institutional reforms proposed by the author involve the devolution of more power to localities, the recognition of voluntary associations based on economic interests, the reorganization of the party at the local level based on neighborhood units, and the mitigation of what is essentially one-man rule. If these changes were instituted, the book implies, the regime would probably consider more seriously the social costs and benefits of policies which have been, until now, evaluated primarily in terms of short-term economic criteria.

It is precisely at the point where the argument stops that the real analytical problems start to surface. Although Cohen does not push this point, we can assume that the present system of politics in the Ivory Coast serves the interests of the administrative-political elite. Can we realistically expect this elite to willingly share power and economic benefits with lower-income urban groups when "a zero-sum definition of public authority is the only possible governing strategy" (p. 7)? Suggestions for change *within* the present system must be predicated on a strategy that explicitly links incremental changes in the short-term interests of the governing elite with structural changes in the system which are in the long-term interests of the majority of the population. In the absence of such a strategy, both technical proposals (altering building and amenity standards for public housing projects, distributing more resources away from the bloated capital city area, shifting expenditures to social services rather than luxurious tourist facilities) and institutional changes have little chance of taking hold. What the author must know is that very similar proposals have been made time and time again by both "in house" and external advisers for any number of other African countries. These proposals have been successful only to the degree that they served the interests of the ruling elite – which is to say, rarely or minimally.

What, then, are the political preconditions to a major reorientation in urban policy? Cohen seems to be saying that the normal operation of urban policy will produce a proliferation of interest groups whose independent political influence will become significant for future policy choice. This social-structure tendency would be reinforced by the kinds of institutional changes he proposes. Quite apart from the (uncited) theoretical parallels between these proposals and, say, the Brzezinski/Huntington "convergence theory" or Apter's "development constitution," the evidence supporting a movement to a more "pluralist" politics is mostly hypothetical. Indeed, the absence of viable intermediate groups and structures of political participation seems to be one of the *main* characteristics of Third-World cities. While this feature is partly both a cause and consequence of urban policy, major changes depend on a transformation in the national (and international) political environment and go far beyond the concerns of this volume.

In spite of my reservations concerning the efficacy of proposals for change, this book is admirably researched and represents a welcome new direction in African political studies. It can be read with profit by anyone interested in Third-World urban problems.

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Sociology and Development. Edited by Emanuel de Kadt and Garith Williams. (London: Tavistock Publications and New York: Harper and Row, 1974. Pp. 374. $19.00, cloth; $6.75, paper.)

Symposia are inevitably of variable quality and are hard to review. This one is no exception. It covers a very wide area, ranging from Leninism to medical care, and from Argentinian slums to industrial protest in Nigeria. The general standard is high, the standpoint generally radical. De Kadt points out that sociologists for far too long had their "gaze fixed intently upon functionality and system maintenance" (p. 1), an attitude unlikely to provide one with valid insights into the problems of societies in the throes of rapid structural change. Partly for this reason, they have had far less impact in the development field than have economists. Yet the skills of sociologists are urgently needed, and in fact economists have tried to fill the gap with some crude "do-it-yourself sociology." Rejecting the conservative implications of functionalism, most of the authors represented here have grappled boldly with the revolutionary implications of the development process, and have much to say concerning the negative effects of the links with the industrialized "metropolis" countries. Sometimes they go rather too far in this direction, yet their approach is a fruitful and challenging one. This is a book to read.

The most stimulating contribution, in my view, is by Aidan Foster-Carter, who dissects "neo-Marxist approaches to development and underdevelopment," from what might be called a critical and independent Marxist standpoint. His paper can be wholeheartedly recommended to anyone interested in what various schools of Marxist scholars have to say, and is a masterpiece of compression; a good bibliography helps those who wish to probe further. The author is not averse to criticizing Marx himself, for instance for his negative attitude to the peasantry. He does not evade the problem that the proletariat in many developing countries is both small and relatively privileged. He boldly asserts the need for Marxists to analyze the underdevelopment and dependence-relationships of Communist-ruled countries too. He also takes ecological constraints seriously: "for Marxists, abundance is out; arguably it was always an ultimately meaningless notion, but henceforth 'scarcity' will have to be accepted as more than just a bugbear of bourgeois economics. . ." (p. 93). As Shakespeare said somewhere: "O excellent young man." More, please.

Foster-Carter and several other contributors go far in the direction of accepting Andre Gunder Frank's view of the role of Western industrial countries: that they "underdevelop the third world." I find this unconvincing as a generalization. Some historic experiences fit the model, but in many instances it seems totally misleading. Surely Western penetration of these societies was facilitated by the degree of under- or nondevelopment which already existed. Is it reasonable to imply that Senegal, Nigeria, Bolivia, Iran, would have developed but for the irruption of profit-seeking and land-grabbing Europeans? Were not the conflicts and contradictions which this irruption brought into these societies a powerful stimulus to social-economic change (and to political protest)? Was the Russian Empire "underdeveloped" by Western capitalism, or did it need Western capital and expertise in order to develop? Lenin, surely, would have regarded the second of these propositions as self-evident. All of which is perfectly consistent with the view that the resulting social tensions should or will have a revolutionary outcome.

Ian Carter attempts to use Frank's model to explain the underdevelopment of the Highlands of Scotland. The depopulation of the Highlands (i.e., the notorious Clearances) occurred because the clan-chiefs-turned-landlords found that it paid economically to substitute sheep for people. So one can blame it on the market. Yet the society which was destroyed was based, as Carter himself points out, on the clansmen being "foot soldiers in clan warfare"; stealing other clans' cattle was quite normal. While not for a moment wishing to justify the brutalities of the Clearances, one must ask was that a society which was likely to "develop" – *whatever* one means by "development"? More alarming still are Carter's criticisms of efforts to develop the Highlands today: thus to exploit local minerals is useless since it would "promote the underdevelopment of the area by reinforcing the satellization of the Highlands" (p. 302). What, then, can one do which would *not* be denounced by Carter? Must those who use the minerals, and those who provide the capital, necessarily reside in the Highlands? Is there to be no regional specialization? Are transport costs, and acidic boggy soils, irrelevant? What, in the author's view, should be the links between a region (or small country) and consumers or suppliers located outside?

Carter, following Frank, distinguishes *undeveloped* from *underdeveloped*, using the latter term as virtually synonymous with distortions engendered by links with the capitalist market. He seems to envisage a path from "undevelopment" to "development" which does not pass through "underdevelopment."

Some contributors clearly consider that this path has been indicated by Mao's China. Jack Gray's paper on Maoist agricultural strategy is solidly based on fact, and shows in some detail the vital importance of leadership and organization and mobilizing the masses. But Wertheim, in his very interesting contribution, sees in China a peasant revolution within which there is "emancipation from below," which he contrasts with India's "manipulation from above." Surely, this is too simple. There is a great deal of "manipulation from above" in China; but how does one achieve drastic change without it?

Wertheim does rightly stress that "development remains a nearly super-human task" (p. 326) even after a social revolution, and in saying this he must be disassociating himself from those, like Frank, who ascribe the problem just to capitalism.

It will have become clear that at many points I do not agree with some of the contributors. Yet this is undoubtedly a valuable and thought-provoking symposium, an extremely useful contribution indeed to development literature.

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Hsi-liang and the Chinese National Revolution. By Roger V. Des Forges. (New Haven and London: Yale University Press, 1973. Pp. xviii, 274. $12.50.)

None of the regional leaders of the last Ch'ing decade held the key post of governor-general longer than the Mongol bannerman Hsi-liang, and few were as energetic or successful in the reform movement. Using a wide range of archival and printed material in Chinese, English, and French, Professor Des Forges has discovered as much as is likely to be known about Hsi-liang's work as an administrator. In spite of some serious weaknesses, his book will be a useful addition to the sparse literature on the *ancien régime* just before the 1911 Revolution.

The richly detailed accounts of the reform movement, occupying much of the book, endorse the now widely held belief that despite Manchu sponsorship the movement was genuine in intent and sweeping in its effects. They also permit many revealing glimpses of the decentralized late Ch'ing polity in action. Des Forges establishes that Hsi-liang himself, as governor-general, played the leading role in such celebrated patriotic projects as the abortive Szechwan/Hupeh railway scheme, the Yunnan opium suppression, and the attempt to play off Russian and Japanese interests in Manchuria against the United States. On the other hand, Des Forges's evidence on Hsi-liang's difficulties with the court, the provinces and the foreign powers, suggests that the regional bureaucrat's freedom of action was rather limited in these years, a conclusion which contradicts the common notion of an unbroken trend to regionalism from the 1860s to the warlordism of the early Republic. Hsi-liang, it is true, retained full control over the reform projects at each incumbency, evaded or defied Peking's instructions when he saw it in the national interest, and even criticized incompetence at court. Yet he remained vulnerable to the censor's attacks, subject to frequent transfer (which he didn't resist even when demoted), and liable to normal tax quotas. His successes were hard won. As the representative of Peking, he had to confront the vocal provincial opinion of the decade – hostile to central authority, strongly reformist, and bent on rights recovery – but his reforming energy, resistance to foreign demands, and ultimately his endorsement of the constitutional movement won provincial respect and support in each of his posts. In his capacity as diplomat, he mastered as well as any contemporary official the art of defending Chinese interests without incurring the risk of foreign retaliation, and used brilliantly the diplomatic weapons of procrastination, a mobilized public opinion, and references to the letter of the treaties. No wonder the court found him indispensable. One suspects that his political durability should be attributed to his skill at maneuvering among the rival interests of the court, provincial elites, and foreign power as exercised through the consuls.

Des Forges has very little to say about the general problem of regionalism or the relationship of Hsi-liang's reforms to reforms elsewhere in China, but he finds the broad significance of his subject at a loftier level of generalization. Rejecting the usual distinction between nationalism and culturalism (attachment to a civilization) and the usual linking of Chinese culturalism with anti-foreignism (wholesale rejection of foreigners and their ways), and dismissing the notion that the concept of the nation-state and its sovereignty is essential to nationalism, Des Forges takes Chinese nationalism as simply the incorporation of culturalism and anti-foreignism into "a more militant stand against foreign encroachment" (p. xiii) and dates its appearance at least as early as the 17th century response to the Russian and Jesuit military and cultural challenges. Such a definition lumps together as nationalists an extraordinary assortment of Chinese and Manchu leaders along with Hsi-liang himself, and nullifies a concept which has helped to periodize and give meaning to the pre-Communist transformation of China.

Similarly broadening the concept of revolution in China, the author asserts a continuity in the "fundamental issues of popular participation and social equality," discerning a "pattern of change from above and below" (p. 195) in a series of events from the establishment of efficient Manchu government in the 17th century to the Cultural Revolution. He stretches the term "National Revolution" – usually applied to the period of party-led anti-imperialist mass movements in the 1920s – to cover China's entire recorded history. The last Ch'ing decade is but one part of its third phase; it is not, however, clearly related to or distinguished from other parts. Hsi-liang is labeled "populist" and "radical," terms idiosyncratically defined and given vague traditional pedigrees. In all this the reminder of continuities is refreshing, and so is the challenge to received ideas. But do we not need concepts which draw distinctions and explain historical change? Instead of doing this, the main function of Des Forges's revised terminology is an unconvincing rehabilitation of Hsi-liang – avowedly upper class and authoritarian, deeply pro-Manchu and prodynastic – as a man sharing some of the goals of twentieth-century revolutionaries.

On matters of detail, the book has some surprising errors. One of the many translations (p. 89) has no fewer than four in the space of 250 words: "at night" for "day and night," "regulations" for "discipline," "officers' school" for "officers," and "supplies" for "military pay." The leading prerevolutionary martyr in Yunnan is made to die of wounds suffered in battle, whereas all the evidence is that he died of illness, probably malaria. Carelessness with the sources in some places affects the interpretation. The Yunnan mining concession to an Anglo-French syndicate in 1902 (clearly described in Des Forges's sources) is misrepresented as a joint Sino-French effort and its operations as assisted by the court, as if to show the nationalist Hsi-liang resisting the treachery of his own government. An otherwise impressive section on the Sun Yat-senist Ho-k'ou rising in 1908 overstates the sympathies between Hsi-liang and young Yunnanese nationalists, claiming that the Tokyo Yunnanese "called for complete rights recovery" (p. 128), a misreading of their manifesto which promises like the rebels to "protect foreign lives and property." Here Des Forges argues that radical Yunnanese saw how unpatriotic the rebels were, rallied to Hsi-liang, and caused the revolt to collapse, but none of the three parts of the argument is really substantiated.

In short, when making use of this interesting study, the wise reader will be not only skeptical of its conceptual claims but also cautious wherever the argument may hinge upon close textual interpretation.

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Liu Shao-ch'i and the Chinese Cultural Revolution: The Politics of Mass Criticism. By Lowell Dittmer (Berkeley: University of California Press, 1974. Pp. 386. $4.95.)

Mao Tse-tung's chosen successor by the mid-1950s was Liu Shao-ch'i. Yet a decade later Liu was ousted from power. In fact, Liu became the symbol in China of a "capitalist-roader," someone who would reverse the gains of China's socialist, devolutionist, egalitarian revolution.

In addition to a political biography of Liu Shao-ch'i which will explain Liu's fall, Lowell Dittmer's lucidly argued book promises a sophisticated inquiry into the role of the great mass of her people in China's politics. Such a study is much needed. The ill-conceived notion that the Chinese polity works in a Stalinist manner, only not quite so bad, is all too popular. Unfortunately, Professor Dittmer's broadly conceived work does little to put such misinformation to rest. While he writes with great authority on numerous aspects of China's Communist Party, Dittmer forgets to mention that starting in the late 1930s, the Mao group in that CP, even while reading Stalin, self-consciously and self-critically began to remake itself to avoid the path of the Soviet Union's CP. The relation between China's more open and honest party and the Soviet party was antithesis. Stalin's party with its purges was for Liu a negative example.

The best part of Dittmer's study – in addition to his delightful use of high culture to richly illuminate elements of political psychology – is his careful reconstruction of the political dynamics of Liu's defeat and departure. Against those who contend that Mao had long since plotted and planned Liu's removal, Dittmer persuasively portrays the fluidity and indeterminacy of China's political processes. This, the centerpiece of his book, is a solid and subtle contribution to our understanding of China's political system.

Nonetheless, Dittmer refuses to accept the consequences of his own analysis. Whereas political scientists such as Michel Oksenberg, Andrew Nathan, Harry Harding and Melvin Gurtov have rewardingly investigated interests, groups, and conflicts within Chinese politics, Dittmer dismisses that substantial corpus of good work with a wave of his hand. Consequently, *Liu Shao-ch'i and the Chinese Cultural Revolution* is of little help in explaining the social roots of continuing conflict, tension, and struggle. Dittmer cannot pursue the nature of such conflict in a system of extraordinary dictatorial centralization accompanied by great popular dispersion of power such that middle levels and bureaucratic links are greatly attenuated. He never establishes any credibility for his major thesis that Liu combined order with revolution, economic efficiency with equality.

Even worse for a purported study of the politics of the Cultural Revolution, Dittmer cannot convey the importance of the topic. Whereas the almost revolutionary upheavals of the Cultural Revolution have led political scientists such as Oksenberg, Richard Pfeffer, Benjamin Schwartz, and Steven Andors to probe that extraordinary attempt to keep alive revolutionary zeal, to prevent the rigidifying of ruling groups, and to revitalize ultimate revolutionary goals, Dittmer's categories are limited mainly to institutions and political maneuvers. I do not gainsay such matters. Dittmer handles them well. Yet virtually to remove from an analysis of revolution its vision, fears, meanings, hopes and promises is to miss much of the impelling power of revolution, much of what makes China such an exciting area to study.

Where Dittmer excels is in his patient scholarly digging to sort out from a morass of data the ways of clarifying differences among the Cultural Revolution's groups and events which others miss. Here he uses some nice and easy quantitative techniques. These permit him to distinguish as no one before him differences in orientation between the Party press and the mosquito press of Red Guards. From those differences he convincingly deduces the divergent concerns of these groups in their various attacks on Liu. He establishes that Liu's symbolic evils far exceeded his real crimes. In many ways Liu's fall was, Dittmer says, his last gift to continuing the revolution.

What is missing from Dittmer's most useful book is a model of purposeful political motion. Consequently his biography of the early Liu is too much an uncritical compilation of awfully uneven data, while his weighing of the charges against Liu seldom situates their sources in group interests and dynamic visions within the Chinese polity.

Dittmer's wonderful predilection for theory is vitiated by his hurrying away from politics The power of his more philosophical moments, however, may well presage truly outstanding theoretical contributions to come.

Sadly, Dittmer's book, despite its numerous real virtues, is bloodless. Except for the details of Liu's fall, it lacks the feel of political life. Dittmer attempts a major contribution to our understanding of the role of criticism in Chinese politics. He, in fact, succeeds in raising our understanding far beyond previous scholarship. Nonetheless, by condemning the Cultural Revolution for its cruel anomalies, he slights more mundane sources of discontent. He wrongly has a few targeted leaders choosing suicide over participation in the struggle campaigns. He does not even mention, on the other hand, that those who lived and prospered in the struggle might be less than grateful to Chairman Mao for treating friends of the revolution in ways previously reserved for enemies. Even those who won may oppose a repetition of such struggle forms. The Cultural Revolution, like the Great Leap Forward before it, is not a model to illuminate a static experience, but a learning and transforming experience. As post-GLF campaigns have limited their damage to the economy, so one now asks how post-Cultural Revolution campaigns can avoid damage to innocent political leaders. To stress only the damage, as Dittmer does, slights the problem of winning popular support for continuing the revolution. One hopes that American political scientists will pursue that question. Certainly committed Chinese revolutionaries will.

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Afghanistan. By Louis Dupree. (Princeton, N.J.: Princeton University Press, 1974. Pp. 760. $22.50.)

Tourists, Peace Corps volunteers, AID technicians, diplomats, and even scholars have long felt the need for a reliable reference book which would collect the myriad bits and pieces of research about Afghanistan that have become available since the end of World War II. Modern research on Afghanistan has tended to be atomized with various disciplines following their own parochial interests. To assemble all this material into some kind of cohesive whole requires a scholar with sound interdisciplinary training and a global vision of the subject matter.

Fortunately, Dr. Dupree is such a scholar and his book is the kind of compendium needed at this infant stage in the development of Afghan studies. As a representative of the American Universities Field Staff, Dupree has spent some twenty-five years, more than half of them in residence, studying Afghanistan and producing the type of sensitive AUFS reports with which political scientists are familiar. Originally an anthropologist, Dupree has the active curiosity and the talent to explore the full range of the fascinating and wide horizons of a country so rich in unanswered questions. Moreover he brings to the inquiry not only the mentality of a Renaissance man but also many fresh and original viewpoints free of scholastic cant and cultural cliche. He has, therefore, produced a book that is replete with information and that offers this mass of factual data well spiced with a refreshing and delightful writing style.

The book is encyclopedic, with sections and chapters on geography, fauna, flora, natural resources, social organization, archeology, anthropology, history, politics, economics, art, and music. Fine photographs and informative charts, graphs, and maps are liberally sprinkled throughout the book and there are excellent appendices and bibliographic materials in the back. Of special interest to political scientists is the last part of the book, which covers the historical and political developments of the last two centuries. Of particular importance is the chapter on the Afghans' attempts to restructure their political system from a traditional oligarchy into a modern constitutional monarchy through the adoption of a new democratically oriented constitution. The analysis of this experiment in political modernization is enhanced and brought to life by the fact that the author was an eyewitness at the debates of the tribal *loyah jirgah* which served as a ratifying constitutional convention. The subsequent failure of the democratic experiment leading to the recent coup which deposed the king and created the first Afghan republic is covered in an epilogue to this massive 760-page volume. Afghanistan is now ruled by a military junta headed by a former Prime Minister and cousin of the deposed king. Dupree points out that the institution of the military junta, which has become the receiver for so many bankrupt democratic experiments in the twentieth century, has some unique features in Afghanistan that make the Afghan case worth watching closely in the next few years.

Dupree's work is, on the whole, descriptive rather than analytic. Its avowed purpose of covering the development of a strategic piece of global real estate through several millennia obviously means that few subjects can be explored in depth and that the treatment has to be multidisciplinary. These limitations are transcended by the scrupulously detailed research and documentation which went into the book and by the author's personal experience and fieldwork in many of the subject areas covered. The book is broad but never shallow or banal. Dupree's willingness to cut across disciplines on matters in which he is not necessarily an expert should be highly commended. Other scholars will no doubt fill in the details, provide corrective data and remedy lacunae. Of course, there will be no shortage of nit-pickers who will ferret out the minor errors, and narrow scholastic disciplinarians may complain with some justice that Dupree has fished for his data with a net too broad in its mesh. But for most academics and nonprofessionals this book will certainly become a classic, a modern Elphinstone, to which many thousands who must read and run will turn with profit and with pleasure when they want to learn something about Afghanistan. In this day of inflationary book prices this single basic volume is a good investment for individuals and libraries who cannot afford a full range of materials on Afghanistan.

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Beyond Cuba: Latin America Takes Charge of Its Future. Edited by Luigi Einaudi. (New York: Crane, Russak, and Company, 1974. Pp. 250. $11.50, cloth; $5.95, paper.)

This very useful volume discusses contemporary Latin America's politics and economics, and analyzes the implications of Latin America's realities for U.S. policies toward the region. All but two chapters are by present or former staff members of the RAND Corporation, whose research program on Latin American affairs Einaudi headed until he entered the State Department, just as this book was coming out. Eleven of the book's fifteen brief chapters were authored or co-authored by Einaudi; they represent most of his publications during a decade at RAND, and suggest the appraisal of Latin America which presumably now informs the Seventh Floor at State.

This collection suffers much less than most multi-authored works from lack of coherence. The central theme is emphasized several times, starting with the cover photo of the spanking new office building housing PetroPeru, the Peruvian state agency which now manages the properties formerly belonging to the International Petroleum Company, an Exxon subsidiary expropriated in 1968. The message is that Latin America has been changing considerably, that "increasingly stable and sophisticated institutions . . . [are evolving] which, in contrast to the past, are now providing determined leadership for reform" (p. 4).

The chapters systematically undermine both the revolutionary image of Latin America as a continent about to burst with purposive violence and the conservative image of a fundamentally unchanging region where established powers are secure. The essays counter a view of Latin American societies as "banana republics" helplessly subject to North American influence, but they also dampen any expectation that other Latin American countries will emulate Cuba's path – a total break with the United States.

The individual chapters argue that forces for major change in Latin America are now expressing themselves *through* institutions rather than primarily *against* them. Revolutionary violence, rural or urban, is no longer a major fact of Latin American life, but the counter-revolutionary containment of such violence has itself introduced pressures for reform within authority structures.

Among the forces for change, according to Einaudi, are two institutions once thought of as pillars of the *status quo:* the Catholic Church and the Armed Forces. The Peruvian Army, for instance, has been an agent for structural change, beginning with agrarian reform, ever since its bitter experience in repressing rural unrest in the early 1960s. And the state is gaining strength throughout Latin America, expanding its economic and political role, asserting itself with the competence provided by hordes of technocrats. Stability, which U.S. policy has long sought in Latin America, is now being achieved, but not on terms necessarily congruent with immediate U.S. aims, for the regimes of Latin America are forcefully demanding a readjustment of the terms of their relationship with Washington. As recently as ten years ago, Shane Hunt pointed out, "the only source available to the Peruvian government regarding the extent of foreign investment in Peru was the U.S. Embassy" (p. 61); now a much more accomplished Peruvian bureaucracy knows not only the extent of foreign investment but understands at least some of its favorite techniques, such as transfer pricing.

If *Beyond Cuba* suffers less than most edited collections from lack of integration, it gains less than the best such collections from the diversity of perspectives a good multi-authored volume can bring to bear. Despite his insightful observation that U.S. policy toward Latin America during the early 1960s was excessively influenced by projections of Caribbean experience, Einaudi has allowed this book to be skewed by overemphasis on the Peruvian case, for example. The Peruvian military government is put forward as a likely (and hopeful) model for Latin America's future, and the "excessive pessimism" of the late 1960s is explicitly disavowed. Readers in 1977 cannot help but wonder, however, whether the "bureaucratic authoritarian" model (to use Guillermo O'Donnell's awkward but valuable phrase) is not more likely to dominate Latin America's future, even in Peru, where deepening troubles have lead to an ever more repressive regime.

A related defect of *Beyond Cuba* arises from the apparent near-uniformity in political perspectives among the contributors. Issues ardently debated within the hemisphere – foreign investment, income distribution, the impact of violence on political development, the political role of the Latin American military, the relevance of electoral institutions – are treated throughout this volume within a very narrow range of dissonance. Brief phrases (a reference, for example, to the "disturbing possibility" of social revolution) allow readers to glimpse the underlying values which most of *Beyond Cuba*'s authors obscure beneath the veneer of social science. These values may also be deduced from the choice of topics and emphases: there is hardly a reference to the CIA in a book subtitled *Latin America Takes Charge of Its Future*, nor is there much discussion of Latin American efforts to control the political activities of multinational corporations or to restructure the terms of technology transfer. Equally glaring is the absence of any sustained discussion of Cuba and its revolutionary experience; like U.S. policy generally, the Einaudi volume attempts to deal with Latin American realities without ever really coming to terms with Cuba.

Beyond Cuba is both most interesting and ultimately least satisfying precisely in the field of Einaudi's major professional interest and current vocation: United States relations with Latin America. Einaudi's essays deal intelligently with the decline of the "Western Hemisphere idea," with the diminishing relevance of North American stereotypes to Latin American conditions, with the absurdity of maintaining in force regulations like those restricting travel to the United States by Latin Americans suspected of harboring anti-Yanqui sentiments, and especially with the need for U.S. policies toward Latin America more substantial than "lowering our profile" or calling repeatedly for a "new dialogue." He sagely observes that U.S. policies toward Latin America often have little to do with the region, and he rightly emphasizes the centrality of economic interests in the making of U.S. policies in the hemisphere. But despite his often acute remarks, Einaudi fails to explain the recurring pattern of inter-American tensions. Nor does Einaudi present a convincing rationale for a coherent new U.S. approach to Latin America. Einaudi bemoans the absence of a new vision of inter-American relationships, but he furnishes none of his own.

Perhaps the reason for this failure is suggested by the contradiction between Einaudi's underlying faith that the interests of the United States and Latin America are mutually supportive (although their congruence may be obscured by inadequate dialogue) and the implications of Daniel Schydlowsky's chapter on "Industrialization and Growth." Schydlowsky depicts a basic Latin American policy conundrum: "either industry begins to generate its own foreign exchange earnings in order to support its high rate of growth, or the rate of growth of industry and thereby of the economy as a whole must slow down to the rate of increase permitted by the availability of foreign exchange; i.e., roughly to the rate of primary production" (p. 129). Schydlowsky suggests various corrective measures: eliminating the anti-export bias in Latin American exchange rate systems, stimulating exports through tax rebates, and inducing multinational enterprises to use Latin American products in the rest of their international activities. He notes that if such remedies are adopted, access to the markets of the industrialized countries will be of crucial importance to the countries of Latin America, which will look on trade, not aid, as the key to their development prospects. Whether Latin America will secure improved access to the markets of the industrial countries, and especially of the United States, is probably the central question in contemporary inter-American relations. Its answer depends, no doubt, more on circumstances, calculations, and coalitions within the United States – and in North America's global relations – than on an analysis, however sophisticated, of Latin America's needs and trends.

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Lost Chance in China: The World War II Despatches of John S. Service. Edited by Joseph W. Esherick. (New York: Random House, 1974. Pp. xxiii, 409. $12.95.)

As an officer of the U.S. Department of State, John S. Service scrutinized wartime Chungking and the Kuomintang-controlled areas. In 1944–45 he was attached to the U.S. Army's "Dixie Mission" to Yenan as a liaison officer and he interviewed at length top Chinese Communist leaders. About one-half of the despatches written by him in the years 1941–45 are collected in this volume. They dissected Chinese politics astutely and were clearly intended to influence the policy of his government. The editor, Mr. Esherick, has furnished an introduction, an epilogue, and copious footnotes and commentaries.

One has to marvel at Service's knowledge of Chinese politics and his perceptiveness. His analyses and interpretations clearly anticipated the theses developed much later by Chalmers Johnson in *Peasant Nationalism and Communist Power,* Mark Selden in *The Yenan Way of Revolutionary China,* and Tien Hung-mao in his study of the Kuomintang government and politics of this period. The parallel which Service drew in 1944–45 between the Chinese Communists under Mao and the Yugoslavian Communists under Tito indicates his global awareness and his comparative framework in the assessment of development in Asia.

The despatches cover the period when the Kuomintang (KMT) was beset with myriad problems and was increasingly unfit to lead the resistance against Japan. Paradoxically, it was in this period that China rose in international stature. Aid from the U.S. and Great Britain bolstered Chiang Kai-shek's regime, although deterioration within the KMT and its government remained unchecked. Chiang met with Roosevelt and Churchill at Cairo as one of the Big Three to plan for a joint Allied strategy; China joined the elite company of the U.S.A., G.B., and the U.S.S.R. in the Security Council of the United Nations. This new status was certainly justifiable in terms of China's overall sacrifice, but it gave false impressions of the KMT government's ability to sustain and prosecute the war. Service prodded his government to exercise some effective leverage over Chinese politics as long as the U.S. was pouring aid into China. He contrasted the bitterness of the KMT circles, when they failed to receive more American aid, with the Chinese Communists' self-reliance under far more stringent circumstances. He reported the "negotiations" between Chungking and the Japanese authorities throughout the war years. He doubted that the people of the Communist-led areas behind the Japanese lines, after several years of de facto Communist rule and political indoctrination, would accept peacefully the imposition of KMT control.

Recognizing that the Chinese Communists were not democratic in the American sense, he presented their rationale for a united front policy and he explained succinctly the New Democracy advocated by them for this period. He characterized the top Chinese Communists as lacking individuality, but pointed out that there was a systematic orderliness in their thinking and operation. The unity of the Communists in this period contrasted with the faction-ridden state of the KMT, described aptly by Service as a "catchall" party.

Service's point of view ultimately collided with the predilection of the trouble-shooter-amateur diplomat General Hurley, who was then ambassador to China, and Service was recalled from Yenan. From then on loyalty and security hearings and dismissal from the State Department became his lot. Reading these despatches today, one cannot help being struck by a number of ironies: (1) In 1944–45 both Hurley and Chiang Kai-shek assumed that the Chinese Communists would follow Moscow's will and that one must work through Moscow to control the Chinese Communists. Service warned against such an unwarranted assumption. He emphasized instead that the U.S. should take the initiative in establishing a direct working relationship with Yenan. And yet Service and a number of his colleagues who shared his view were dogged throughout the cold war years because they allegedly aided the Russians. (2) Despite Service's affinity with the Chinese, his American patriotism comes through in the despatches From a Chinese (either KMT or Communist) perspective, Service reasoned from the vantage point of American national interests. His concept of "intervention" in the final analysis was flattering to neither the Communists nor the KMT. His cognizance of the growing strength of the Chinese Communists prompted him to urge his government to adopt a more realistic policy in China. How ludicrous that his loyalty was questioned! (3) As Mr. Esherick has pointed out, the policies advocated by Service were in fact never adopted, yet for having voiced his preferences he was blamed for America's failure in China.

Professor Esherick has performed a valuable service in making the despatches readily available to the reading public. Service's professional competence and devotion to his nation's cause may yet inspire a new generation of public servants, despite the sometimes prevailing atmosphere of complacency and cynicism in government circles. In recommending this volume, I would like to point out that Service's characterizing Madame Chiang Kai-shek as "puritanical" (p. 93) is off the mark. Despite rampant corruption in Chungking which Service has aptly delineated in this volume, readers must not assume that all KMT officials were corrupt. Many high KMT officials, though very anti-Communist, were not corrupt; not having amassed a fortune, many of them as refugees abroad after 1949 earned their living through honest labor. Despite these differences in emphasis, I highly recommend this volume not only to people interested in Asia, but, more particularly, to those who are concerned with decision making in our government.

SUSAN H. MARSH

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How the United States and Japan See Each Other's Economy: An Exchange of Views between the American and Japanese Committees for Economic Development. Edited by Isaiah Frank and Ryokichi Hirono. (New York: Committee for Economic Development, 1974. Pp. 143. $5.00.)

To political scientists, this joint work is primarily of interest for its treatment of the government-economy relationships in each country. The U.S. Committee for Economic Development (CED) is well known as the leading business spokesman for twentieth-century economic liberalism. What is less well known is that there is a Japanese counterpart whose collaboration with the CED began in 1960. The principal focus of studies sponsored by the two groups has been on international economics; the book under review is a companion volume to a joint statement on this subject issued recently. No merger of views was attempted in the present volume, however, and thus the inevitable differences in national perspectives are evident.

That the Japanese government is much more involved in business decision making than is the American government excites more envy than disdain from the American CED. Its paper suggests that the United States has something to learn from the aggressiveness of the Japanese government "in targeting and encouraging the establishment and growth of new and dynamic industries of high capital intensity and advanced technology" (p. 88) while also actively assisting the process of phasing out declining industries. The "protective but basically passive role of the U.S. government" (p. 88), which produces such mild measures as restrictions on imports and some assistance to workers in industries suffering foreign competition, is inadequate in today's world, the American paper strongly implies. The apparent explanation for this intellectual abandonment of the traditional posture of business self-sufficiency (whatever the reality) and flirtation with the intricate mechanisms of Japanese government-business interaction is the CED's emphasis on international trade. "In short," says the American paper (pp. 88–89), "it would seem essential today for the United States to devise and adopt more comprehensive and effective policies for lubricating the shift in industrial structure in line with a more rational international division of labor." Given the present situation, however, the American paper finds it expedient to label administrative guidance – which it defines as "the varying means of persuasion used by the government to realize the nation's economic goals and priorities" (p. 20) and which it calls the most important of the Japanese mechanisms – "a significant inhibitor of trade" (p. 91) that should be the subject of trade negotiations between the two countries. The CED also finds it hard to swallow the widely publicized notion that lifelong employment of workers by a single company has contributed strongly to Japan's economic growth. Yet, only one reservation – the Japanese tolerance for cartelization – beclouds the Americans' admiration for Japan's pragmatic policy toward industrial concentration. The paper is critical of U.S. antitrust policy for looking at size in terms of shares in the domestic market alone in a day of multinational corporations and increasing ratios of international trade to gross national product.

The relatively brief Japanese contribution to the volume is less preoccupied with the international trade implications of domestic economic practices. At the same time, it is remarkably blunt about the U.S. economy: "The dominant belief in individualism and the fear of government encroachment upon the rights of individuals and of government intervention in private economic activities have thwarted the development of appropriate public policies on such national problems as industrial adjustment, deteriorating balance of payments, urban decay, and environmental disruption" (p. 97). The Kennedy-Johnson domestic social welfare programs and Nixonian foreign policy changes away from a cold-war mentality are regarded as heartening. In a spirited postscript, the Japanese take exception to the American paper's criticisms of Japan's restrictions on international trade, excusing them as normal for countries undergoing development. Specifically, administrative guidance is defended as a substitute for the legal controls that guide industrial growth in the West, and the lifetime employment system for workers is characterized as a plus for the economic stability it has given to industrial workers.

The international oil crisis, which now preoccupies all industrialized economies, outdates some of the perspectives in this volume, as the authors of the papers themselves recognize. The book contains enough plain speaking, nevertheless, to warrant at least a modicum of attention from students of economic policy making.

LLOYD D. MUSOLF

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Public Planning: The Inter-Corporate Dimension. By John K. Friend, John M. Power, and Chris J. L. Yewlett. (New York: Barnes and Noble Books, 1974. Pp. xxviii, 534. $22.50.)

Academic writing on local politics in England has long been caught up in a prescriptive concern to improve local government. Until fairly recently, academics were worried about the basic size of most local authority areas. The dominant orthodoxy asserted that increased efficiency demanded the creation of fewer, larger local authorities which would then be in a position to reap economies of scale. There was little concern with decision making and management processes, for writers were content to assume that local authorities functioned smoothly as organs of representative democracy. The passing of the London Government Act and the more recent Local Government Act redrew the map of British local government and shut the door on this phase of reform as fewer, larger authorities were created. These changes forced concerned students of local government to carve out a new role of relevance for themselves, and they have increasingly turned their attention to the problem of managing the new authorities. The absence of any real indigenous tradition of writing on this subject has forced British academics to look to America, and from the late 'sixties they have been urging local authorities to adopt PPBS and engage in corporate planning.

This book has to be seen as firmly located within this reformist tradition, but the authors seek to push local authorities beyond corporate planning because "corporate planning is not enough: That, distilled into as few words as possible, is the central thesis around which this book is built" (p. xxi). "Interdependence between institutions is a pervasive feature of the structure of all societies" (p. 3), and so no matter what field of public policy we take, decision makers must recognize the "inescapability of interagency processes" (p. xxvi). In order, therefore, to deal with any "decision problem" a "policy system" will have to evolve – that is, there will have to be "decision-networks" or "planning-networks" and the activation and management of these networks is dependent on "reticulist" skills. The decision-networks created by reticulists will necessarily transcend the involvement and perspective of any single organization and will involve "co-ordinative or connective planning" based on what the authors choose to call a "multi-organization."

The authors have a twofold aim. First, through a case study of interagency decision making they aim "to move towards a richer understanding of the interorganizational – or "intercorporate' – dimension of the planning process" (p. xxiii). But, second, they are also propagandists who wish "to contribute towards the development of practical working guidelines for use by those who must operate within this general field, including those concerned with the design or adaptation of relevant institutional forms" (p. xxiii), and they are therefore active in "seeking ways in which the concepts developed in this book can be made of practical value" (p. 445).

In working toward these two aims, the authors divide their study into three parts. The first part serves "to provide a background for the more detailed case material that follows" (p. xxiv). The authors note that since the Second World War, there has been a recognition of the need to bring about "a controlled dispersal of population and employment away from . . . the congested areas of Birmingham" (p. 59), and although "a state of stalemate persisted throughout most of the nineteen 'fifties" (p. 70), by 1961 an interagency agreement was reached which led to the creation of the Droitwich Development Committee and the Droitwich Development Group. These structures were to provide the nucleus of a multi-organization policy system that was to manage the expansion of Droitwich.

In part two, the authors move to the meat of their case, and offer "an analysis of the ways in which the multi-organization policy system . . . proceeded to deal with a range of fields of decision including highway construction, housing and industrial development, and central area renewal" (p. 127). The focus is on the Development Group, and for each issue the authors identify the primary decision makers and the decision networks which they built around them drawn from personnel in appointed government agencies, central government departments, private enterprises, and community interest groups. We are provided with a rich descriptive analysis of the ways in which a number of issues were handled in Droitwich, but the authors' "ultimate purpose" is to move beyond the case to "form more general propositions" (p. 129).

In the final part of the book, the authors do generalize; tease out what they see as "some prescriptive implications for those concerned with innovation in planning" (p. 348); and outline hypotheses which "combine to suggest a set of opportunities for working towards the progressive enrichment of planning processes" (p. 471). Although I find much of the detail of this part of the book either meaningless, confusing, or obvious, the overall thrust is clear: intercorporate planning is needed, therefore reticulist judgments must be developed, and to do this a "public policy profession" of reticulists should be created, "backed by appropriate forms of professional training" (p. 501).

This study came out of the Institute for Operational Research in London, where two of the authors are engaged in operational research. Although all the authors are sensitive to the limits of OR and concerned to integrate the perspectives of political science into their work, political scientist John Power entered the project at "a comparatively late stage" (p. xvi) and left before it was finally completed. Not surprisingly, the study is grounded in a limited appreciation of political science, and only scant regard is paid to the political and ideological dimension of decision making. In fact, we are provided with a top-down view of planning and social change. The authors address themselves to the planners rather than the planned. It is always difficult to mix description and analysis with prescription and reform, since an interest in reform often serves to color and limit description. The concern to be "practical" means accepting, and working within, the structure of politics provided by dominant values and interests. The argument that political decision making could be improved if only we had skilled reticulists ignores the social, economic, and political forces behind the political status quo; it also fosters the illusions that priorities can be changed at no real cost to anyone and that governmental decision making somehow floats free of underlying relationships. This is a fine study, but it must be seen as an embodiment of the technocratic ideology: there are no outrageous questions here.

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The Private Government of Public Money. By Hugh Heclo and Aaron Wildavsky. (Berkeley and Los Angeles: University of California Press, 1974. Pp. 399. $15.75.)

Although several standard textbooks explain the legal forms by which public expenditure in the U.K. is regulated and scrutinized by Treasury, Cabinet, and Parliament, this new study by Professors Heclo and Wildavsky is the first full-length book to explain the political and administrative processes which lie behind these forms and which have changed a great deal in the last few years. Aaron Wildavsky has already established an impressive reputation in the study of budgetary processes, and Hugh Heclo has written extensively on European social policy. The reader will come to their new book with high expectations.

These will be fulfilled if, and only if, he is prepared to do some work to extract the main threads of the argument. For the book is deliberately written in the leisurely style appropriate to a work of descriptive anthropology. The authors find the clue to their subject in the mores of the small community of scarcely more than several dozen politicians and permanent officials who maintain constant personal touch on these subjects. This emphasis is especially apparent in chapter headings such as "Kinship and Culture," "The Nuclear Family," "Village Life in Civil Service Society," or "The Earthly City."

A common feature of such closed communities is secretiveness towards outsiders, reinforced in the British case both by an Official Secrets Act and the self-protective doctrine that the advice given by civil servants to Ministers is on a par with the secrets of the confessional. Faced with this attitude, the academic investigator has an unenviable choice to make. He can try to get his teeth into the red meat of policy issues. But in that case he will have to glean what he can from parliamentary speeches and reports, memoirs, and *sub rosa* conversations. Alternatively he can ask for official cooperation; and he will obtain a wealth of information and observations – much of it quotable although not attributable – on institutions, attitudes, conflicts, and personal interactions, provided he is content to generalize and avoid the specific issues and controversies of the day. The authors chose the latter course. This is perfectly legitimate; but it entails losses as well as gains. It is not sufficient to say "personalities and issues which change with the headlines will surely be different" (p. xix). The "main features" on which the authors wish to concentrate are difficult to grasp without some idea of what British governments spend their money on, and of the specific issues and controversies.

The best way the reader can remedy this deficiency is to have by his side a less ambitious but more policy-oriented study by Rudolf Klein and associates (Social Policy and Public Expenditure [London: Centre for Studies in Social Policy, 1974]). This will remind him that rather more than a quarter of the U.K. gross domestic product is absorbed by direct expenditure on goods and services by the public authorities, and rather more than two-fifths is absorbed by a broader definition of public expenditure which includes transfer payments such as pensions, subsidies, and debt interest. In the two decades since the early 1950s, the largest growth has been in education, while defense spending has actually fallen if measured at constant prices.

Heclo and Wildavsky have, of course, more ambitious aims. These are stated as the description of the "expenditure process" in British central government and the use of this process "to illuminate the characteristic process of that government" (p. xv). Although appearances suggest the opposite, it is in the first task (which on the whole occupies the second half of the book) that they are most impressive.

In discussing the "political administrators," the authors coin a number of striking aphorisms such as: "Governing public money is a private affair," (p. 2), "The medium is government and the message is public money," (p. 3) or "Interest groups . . .. are not outside the corridors of power, merely difficult to hear as they glide effortlessly into their places as unofficial appendages of government" (p. 8). At the end, however, I was wondering which of these perceptive remarks would apply equally well to most closely knit communities of people with a similar background, and which aspects were specific to British government.

The most informative part of the book is that dealing with the procedures for projecting and regulating public expenditures up to five years ahead, known as "PESC," after the Public Expenditure Survey Committee which supervises the effort. This is praised as being more advanced than anything existing in other countries; but the authors are in no doubt about the snags and disadvantages. A straightforward survey of the future cost of "existing policies" is inherently ambiguous. Before the supposedly impartial report is ready for Ministers, a great deal of haggling has already occurred about the meaning of "existing policies," their cost, and the way to project them.

The authors point out that "PESC" has shifted the emphasis from monitoring of past expenditures to forecasting – and forecasting in terms of "funny money" with everything expressed at "constant prices," the meaning of which is subject to endless doctrinal dispute. The book makes clear that the rolling forward projections make it even more difficult than before either to initiate new programs and/or to terminate old ones. Each department becomes acutely aware of whether it is getting its fair share of the available increment; and fundamental questions may have become even more difficult to pose.

The basic job of the Treasury, according to Wildavsky and Heclo, is to control the total of public spending, rather than to improve its allocation between departments or the efficiency with which it is spent. The last section of the book is devoted to the attempts that have been made to fill this gap by instituting "Program Analysis and Review" and to install a small Central Policy Review Staff to advise the Cabinet collectively. The aim has been to devise some more rational way of allocating funds than the political tussle between rival spending ministers. The authors stress that success to date is extremely limited. But they might have added that the nature of what was attempted in the early 1970s could not be understood without a consideration of the very peculiar relationship between Edward Heath and the Conservative Party which he led, as well as the enigmatic personality of the then head of the Civil Service, Sir William Armstrong.

Unfortunately the authors are unable to do this because of the self-denying ordinance they impose upon themselves. But after several pages of witticisms at the expense of the reformers, they do come out with some strong general strictures of their own. The "disposition to agree" among officials does often exclude from debate ideas that ministers might have favored if only they had known about them. Heclo and Wildavsky stress that there is a case for avowed partisans to analyze the collective advice of the Civil Service at private meetings of Ministers. The overconcentration on "macro" policy and the neglect of value for money within the overall total, which they stress, will have to be remedied. Some capacity to analyze expenditure should, in their view, be available to the Cabinet collectively. They hint that the proposed, and hitherto defeated, plan to put together in a central "Bureau of the Budget," the public expenditure divisions of the Treasury, the Civil Service Department, and the Cabinet Office may yet have to be revived. They insist that there is too much consensus and not enough conflict within Whitehall – the authors have not, one is relieved to discover, gone native after all.

But the fundamental question is why public expenditure should be regarded as a bore, not merely by ordinary citizens, but also by parliamentarians. The debates on the five-year rolling plans, so far from being the great occasions of state which the founding father of PESC envisaged, have been a fiasco.

My own suspicion is that public expenditure is a nonsubject. The original view of a pyramidal structure, which started from a strategic decision about the share of national resources going to the public purse, a subsequent division among broad functional subblocs such as social services and defense, and so on down the line, has proved untenable. The authors clearly sympathize with the old Treasury hands, who believed that the size of public spending could not be determined in abstraction from its composition and from general policy. But they could have clarified the point with policy examples. I should like to suggest a simple one: Is too much or too little spent on public sector housing? Too little if one believes that rented accommodation should be treated as a collective good to be supplied at well below market price; too much if one believes that accommodation is essentially a private good and the state's role should be limited to income support.

The main reason that MPs do not take Public Expenditure White Papers seriously is, however, a simple one and is referred to by the author. The White Papers simply add together known policies, without enunciating any implications for taxation. Even inside the Cabinet the options are presented in terms of whether personal consumption should grow, by say one and one-half per cent or two per cent, over the next few years. This question has very little meaning to politicians, who are in any case rightly distrustful of the forecasting models. The great parliamentary battles of the past have been fought over revenue and taxation, not spending; and public expenditure projections will be taken seriously only if their tax implications are also stated.

The conclusions I have been trying to extract are contained in the book; but they do not stand out boldly; and other reviewers might easily have concentrated on other aspects. The book's main value is in conveying the essentials of the public expenditure process in a secretive and fetish-bound governmental system. It is not a substitute for official White Papers and handbooks or for a knowledge of recent British political and economic history. But it will immunize even a mildly perceptive reader against the fads and fashions that sweep through the British – and probably other – government machines with bewildering rapidity.

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The Dimensions of British Radicalism: The Case of Ireland, 1874–95. By Thomas William Heyck. (Urbana: University of Illinois Press, 1974. Pp. 297. $10.00.)

Professor Heyck describes his book as "an extended essay in definition of the late Victorian Radical movement in its historical context" (p. ix). On balance, however, there is more historical context than definition, unless "definition" means rather less than I have in mind.

Heyck demonstrates very effectively that the Radicals approached Ireland almost entirely from an English perspective. Their sympathy for land reform and home rule was not the product of a Radical philosophy, except for a generalized hostility toward the landed establishment. It was a tactic to free Parliament and the Liberal Party from the shackles of the Irish question, so that Radicals might set about restructuring English society. Concerning the 1886 Home Rule Bill, Heyck writes, "There was nothing peculiarly 'Radical' in the reasoning of the pro-Home Rule Radicals" (p. 134). Ireland was an obstacle to be avoided or, pressured by Parnell, to be accommodated by Radicals. The Irish Question did not gain acceptance into the central core of Radical issues. In this sense Ireland forced the recognition of the geographic dimension of Radicalism – it stopped at the water's edge – but this is to say very little about the parameters of Radicalism as a philosophy and set of principles.

Heyck spends the first chapter describing the personnel, structure, and ideas of the Radical movement. Herein lies material of considerable interest to political scientists. For example, he joins a growing list of historians in using modern statistical methods and computers to analyze party formation and factions in the nineteenth-century House of Commons. By using multiple discriminant analysis he has identified the number of Radical MPs more accurately than ever before. Heyck and Klecka described this in greater detail in the *Journal of Interdisciplinary History* (Autumn 1973). Their conclusions are summarized in this book, and a prototype Radical is presented – a pious, nonconformist, anti-establishment businessman dedicated to principles of political equality, disestablishment, self-improvement, temperance, and social reform. The core beliefs of Radicals are briefly described, but the discussion is too brief, and in later pages Radicalism itself becomes rather lost in the description of tactics employed by Radicals in struggles over confrontation and home rule. Two additional problems arise in the rest of the book in connection with identifying precisely the dimensions of Radicalism. The first is that, while Heyck presents a proto-typical Radical in chapter one, and thereafter often writes of *the* Radicals, he nevertheless makes it clear that they were always divided among themselves on Ireland. Organizational diffusion, for example, was a fundamental trait. The dilemma posed by coercion versus reform was to divide Liberal Radicals for another generation, so that the Irish Question did not prove as definitive as we might have been led to believe in the introduction. Most Radicals responded to Irish confrontation with a tactical sympathy, but some did not, and, hence, the dimensions are blurred. This leads to the second problem, the role of Joseph Chamberlain, the leader of the Radicals, who finally rejected Irish nationalism. Heyck explains Chamberlain's political scheming not by reference to his Radical, or even Unionist, principles, but by his authoritarian personality (*vide* Adorno et al.). This may be valid, but one cannot hope to define the dimensions of Radicalism in the light of the Irish question if the leading Radical's behavior was essentially idiosyncratic.

The book makes clear that Radicals saw the Irish Question less as an issue of conscience or principle than a problem to be resolved to release Parliament for other things. How and why this was done will be of less interest to political philosophers and political scientists than to historians. For them, Heyck still has much to offer in a well-written and -researched book.

ALAN WARD

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Sverges Kommunistiska Parti 1939–1945. By Yvonne Hirdman. (Stockholm: Allmänna Förlaget, 1974. Pp. 311. Sw.Kr.38.–.)

Written in Swedish with a ten-page summary in English, this is the second volume in a series published by the Stockholm University Institute of History on Swedish parties during World War II. The Social Democratic Party was the subject of the first volume, future volumes being reserved for the Conservative, Agrarian, and Liberal parties.

From a comparative point of view the wartime history of the Communist Party is of special interest. As Yvonne Hirdman, a Swedish historian, points out, it was not a Swedish party in the same sense as the others, either in fact or, during the greater part of the war, even in form. Until the dissolution of the Comintern in 1943 it was officially designated as being merely the Swedish section of that international body or party: "Sverges Kommunistiska Parti (sektion av Kommunistiska Internationalen)." Its history therefore closely parallels that of other Communist parties. They all passed through three distinct phases in the course of World War II, starting with the Hitler/Stalin pact of August 23, 1939. The first phase came to an end with Hitler's invasion of the Soviet Union on June 22, 1941. The second phase comprises the period up to the dissolution of the Comintern in May, 1943, the third phase extending from then until the end of the war in Europe two years later.

The history of the Communist Party of Sweden has some distinctive features, because it operated in a neutral country bordering on neighbors involved in different ways in the war: Finland was fighting the Soviet Union, Denmark and Norway were occupied by German forces. Like other Communist parties it suffered a sharp decline during the period of the Nazi-Soviet pact, but prospects at once became much brighter when the German attack was launched on Russia. From that moment Communist influence was again on the increase. Although it took a long time before the Soviet Union began to show its impressive strength, the sufferings of the Russians and their heroic struggle against the ruthless German invader evoked great sympathy; and the alliance between Russia and the Western Powers gave a new air of respectability to communism. In Sweden the party was never very strong, however. At the 1940 election it had received but 3.5 per cent of the votes cast. In 1942 the figure rose to 5.9, and when the party reached its zenith shortly after the war ended, it polled 11.2 per cent of the vote. The number of Communist seats in the Swedish Diet increased from four in 1940 to seventeen at the end of the war.

Yvonne Hirdman's emphasis is on the policy followed by the party at the national level. She does not analyze its electoral base, its organization, or its relations with the labor unions, but she is able to present an illuminating study of its strategy and tactics under the most widely changing circumstances. With great difficulty the leadership avoided having their party declared illegal during the period of the Nazi-Soviet pact. Conditions were then entirely different, of course, from those of the two succeeding periods; but the latter also differed slightly one from the other. After the dissolution of the Comintern the Swedish party still followed international Communist tactics which, after June, 1941, demanded collaboration with democratic parties, particularly the Social Democrats. Yet a gradual change seems to have occurred within the SCP during the last phase of the war, when it took part in democratic politics without being any longer supervised by the Comintern. It appears as if it was on its way to becoming a reformist, parliamentarian party. However, the development was stopped and reversed in 1947–48. An explanation is indicated in the following words on the last page of the English summary: "The Party's basic problem, i.e., its dependence on the Soviet Union, had not been altered."

Yvonne Hirdman has made a thorough study of the Swedish and foreign Communist press, numerous pamphlets, and parliamentary records; still more important, she has had access to the wartime archives of the Communist Party, including its internal circulars and unpublished Politbureau and Central Committee records and protocols, which are quoted extensively. Her book includes a detailed bibliography, and there is a sixteen-page appendix of illustrations.

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Soviet and East European Foreign Policy: A Bibliography of English and Russian Language Publications 1967–1971. Edited by Roger E. Kanet. (Santa Barbara, Calif. and Oxford, England: ABC-Clio Press, 1974. Pp. xv, 208. $15.75.)

This slender volume contains 3,237 citations of books and articles on the subject of foreign policy of the Soviet Union and of the eight East European countries. (Albania's foreign policy, for example, can be found in nine listings: three general, four dealing with Albanian-Chinese relations, and two with Albanian-Soviet relations.) According to the editor, the bibliography was originally compiled from Soviet sources for listing in the *Canadian Slavic Studies, Bibliography* and from English language sources for the *American Bibliography of Russian and East European Studies, 1967–1971*. Entries dealing with the subject of Soviet and East European foreign policy from the two sources are here put together. Some sources of information are not included in the Bibliography (*JPRS Reports;* IASP translation journals such as *Soviet and East European Foreign Trade; Current Digest of the Soviet Press;* etc.) and the editor points this out. East European publications on the subject are excluded as well. The selected items are not annotated, and "no attempt has been made to note [their] relative significance." Entries are listed alphabetically according to authors and are numbered. The index is arranged according to subjects and refers back to the numbered entries.

Bibliographies are labors of love. Unglamorous, they do little for the bibliographer's reputation and professional standing. As a service to the profession, they rank in attractiveness even below book reviews. Time-consuming to assemble, they exclude any spark of creativity *a priori*. Their quality depends on their inclusiveness, attention to detail, and precision of information – something that the bibliographer is never sure of. His attention to grubby detail is quietly savored by the user, but his omissions and mistakes are criticized with gusto by the reviewer.

And yet, good bibliographies are great time-savers. We compile bibliographies all the time, for one reason or other. Those who help us are welcome and appreciated. This is why, on behalf of those in the profession who will have an occasion to use this bibliography, I wish to express welcome and appreciation to Roger Kanet. As long as we do not require some published bibliographical work from each member of the profession – as a condition of promotion or some such – we must be grateful to the few individuals who volunteer this service.

This is a good bibliography. The index, the consumer's key to the bibliography, could be improved upon by italicizing the major items, thus differentiating them from the marginal sources. But the collection is entirely satisfactory. Let's hope others will take off where Roger Kanet stopped.

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Business and Politics in India. By Stanley A. Kochanek. (Berkeley: University of California Press, 1974. Pp. 400. $16.50.)

Stanley A. Kochanek has dealt with some of the problematic features in the development of modern business associations in India and has assessed the role of the Indian political culture in keeping these associations from performing their due role in capitalist development. He has ably chronicled the twists and turns of this story and provided rich and painstaking documentation. The book should be read by all those who are interested in the role of interest groups in political development.

Kochanek's analysis can be summed up as follows: Modern India presents evidence of rapid strides (compared to a long antecedent period of stagnation) in almost all sectors of society – business, industrial, military, professional – which do not however add up to a "modern" society as the term is normally employed in the West. The basic difference is the lack of strong and cohesive interest groups in India despite a great deal of professional development. As a result, business interests have not acquired an autonomy and an inner cohesion that would enable them to acquire dominance over society. This has further reinforced the traditional dominance of the literati from which the modern urban middle class of intellectuals, bureaucrats, and politicians has developed. This middle class provides the backbone of political democracy as well as the cause of low level of performance of democratic institutions. The present "crisis of performance" in India is a reflection both of the failure of a bourgeois revolution taking place – on the basis of which a proletarian revolution might have been possible in time – and of the staying capacity of the professional middle class that has not permitted more authoritarian forms of domination to develop despite the power of both the military and the bureaucracy. The answer to the crisis of performance is likely to come from further expansion of the functions of democratic government and corresponding increase in the authority of the ruling party than on the basis of a strong constellation of interest groups as in Western democracies.

According to the analysis presented by Kochanek, four factors have inhibited the growth of business and industry into a cohesive and powerful national force. First, traditionally Indian society has been dominated by a group of *literati* which has always looked upon business with suspicion and contempt. Second, Western education, by drawing upon the same literati to form a distinctive *elite*, has accentuated this suspicion and prevented a sizeable and composite bourgeoisie from emerging. Third, the highly pluralist character of Indian society has kept the business sector itself riven with differences and hostilities based on caste, community, kinship and linguistic-regional ties, thus preventing the rise of a cohesive bourgeoisie despite the formation of a cluster of voluntary associations (which itself reflects these divisions and reduces the larger all-India associations to a loose and "federal" status). Finally, lacking such internal cohesion and resulting dominance over national affairs, business has come to depend increasingly on government – and the ruling party – in ordering its affairs, and it has thus remained a dependent group with little grit and power of its own. This, in turn, has obliged business to enter into legitimate as well as illegitimate deals with bureaucratic and political elites, wielding indirect influence that is quite considerable but unstable and transient. Modern India is, therefore, more a creation of the intellectual and politicio-administrative middle class than of the bourgeoisie. This has given rise to a model of development that is different from both the liberal capitalist and the Marxist paradigms of development. Kochanek does not use this language, but the analysis presented by him leads to the above position.

On one point, however, Kochanek falls prey to a widely held Western perception about India (shared by some Indians too). This is that adult franchise has given a new lease on life to caste and community interests and prevented more integral, modern, forms of associations from emerging. A proper understanding of the impact of adult franchise should in fact lead to an opposite conclusion, namely that caste distinctions and traditional discriminations are being overlaid by a process of considerable mobility as well as mobilization which, in course of time, has given rise to new forms of identity and association and has eroded the legitimacy of the core principles of caste segregation. The decline of upper castes and the rise of middle and lower castes underscores this phenomenon. It is this, rather than the persistence of traditional patterns, that has prevented the bourgeoisie from acquiring a position of dominance in India. In this light politics has to be viewed as a catalyst of major social transformation and not just an impediment to the capitalist path of development. The capitalist path cannot provide a model for India given India's immense size and problems and the incapacity of the capitalist mode to resolve them. India never had a revolutionary bourgeoisie. The mentality of the capitalist class in India has always been that of the petty bourgeoisie. It can only survive as a satellite of the state.

RAJNI KOTHARI

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Modern Greece: Profile of a Nation. By D. George Kousoulas. (New York: Charles Scribner's Sons, 1974. Pp. 300. $10.00.)

The author of this volume, a professor of comparative politics, makes a serious attempt to present a "fairly objective, informative, and yet lively" political history of modern Greece. Chronologically, he covers the period from the Greek War of Independence (1821) up to and including the seven years of the Papadopoulos dictatorship and has as his primary objective the examination of the political, ideological, social, and economic underpinnings which, taken together, form the basic content of modern Greek political ethos and culture.

This well-written book is based on two kinds of sources: secondary works available in several languages which represent diverse points of view; and the author's first-hand experiences gained from his direct involvement in recent Greek political affairs. Having been an unofficial adviser to the major figures of the Papadopoulos regime and a contributor to the military-sponsored Constitutional charter of 1968, Professor Kousoulas presents a point of view which deserves to be heard.

Modern Greece could be appropriately viewed as a conservative explanation of Greek politics. Such a perspective needs neither apology nor defense, unless it interferes substantially with basic rules of fairness and objectivity, as I feel it does in this case. I should hasten to add, however, that the apparent subjectivity is the result of indiscriminate use of concepts which lately have acquired indistinct connotations.

Recurring throughout the book are terms which in recent times have acquired an ideologically recognizable, if not negative, connotation. Their use in the context of this book lends support to its classification as a "conservative viewpoint." For example, repeated in the book is the author's preference for *stability, maturity, reason,* and his aversion to *emotionalism, immaturity,* and the like. They are indeed legitimate preferences, but the problem emerges when the terms are left undefined. So far as I know, there are no acceptable measurements of the psychological temperament of peoples or political elites. However, if the ability of a people to govern themselves by democratic means is to be taken as an indication of sophistication and maturity, then, I submit, the Greek people and their leaders rank very high indeed. The modern Greek state, which was launched in the 1830s with the support of the major powers, enjoyed 112 years of some form of parliamentary democracy out of a total of 145. Yet, in this account the Greek peasant is labeled "docile and illiterate" (p. 111), thus inferentially not ready for democracy; in another instance we are told that "logic has long deserted the Greek electorate" (p. 118); but the military is viewed as being "always a sensitive recipient of public sentiment" (p. 89). Such statements, often made without adequate justification or explanation, can easily be refuted by utilizing the same historical record. For example, if the military is to be considered as the "sensitive recipient of public sentiment," how is one to explain that the Greek military has made an enormous mess whenever it interfered with the political affairs of the country? How can one explain the universal dislike for the Greek rulers manifested following the downfall of every dictatorship? Is the Greek electorate still to be considered "immature" and thus in need of the strong hand, when by making use of the electoral process it recently brought to power a most stable parliamentary majority to deal with pressing national issues? Hardly so.

In addition to the problems one has with the ideological tilt of this book, there is also the problem of selective emphasis. In my view the author understates the pervasive role of Greek military conspiracies, which since the mid-'forties have played havoc with the democratic processes of the country. Only one conspiratorial organization is perfunctorily mentioned (I.D.E.A.), while the Papadopoulos conspiracy, in existence since 1957, is ignored.

But the most problematic issues raised by this book concern the events of the 'sixties. The author places the blame for the disruption of the political process in the 1960s on the shoulders of the political leadership, a small part on Queen Frederika, and almost no blame on the military-security establishment which, in fact, caused most of the disruptions. To do that, in my view, is to attribute to the victim and the criminal equal guilt. It is generally acknowledged by Greek commentators and political leaders that the political instability which troubled Greece in the 'sixties was, by and large, caused by those who came to "save the nation" in 1967. A second problem is related to the characterization of the 1968 Constitution as "democratic and modern" (p. 283). One has great difficulty in accepting a constitution as "democratic" when it in fact legitimizes the right of armed forces to become the regulator and arbiter of the "political and social system" of the nation.

At best, then, *Modern Greece* can be considered a useful and conservative interpretation of the development of the post-1830 Greek state and a contribution to the on-going debate on the meaning of modern Hellenism, by a prominent scholar of the subject.

NIKOLAOS A. STAVROU

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Industrialization, Community Structure, and Socialism: An Ecological Analysis of Norway, 1875–1924. By William M. Lafferty. (Oslo: Universitetsforlaget, 1974. Pp. 364. $10.00.)

This is one of those rare books which offers stimulation and new insight to all three kinds of students of comparative politics – the empirical theorist, the methodologist, and the single-country specialist. The problem of the relationship between social and economic structures and the development of socialist ideologies was first addressed in an earlier volume (Lafferty: *Economic Development and the Response of Labor in Scandinavia* [Oslo, Universitetsforlaget, 1971]). The present volume considers two questions: the dynamic relationship between industrialization and labor response (Theory I) and the static relationship between social structure and the attractiveness of specific socialist ideologies (Theory II).

The author (Research Director at the Institute for Social Research in Oslo) prepares the non-Norwegian specialist for his discussion with two excellent summary chapters; one of them outlines the pertinent contours of Norwegian economic development in the nineteenth and early twentieth centuries; the other neatly sorts out the often considerable confusion of the early history of the labor movement. The elections of 1921 and 1924 seem "tailor-sewn" to Lafferty's specifications; in 1918 the Tranmaelite radicals had taken over the organs of the Norwegian Labor Party, in 1921 the moderates split and formed the Norwegian Social Democratic Party, and by 1924 there was a final split in the mother party and the Communists formed their own NKP. The electoral contests among these three parties provide a clear indicator of the relative strength of three socialist ideologies at a time immediately following very sharp increases in industrial activity, the surge of 1905–1915.

Under the rubric of Theory I, Lafferty reviews five broad theoretical statements about the impact of industrialization on radicalism: Marxism, "vulgar Marxism," anomie, curvilinearity, and contextualism. The analysis "explores" these theoretical statements (the author reserves "test" only for the anomie variant, the Bull-Galenson proposition) by grouping the 589 communes in Norway in 1900 according to urbanization, and then examining the impact of levels and types of industrialization, industrial change, and social disruption on the dependent variable, socialist strength. Both the methods (regression analysis and causal circuits) and the interpretations are rigorously careful and self-conscious; not surprisingly, no single conclusion emerges, except, perhaps, that everyone had some insight, and no one had all. Lafferty suspects that in fact these variables operate differently in different contexts, that a strong increase in industrial activity meant different things in different types of communal settings.

Theory II is Lafferty's highly suggestive contribution to comparative political ecology. From the two principal elements of community morphology (symbiosis and commensalism), Lafferty derives two types of communal groups – corporate and categoric – and eventually a fourfold typology of community structure. He then hypothesizes the specific socialist ideology attractive to each type of community structure. Thus a traditional society with nondifferentiated familial corporate groups, such as self-sustenance farming, should show a very weak socialist potential; a hierarchic agricultural or industrial society with differentiated corporate groups should have a strong and radical socialist potential; and the corporate pluralist type with integrated multitypical corporate groups would be expected to exhibit strong but moderate socialist potential.

In this section Lafferty gives up some of the strict nomothetic goals of his analysis of Theory I, in favor of a "data-structuring" strategy, which includes more data and introduces a regional dimension. He also finds other analytical methods appropriate, adding to the regression analysis a factor analysis and then a selection and intensive look at twenty-seven critical core areas of socialist strength.

Lafferty has long been pleading with us to develop level-specific theories and analyses, and this is a superb example of the useful consequences of such careful social analysis. Patterns visible at one level may disappear at another; previously unnoticed or unexplained phenomena achieve first clarity and then comprehensibility; generalizations are often sharpened or discarded. His highly critical use of a variety of statistical analyses and his candor about data, measures, indicators, and interpretations should be adopted by us all. I hope very much that this work will lead to studies which "drop down to another level" and include an intensive historical study of a very few communes, focusing on labor's response to the immense structural and symbolic strains of industrialization. I also hope that next report will not be marred by the first distracting and then irritating typographical errors.

PENNY GILL

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Church and State after the Dreyfus Affair: The Separation Issue in France. By Maurice Larkin. (New York: Barnes & Noble, 1974. Pp. 294. $21.50.)

The clash between "the One True Church, Holy, Roman and Apostolic" and the secular, French "One and Indivisible Republic" is bound to be an enthralling subject. This book – whose subtitle, "The Separation Issue in France" reflects the content more accurately than its title – is centered on the conflict occasioned by the separation of church and state after centuries of relatively harmonious collaboration. The first third of the book sketches in the pre-Separation background, with particular emphasis upon the state of grass-roots Catholicism in France and the way in which the Concordat had worked in the nineteenth century. The last chapter consists of a highly condensed postscript on the post-Separation situation, followed by the full text of the 1905 Act.

The originality of this book is that it brings out the reluctance of the leaders on both sides to countenance a separation of Church and State in France. Dr. Larkin shows that Pope Leo XIII leaned over backward to accommodate successive French governments following his decision in favor of *ralliement*. While his successor, Pius X, reversed Leo's pro-French line, it having become clear that France would not help the Pope against the Italian government in his urge to recover at least some Papal territory, he and his Secretary of State, Merry del Val, would have preferred to preserve the Concordat. Larkin demolishes the conventional wisdom about Combes as a *mangeur de curés*, remorselessly pursuing the severance of State from Church. On the contrary, Combes, like the majority of his 1902–5 government, would have preferred to keep the Concordat; but the pressure from the Left, notably by the recent convert to Socialism, the Dreyfusard Francis de Pressensé, helped by an alliance of Socialists, Radical-Socialists, Radicals and *Union Démocratique* liberal-conservatives, forced his hand.

Perhaps Larkin could have brought out even more clearly than he does why the ecclesiastical leaders in Rome and especially in France, as well as the anticlerical political leaders, predominantly supported the preservation of the status quo. The reason appears to me to be their common opposition to pluralism and their desire to retain the traditional alliance of the spiritual and temporal powers. From the standpoint of the politician in office, the power to control ecclesiastical appointments and to curb the propensity of clerics to engage in political interference of a hostile nature, was very attractive. The cost incurred – protecting an established church aligned with one's political enemies and providing the salaries of some of its clergy – seemed cheap. From the viewpoint of the Church, the advantages of the bargain were clear to the hierarchy, the Vatican, and the Bishops, who saw the link with the French state as a bulwark of their authority. Dr. Larkin demonstrates that the more extremist rank and file on both sides wanted to destroy this collusion between the political and religious establishments, because they wanted either to eradicate ecclesiastical influence over the minds of the people or use the Church's influence to restore the monarchy or revive a hitherto straightjacketed religious fervor.

The pluralist compromise offered to the Church in the form of a system of religious associations was contemptuously spurned by the Pope as a plot inspired by Freemasons to weaken the Papacy's hold over the French Church, a Gallican or schismatic device to destroy the unity of the Roman Catholic Church. Dr. Larkin provides an illuminating case study of how the traditional French mode of settling disputes, not by negotiated compromise but by the intransigent affirmation of inflexible principles, led to a crisis from which the Church emerged shorn of much of its wealth but in a position to counterattack secularism over the next half-century. As such, this book deserves a wider audience than that of specialists simply concerned with Church-State relations in France because it shows how the Separation crisis led to important unforeseen changes in the French political process.

JACK HAYWARD

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South Africa: Economic Growth and Political Change, With Comparative Studies of Chile, Sri Lanka and Malaysia. Edited by Adrian Leftwich. (New York: St. Martin's Press, 1974. Pp. viii, 360. $12.95.)

Will economic growth erode *apartheid* in South Africa? Will the rational imperatives of growth result in nonviolent political change in a liberal and democratic direction? A new generation of young scholars in England and South Africa is reconsidering these perennial questions. They are also reconsidering traditional liberalism and Marxism in seeking better to understand a political *status quo* that seems intractable despite the impact of economic growth and "modernization." This volume contributes to the debate. Before discussing it, this reviewer hopes that reference to related studies will be of interest to American political scientists.

In South Africa itself, English-speaking and Afrikaans-speaking academics and others, most of them white, dealt with these problems in an important "Study Project on Christianity in Apartheid Society" (Spro-cas) during 1969–1973. Many of them challenged the political appeal and relevance of liberal individualism in a "plural society" under police state control. Foreseeing no revolution and rejecting violence, the Spro-cas political report pointed toward eventual radical change by proposing an elaborate series of steps that give priority to groups and localities.

Meanwhile, young academics in England, particularly former South Africans, have been engaged in a lively, if not always novel, discussion of race and class and the dynamics of change. Reconsideration of liberal interpretations has come partly in reaction to the publication of the *Oxford History of South Africa*, edited by Monica Wilson and Leonard Thompson. Influential articles have been written by Frederick Johnstone, who argues that economic development does not undermine white supremacy but constantly reinforces it, and by Harold Wolpe, Martin Legassick, Stanley Trapido, and others. Although more radical in perspective than Spro-cas, these writers are generally as pessimistic about the likelihood of rapid political change.

The intellectual ferment in England is sometimes frustrating to American academics because of frequent references to unpublished manuscripts and unfinished M.A. and doctoral theses. Happily some of this work is finding its way into the pages of a new interdisciplinary – and ideologically uncommitted – journal of high quality, the *Journal of Southern African Studies*. Note should also be made of the new and radically committed *Review of African Political Economy*. By comparison, intellectual concern with southern Africa is scattered in the United States, and it has no centers comparable to the University of York, with which the Leftwich volume may be identified, or the University of London.

The argument that economic growth will bring about changes that will make South Africa less objectionable than it is has an understandable appeal to U.S. and other Western policy makers and businessmen, who are not prepared to abandon wishful thinking or to endanger profits. The argument has been at issue in an international "Study Project on External Investment in South Africa and Namibia," whose reports began to appear in 1975. This ambitious study project, with participants in England, South Africa, the United States, and Western Europe, is inconclusive on the issue, although it contains much valuable data and argument.

Ironically, the economic determinists among students of South Africa tend to be the liberals rather than the radicals. South African policy makers, also, fear that "economic integration" will lead to "political integration." Michael O'Dowd, a staff member of a mining finance company in Johannesburg, presents the liberal "reformist" argument both in the "Study Project on External Investment" and in the Leftwich volume, where he appears as a foil. Following Rostow, he sees stages of economic growth that he predicts will result peacefully in radical political change in a democratic direction in about 1980.

The book under review is repetitive and put together in a somewhat haphazard way, but the individual essays are provocative contributions to the debate. Inserted in the middle of the book are two carefully written analyses by York political scientists: James Jupp on Sri Lanka and Malaysia, and David Skidmore on Chile. There is little here to encourage the economic determinist, but the relevance to South Africa is not drawn since the book lacks a concluding, evaluative chapter. Nevertheless, common themes run through these and the remaining nine essays. (Incidentally, all are by whites.) In general, except for O'Dowd's, they argue the inapplicability of developmental patterns found elsewhere (Max Weber in particular is criticized) and the importance of unique historical circumstances and political determinants in understanding political change. All the writers, again with the exception of O'Dowd and Jupp and Skidmore, are generally pessimistic about nonviolent and radical change in South Africa.

Only two of the essays have appeared elsewhere: "The Plural Society in Africa" ("plural society" being distinguished strictly from American pluralism) by Meyer Fortes, the Cambridge social anthropologist; and a discussion by John Rex, sociologist at the University of Warwick, on the usefulness of combining Marxism and pluralist theory in the analysis of South Africa.

Fully one-third of the book is taken up by essays written by Norman Bromberger, an economist at Essex, and Adrian Leftwich, a political scientist at York. Both were politically active as students in South Africa in the early 1960s and were detained in prison. They are disillusioned with the liberal hopes of those days, impatient with analysts who, they believe, underestimate South African complexities, and determined to look realistically at possibilities of change within the existing structure of power. They question and criticize Johnstone, Wolpe, Legassick, and Trapido, among others. Bromberger's article on "economic growth and political change" is extraordinary: often jejune and pretentious in its self-consciousness and not ripe for publication, yet in its insistence that South Africa's long-run future is "radically 'open' " (p. 76), it is fresh and worth careful reading.

Like Rex, Leftwich seeks to combine pluralism and Marxism. I find his application of pluralist theory to South Africa rather turgid and productive of few new insights. Nevertheless, the serious student will be attracted to his industrious effort. Although Bromberger is less pessimistic than Leftwich about the political consequences of economic growth, both articles give surprisingly slight attention to the problems of political breakthrough.

Four political scientists contribute the remaining chapters. David Welsh of the University of Cape Town and a key member of Spro-cas skillfully analyzes "the political economy of Afrikaner nationalism"; Ruan Maud caustically dissects "the myth of White meliorism"; James Barber of The Open University demonstrates that economic strength undergirds South Africa's "outward policy," which (along with foreign investment) supports the *status quo;* and Christopher Hill of York cogently and pessimistically assesses "the future of the liberal spirit" in South Africa.

Maud, unlike O'Dowd and Bromberger, sees in capitalist economic development a heightening of racial antagonism and white fear. Welsh concludes with the hope that enlightened Afrikaner nationalists, who are indeed profoundly affected by economic growth, may eventually make political compromises, but such hope finds scant support in his cold analysis of white group loyalties and fear. Neither he nor Maud nor Hill – nor I for that matter – can see how, in South African circumstances, the black majority can effectively and nonviolently exploit economic growth to produce a political breakthrough that would end white minority domination.

THOMAS KARIS

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The Stalinist Terror in the Thirties: Documentation from the Soviet Press. Compiled with preface and introduction by Boris Levytsky. Hoover Publications Series, No. 126. (Stanford, Ca.: The Hoover Institution, 1974. Pp. xxvii, 521. $14.50.)

As Alexander Solzhenitsyn and other members of the human rights movement in the USSR have recently reminded us, there can be no progress toward democracy in that country until the régime disavows frankly the grim legacy of Stalinism. Reluctant half-measures are not enough: it is essential to carry to their logical conclusion the investigations into the purges of the 1930s and other crimes which cost the lives of millions of innocent people, to publicize these facts widely, and to bring those responsible before an impartial court of law. Unless such measures are taken, no confidence can be placed in the assurances by party and government spokesmen in regard to a supposed "strengthening of socialist legality" since 1953 – for there is no guarantee whatever that the modest improvements in the personal security of Soviet citizens granted during the last two decades may not be suddenly taken away and the country plunged into a new bout of mass terror. Unfortunately the present rulers evidently take the view that they cannot afford any limitations to be placed upon their freedom to govern as they please. Far from accelerating the rehabilitation process inaugurated during Khrushchev's tenure of power, they have sought to suppress discussion of the whole issue. By taking police measures against anyone courageous enough to draw public attention to the régime's unacknowledged misdeeds, they apparently hope that the whole matter may lapse into oblivion.

It is up to scholars and journalists in other countries to insure that the memory of these innocent victims of police persecution is kept alive. Though little can be done on behalf of the nameless millions, there is at least some published information about members of the elite: politicians, senior officers of the armed forces, economic managers, cultural figures, and the like. The purpose of the present volume is to make such material available in English translation as a reference work for students of Soviet affairs. It is a kind of necrologue of those individuals whose reputations the authorities have thought fit to redeem. Introducing his theme, Boris Levytsky comments on the differing treatment meted out to persons in various categories. Among cultural figures, who suffered losses comparable with those of any other group, relatively few have been granted formal and complete rehabilitation. The probable reason for this, one might suggest, is that they do not have behind them a lobby as strong as that of the military, who have been the most successful in wringing concessions from those who decide such matters. Levytsky is right to conclude (p. 33) that the present generation of Soviet functionaries "has succeeded not only in dissociating itself from Stalin and his system but also in containing de-Stalinization within certain bounds"; he exaggerates, however, when he goes on to assert that "the hierarchic structure of the Stalinist government system . . . allow[s] for communication between the leadership and the people, but only in one direction: vertically down. No provisions exist for a reverse flow." There is a good deal of evidence for the existence today of informal pressure groups, some of which can occasionally influence the decision-making process. This was not the case during the *Ezhovshchina.* Slight as this difference is, it has considerable potential importance.

The body of this volume consists of some 400 items of information on 234 purge victims. (It may be recalled that at the Twentieth Party Congress in February 1956, Khrushchev said that 7679 persons had been rehabilitated; the total number is unknown.) These items are drawn from biographical entries in encyclopedias, collections of memoirs, and historical monographs, most of them published in the early 1960s. Levytsky has made a thorough search of the relevant literature, including the regional press. It is not his fault if the material, which for the most part has a hagiographical flavor, makes unpalatable fare for the nonspecialist Western reader, or if it tells us little if anything about the circumstances that led to these individuals being "repressed" (to use the official euphemism). In the nature of things, most of the evidence that has been published relates to earlier phases of the victims' careers. It is a pity, however, that Levytsky did not take the opportunity to give full bibliographical details of the sources from which this selection was made, or to provide a scholarly commentary. It would have cost little effort to have resolved obvious inconsistencies in the texts as published: for example, V. A. Antonov-Ovseenko's date of birth is given variously as 1884 and 1883 (pp. 88, 80); and V. G. Knorin is said to have died in 1937 and 1939 (pp. 243, 249). Surprisingly, no use is made here of the excellent work on the purge by Robert Conquest, nor for that matter, so far as can be seen, of any other Western source. Still more regrettable is the large number of inaccuracies that have been allowed to creep in during translation. In the first ten condensed biographies of military leaders taken from the *Sovetskaia istoricheskaia entsiklopediia,* I counted no less than twenty-one errors, some of them relatively minor (dates, initials, etc.) but others more serious. Omissions have been made without any indication of the fact, and whole sentences are jumbled. Thus in the case of S. V. Kosior we are not told that between 1928 and 1938 he held the post of general secretary of the Ukrainian Communist party, although this is clearly stated in the source. The thirteen-day difference between the Gregorian and Julian calendars frequently causes Levytsky and his team of editorial assistants to stumble, so that the celebrated meeting of the Bolshevik Central Committee on October 23, 1917 (i.e., October 10, 1917), which endorsed Lenin's insurrectionary strategy, becomes "the session of the Central Committee which was held October 10 to 23" (p. 200). These elementary lapses cast some doubt on the scholarly quality of the work as a whole, despite its prestigious imprimatur; they are particularly unfortunate in a volume designed to set straight the historical record which for political reasons has been tampered with by the Soviet censors.

Nevertheless it would be wrong to conclude on a carping note. This volume should encourage further work along the same lines. Ideally, in this electronic age one would hope for an exhaustive system of biographical information on all Soviet personalities past and present, which would be of inestimable value to researchers in many disciplines.

JOHN KEEP

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Deutschlands Doppelte Zukunft. By Peter Christian Ludz. (Munich: Carl Hanser Publishers, 1974. Approx. $3.00.)

By now it is no longer fashionable to argue for the academic discovery of the GDR since an impressive, and constantly increasing, number of works on East German politics and social structure are available. This consideration bears special weight in West Germany. Perhaps too much stress is placed on individuals in attempting to determine how traditions of discourse get started, but Peter Christian Ludz's work on the GDR from the middle 1960s to the present cannot be underemphasized. Beginning with the first systematic, and quasi-empirical, analysis of East German social structure, his most significant contribution undoubtedly lies in the field of elite recruitment and ideological change. *Parteielite Im Wandel* (Cologne and Opladen, 1968) is a masterpiece which probably never will be eclipsed.

Deutschlands Doppelte Zukunft is Professor Ludz's first attempt to link East Germany's evolution into a stable political system with parallel events in the Federal Republic. Though his latest book does not proclaim itself to be a theory-building exercise, the "intra-German" dimension easily fits within the more general rubric of "divided-nations" research which is gaining increasing attention in Western European and American social science circles. His yearly commitment to the publication of Federal Reports, *Berichte zur Lage der Nation,* on the nature of social, economic, and political development in the two German states has made this latest book a logical intellectual step.

Though the writer divides his analysis into five interrelated sections, there are two general foci around which the examination turns. In the first case, Ludz carefully examines the "domestic" dimension of political change in the GDR and Federal Republic. In the wake of several reverses in recent state elections, plus the "Guilliame affair," one is forced to take issue with Ludz's glowing account of the Left-Liberal's track record on the domestic level; his discussion on the relevance and impact of Brandt's Ostpolitik goes down far easier, however. For the GDR, Ludz supplies several interesting perspectives. While properly crediting the ruling Socialist Unity Party (SED) with impressive achievements in the realms of economic planning and a more limited degree of political stabilization after August 13, 1961, he contends that a serious gap exists between the regime's ability to engineer social change and its timid attitude toward challenging the outdated assumptions of official Marxism/Leninism. He concludes with the observation that "spontaneous process of adaptation (Lebensbewältigung), and social change more generally, are limited and canalized by the SED leadership. The modernization process is thereby delayed" (p. 87). Instead of devoting critical attention to the challenge of "refunctionalizing" the Party's operative ideology, Ludz suggests that the primary means employed by the regime to instill mass support for its goals is through the medium of foreign policy. Especially in light of the "recognition wave," which has now peaked, the rather crude attempt by the Party and govermental apparatus to sell its legitimacy to the population by proudly referring to the most recent opening of a new foreign embassy in East Berlin is the best example of this tendency.

The second focus of analysis revolves around the intra-German relationship which has become increasingly visible and dynamic in light of the *Basic Treaty*. Summarizing the conflicting positions taken by the West and East German governments on the "national question," Ludz devotes central attention to the Left-Liberal policy toward reunification with the GDR. In this vein, he argues that the most pragmatic, and ultimately most successful, position for the Bonn government was the one enunciated by Brandt in the fall of 1969. Having successfully overturned the discredited Hallstein doctrine, the Left-Liberal coalition has developed a policy which attempts to overcome gradually the political and psychological distance separating the two German states. This position, so argues Ludz, at least allows for an intelligent compromise between accepting the reality of national division while maintaining a cautiously optimistic attitude toward a gradual, and as yet unarticulated, transformation of the division in line with broader developments in superpower positions toward divided Germany.

The overall value of this effort should not hide two of its weaknesses. In the first case, Ludz tends uncritically to dichotomize models of West and East German citizenship to conform with an argument he has earlier criticized for being theoretically and empirically inadequate: Thus, West German civic culture is open and spontaneous, while East German civic culture, and the roles citizens are called upon to play in it, is controlled and manipulative. True, but the complexities of both systems transcend these relatively simplistic observations. Second, despite an attempt to make the book relevant for policy makers and analysts, the writer never supplies a clear and definitive outline of how reunification *within* the Brandt/Schmidt framework is to be achieved. Perhaps this last criticism is unfair, yet the author promises to contribute something in this admittedly slippery area. He never does.

JOHN STARRELS

George Washington University

No Peaceful Way: The Chilean Struggle for Dignity. By Gary MacEoin. (New York: Sheed & Ward, Inc., 1974. Pp. vii, 230. $6.95.)

Salvador Allende's election as Chile's president and the military coup that brought his administration to an end three years later have spawned hundreds of publications about Chile. Much of this literature has been biased either in favor of or against Allende. This book, which purports to be an analysis of the reasons for Allende's election and overthrow, is no exception. In content and tone it tends to be a work of political propaganda rather than a scholarly analysis.

While Allende's hope of turning Chile into a socialist democracy through constitutional means was a highly laudable goal, many of the means employed by the Popular Unity government to achieve this goal were not as praiseworthy. But, instead of a balanced critique of the "Peaceful Road to Socialism," including an analysis of the Popular Unity government's errors as well as the plots hatched against the government by groups like ITT and the CIA, MacEoin attempts to convince the reader that the Popular Unity government represented Chile's salvation and failed through no fault of its own. In attempting to prove this thesis, MacEoin, who is a freelance writer and holds a Ph.D. from the National University of Ireland, draws an oversimplified and misleading picture of Chile before, during, and after Allende. For example, he charges that during the Frei administration ". . . the army was called out to shoot down obstreperous workers and peasants" (p. 35), while even during 1972 and 1973 Chile's middle classes enjoyed four- to six-course meals twice a day and "meat meant only steak." Further, "the most ordinary middle-class family could count on having at least one maid, usually a Mapuche Indian girl paid next to nothing and often treated half like a pet, half like a beast of burden" (p. 129).

MacEoin also presents Allende's 1970 victory in a way that leaves the reader with a misleading impression of the nature and extent of support in Chile for the policies of the Popular Unity government. For example, he asserts that in 1970 Allende ". . . took office as President with a mandate to lead his country to socialism by constitutional methods" (p. 1). In point of fact, Allende had no such mandate. He received only 36 per cent of the popular vote in 1970, three per cent less than he received in 1964.

Concerning the Allende administration's policies in the countryside MacEoin asserts that the slight increase in agricultural production in 1971 was due to the Popular Unity government's agrarian reform policy (pp. 120 and 130). He does not explain that the same policy was responsible for a sharp decline in agricultural production in 1972 and 1973. These are just a few of the many half-truths in this book.

MacEoin spends many pages attempting to place the blame for the failures of the Popular Unity government upon multinationals, U.S. opposition, the CIA, and right-wing plots within Chile. The author spends little time examining the many fatal mistakes in political and economic policy made by the Allende administration itself. In point of fact, much of the opposition to Allende was provoked by the actions of left-wing extremists within his own coalition. Hundreds of illegal take-overs of farms and factories, and other violations by the Allende administration of both the letter and spirit of Chilean democracy, produced a backlash of uncompromising opposition to the government among many segments of Chilean society. MacEoin's description of the illegal takeovers of farms by peasants as a response ". . . to the most advanced revolutionary ideas . . . without concern for legal *details*" (emphasis mine, p. 27), illustrates an attitude toward Chile's legal system that prevailed among many government officials during Allende's administration.

Certainly, uncompromising foreign and domestic opposition all played a role in contributing to Allende's problems. But so did the Popular Unity government's own actions and policies. By virtually ignoring the latter while exaggerating the former, MacEoin has written a book which is essentially a work of political propaganda.

EDWARD GLAB, JR.

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Soviet Government: A Selection of Official Documents on Internal Policies. Edited by Mervyn Matthews. (New York: Taplinger Publishing Co., Inc., 1974. Pp. 472. $30.00.)

This is a collection of ninety-two original documents on internal policies of the Soviet government. Sixty-four of these already have appeared in English translation in various sources, and the remainder have been translated by the editor. The publication complements an earlier and similar work, *Materials for the Study of the Soviet System*, edited by James H. Meisel and Edward S. Kozera (Ann Arbor: The George Wahr Publishing Co., second edition, 1953). Only one document from the Meisel and Kozera collection is included in this latest compilation. Slightly more than half the documents gathered by Matthews date from the period 1917–1952, with the remainder on policies for the years following.

Useful but very brief introductory notes place the documents in their historical context. Although the notes are not intended to provide any semblance of textual continuity, they broaden the book beyond a mere collection. The reader is directed to the many available works for further explanation.

The editor recognizes that the documents represent his judgment about what is important, and he generally succeeds in fulfilling his selection criterion: to choose documents which "embody permanent elements of the Soviet system or which characterized government policy over long periods of time" (p. 13). Documents are divided into five parts: (1) government and administration, (2) the party, (3) legality, the courts, and the police, (4) the peasant and the land, and (5) the worker and his labor. Under the umbrella topic of the first category, the editor has included several documents related to religious policy and industrial policy – areas that may well have been expanded to include more documents deserving separate treatment. Moreover, Western social scientists may find the book lacking because of the failure to include a section on major social policy areas such as the family, culture, and education. Recent Western studies and textbooks are demonstrating a growing attention to these areas, and the ready availability of supplementary documents in translation would be useful.

The editor correctly recognizes the lacunae in sources on internal policy of the Soviet Union, and notes that what has purported to be "official" Soviet government policy might be supplemented or supplanted by other unpublished documents that more nearly reflect official Soviet policy.

Despite these weaknesses, the volume has brought together many important Soviet documents from scattered sources. Libraries and researchers should find this work a frequently consulted addition on their reference shelves, and teachers, especially of Soviet history, government, and economics should find it a helpful textbook supplement.

KENNETH N. CIBOSKI

Wichita State University

Sovremennaia klassovaia struktura SShA (The Contemporary Class Structure of the USA). By Anatolii N. Melnikov. (Moscow: Mysl Publishers, 1974. Pp. 301. Rubles 1.24.)

Although consideration of the class struggle has been the basic component in the Soviet analysis of American politics for decades, it was only in the 1960s that the first detailed Soviet studies of the class structure itself were published.

Melnikov's book, the latest of its kind, is the most comprehensive, sophisticated, and advanced Soviet study. He has attempted to synthesize all previous Soviet and much American research on class, occupation, and income in order to fill theoretical and substantive information gaps, make distinctions, sharpen definitions of terms and arrive at more precise approximations of the numerical size of each class, stratum, "interstratum" *(prosloika)*, and even smaller groupings in America.

The results of these operations are aggregated in a table (p. 299) quantifying membership in the three major classes, something only very rarely essayed by Soviet scholars heretofore. In 1971, of a total 84.1 million persons gainfully employed in the civilian workforce, 56.7 million (67.4 per cent) were in the working class, 23.9 million (28.4 per cent) were in the petty bourgeoisie and the other components of the middle class, and 3.5 million (4.2 per cent) belonged to the "grand bourgeoisie" constituting the upper classes. In his detailed analysis of each class Melnikov creates a complex picture reflecting the heterogeneity of social life in the United States. The component elements of each class are identified and quantified, and recent trends affecting the composition of each element are analyzed.

Within the working class, for example, he reports that the proportion of blue-collar production workers has been declining and the proportion of the white-collar office, store and leisure industry workers has increased. Melnikov considers most white-collar workers to be proletarians for two reasons: They do not own the means of production so must hire themselves out, and the work they perform is becoming more and more like the kinds done by blue-collar workers; that is, they are becoming adjuncts of machines.

Yet, even though Soviet analysts term this situation an "objective social condition," the subjective social consciousness of white-collar proletarians remains very petty bourgeois lower-middle-class, according to Melnikov. The same is true of those among the blue collars who once owned small and medium farms – these former members of the rural petty bourgeoisie have been proletarianized in terms of having lost their ownership and having become factory workers, but their past social consciousness has to a large extent remained.

These differences in consciousness, Melnikov asserts, produce variations in perceptions, attitudes, and even actions which inevitably have a negative effect on the proletariat's unity and result in what many Soviet writers have called the political passivity of the working class. In all, Melnikov enumerates seven causes of that passivity and implies that they will not be overcome easily or soon.

Similar analyses are made of the "intelligentsia" (scientists, managers, technicians, physicians, attorneys), the rural proletariat, and the capitalists.

In Melnikov's book we have an example of a narrowly technical study which nevertheless has some broad theoretical and practical implications regarding the course of the class struggle in America. Melnikov does not himself draw these implications, but others assuredly will. His message seems to be that on the basis of somewhat refined Soviet Marxian perspectives, at any rate, the end of the existing American political system seems to be quite far down the road.

Other Soviet analysts view the matter differently, but their disagreement will have to be expressed cautiously because of the awesome sponsorship of the book under review. The volume was published under the auspices of the Main Editorial Office of Textbook Literature, an arm of the Communist party's Central Committee, which normally puts its approval stamp on only the most orthodox writings. Perhaps all that indicates a significant broadening of Soviet perspectives in an area crucial to Marxist-Leninist theory.

RICHARD M. MILLS

Fordham University

The Internal Organization of Ch'ing Bureaucracy: Legal, Normative, and Communication Aspects. By Thomas A. Metzger. (Cambridge, Mass.: Harvard University Press, 1973. Pp. 469. $20.00.)

The third generation of scholarly interpretations of China's imperial bureaucracy is now appearing. The first generation, of which Max Weber was but one of many representatives, featured broad comparative conclusions, highlighting either the precocity of this long-evolving merit-based organization or the venality to which it had sunk, in part through an alleged stifling impact on commercial initiative. The second generation, marked by a resurgence of publications during the late 1950s and early 1960s, provided essential details for reinterpreting both ideal and actual patterns and for explaining the bureaucracy's position within some wider environment. Many of the important writings on premodern Chinese social structure belong to this generation of studies, including analyses of extensive social mobility through the examination system and of imperial control achieved in local areas with few personnel. Now the new generation eschews treatment of the environment in which the bureaucracy operated, instead focusing intensively on aspects of the bureaucracy's internal structure. Thomas Metzger's book typifies this new breed in relying heavily on a specific body of primary sources chosen from the wide assortment of Chinese administrative publications, in restricting the time perspective primarily to the first two-thirds of the Ch'ing dynasty (1644–1911) and in widely employing contemporary social science concepts. Drawing on detailed information concerning the relationship between law and bureaucratic organization, Professor Metzger refutes oversimplified impressions of China's long-standing governmental system that have lingered from the earlier generations of writings.

Metzger's combativeness is targeted against those who have portrayed the Ch'ing bureaucracy as rigid, tradition-dominated and subjugated by the emperor's arbitrary use of coercive sanctions and its officials as lacking an ethic capable of both motivating responsible behavior and defining inadequacies in the existing political order. Basing his arguments on a close study of laws dealing with administrative discipline, he emphasizes sources of flexibility, practicality and motivation to perform effectively in office. Above all, he stresses the universalistic application of sanctions independent of the status of the offender. Essentially Metzger steps down to a level of generality involving detailed descriptions of how laws in particular contexts were made, communicated, and applied to expose the superficiality of sweeping generalizations pertaining to a timeless Chinese bureaucratic system.

With the exception of sinologists engaging in the same field of research, one's attention span can easily be overtaxed by any attempt to read this book through. To counterbalance one-sided conclusions, Metzger concentrates on how complex things really were. Of course, variables such as administrative flexibility cannot be easily reduced to simple measures and do merit a multifaceted approach including study of such factors as: (1) the frequency with which law books were revised, (2) the diverse ways in which historical precedents were used, (3) the degree of clarity in the bureaucratic language, (4) the amount of specialization in the communication of laws, and (5) the frequency and severity of sanctions used to control the performance of civilian officials. This book provides the fullest examination of these and other factors available anywhere. Reading it tells us a great deal about legal aspects of Ch'ing bureaucracy, but much of the information is difficult to relate to general themes. For instance, the preoccupation with nuances in administrative vocabulary is reflected in the large fifteen-page glossary incorporating approximately 750 terms (exclusive of personal names) introduced in the text.

Problems of integrating specifics with generalities and sinology with the social sciences have become acute for this third generation. In this book, much of the social science terminology and discussion of contending arguments appears in the introduction and conclusion, leading to a certain bifurcation between the bulk of the material concentrated on specifics and its most reflective parts. In two other respects, the scope and structure of the book are not entirely adequate for answering general questions of interest to social scientists. First, the author does not steer a clear course between ideal patterns of behavior as reflected in legal codes and actual patterns as seen in the actions of officials. While the book mostly clings to the ideal side, it cannot avoid straying; at the end of sections and chapters conclusions based on ideal patterns are often modified with brief, but insufficient, references to existing conditions. I would have preferred more attention to actual patterns, e.g., in the discussion of how laws on administrative punishment influenced civil officials it would have been appropriate to analyze in greater depth the actual impact of the system of personnel management on individual careers. Second, conclusions (often phrased as criticisms of existing generalizations) about the Ch'ing bureaucracy cannot be properly evaluated because of the absence of comparisons over time and especially across national boundaries. To argue that the Ch'ing bureaucracy is relatively flexible raises the old question: compared to what?

By now, social scientists should be aware that broadly based comparisons of formal organizations ought to give high priority to the Ch'ing bureaucracy as an unusually well-documented, large-scale premodern case. Metzger's book contributes significantly in adding to our detailed knowledge of that bureaucracy and in opening the way for a new generation of studies, which now can generalize on a firm empirical foundation and revitalize the pursuit of explicit comparative objectives.

GILBERT ROZMAN

Princeton University

African Aims and Attitudes: Selected Documents. Edited by Martin Minogue and Judith Molloy. (New York: Cambridge University Press, 1974. Pp. 400. $19.50.)

This collection of documents is an eclectic sampling from the works of political and social leaders in Africa. The wide-ranging collection contains ninety pieces culled from the writings of a wide variety of writers and organizations: Fanon, Nyerere, Banda, Keita, Senghor, the OAU, various liberation movements, and so on. The selections are grouped into five parts: colonialism and decolonization, nation building, pan-Africanism, the continuing struggle, and political attitudes of the military. The value of the collection rests primarily on the intrinsic importance of some of the selections (for example, the "Arusha Declaration") and on the breadth of opinion represented. Nearly all the articles chosen were written in the 1960s.

Although the book lacks a common theme and presents few selections in ways to highlight the major differences in African attitudes, its claim to importance is credible. The inherent value of the words penned by historically influential Africans, argues Minogue in his introduction, compels us to give them serious attention. He states: "What a man of power thinks, both publicly and privately, will enable us to understand – even predict – his political actions; and it would be wrong to say that what he thinks publicly is less significant in this context than what he thinks privately, for such a stance disregards the fundamental role of the political actor in social life, which is to provide a public channel for publicized actions which affect the public interest" (p. 12). Minogue, a journalist and institute director, is right; and he and his co-editor, Molloy, have rendered us a service in pulling together this collection of the symbolic expressions of Africans.

Unfortunately the collection has serious flaws. Its purposes are not entirely clear. The political documents are heavily skewed toward East Africa, while literary expressions represent principally the West African traditions. The pieces chosen do not seem based on consistent purposes: is the collection to be representative; is it to contain the best (most influential) statements of opinion? Neither criterion can be said to be satisfied. The excessive emphasis on Idi Amin's ideas (three of eight statements on the military are by him) reflects curious taste. In addition, the organization of the articles appears haphazard, as if the five parts of the book emerged largely after the selections were chosen.

A second flaw is the failure to provide a rich enough context for the pieces. A good deal of the meaning and purpose of these documents is lost for any reader not already familiar with African history and politics. The brief introductions are entirely inadequate to give the reader the sense of relevance and historical setting of each selection. For instance, in reading the pieces under the "pan-Africanism" rubric one might be tempted to believe that the institutions of unity were waxing strong, whereas, notably in the case of East Africa, they are waning. The nine selections on institutions are close to useless; for the *cognoscenti* they do not serve as good references because they have been edited, and for the novice they are possibly misleading and probably incomprehensible. The book would have been enhanced by fewer individual excerpts and a larger contribution by the editors to set each contribution into a relevant context. Understanding the issues and historical forces which led up to a particular pronouncement or literary expression is vital in many cases to adequate comprehension.

The book closes with some useful sketches of the principal African contributors and a short bibliography. The volume may be valuable in conjunction with others as a source on African politics or for interesting reading by those generally familiar with African history. The excerpts from Kaunda, Nyerere, Fanon, Toure, and Senghor, as well as some of the documents from the OAU and various liberation movements, give the reader a taste of the themes and problems that have concerned Africans in the last decade. In some instances their writings have eloquently addressed the political and cultural needs of their society. In bringing together these various strands of African thought, the editors have pursued a valuable but elusive goal. Although their purposes are worthy, they are not clear. The criteria for selection, organization, and presentation that one might infer from the collection seem analogous to those governing a supermarket display counter. It would have been far better to choose articles as if they were keys that could open a vault. For those with adequate historical and theoretical currency, this book has an attractive range of wares, but those without such resources are likely to be dismayed. If there are common themes, shared purposes, or similar causes contained in African writings and documents found here, it will be the reader and not the editors who will have discovered them.

RAYMOND F. HOPKINS

Swarthmore College

Courts and the Political Process in England. By Fred L. Morrison. (Beverly Hills, Calif.: Sage Publications, Inc., 1973. Pp. 224. $10.00.)

Most works on non-American judiciaries reflect a formalistic tradition. Professor Morrison's lucid and concise political examination of the English judicial system contributes to the meager body of politically oriented comparative judicial studies. An Eastonian systems approach also enables Morrison to examine English courts in relationship to, not in isolation from, the overall political process.

An analysis of the sources and implications of the narrow English judicial role is the volume's principal theme and contribution. Morrison finds that judicial recruitment does not have a partisan bias and that English judges are recruited from a narrower social and professional base than American judges, phenomena which may reciprocally relate to a narrow judicial role. After examining the relationship of the courts to Parliament and to administrative, private, and criminal-law outputs, Morrison suggests that the judicial role is largely responsible for the limited impact of the courts upon the political process. In discussing the frequent service of English judges on Tribunals of Inquiry, Royal Commissions, and similar bodies, he observes, however, that a narrow role gives English judges broader and more influential extrajudicial roles than American judges have. Morrison also suggests that the narrow judicial role helps the Lord Chancellor reconcile his multiple judicial, political, and administrative roles.

One would anticipate a narrow judicial role in a system with parliamentary supremacy and a formalistic tradition in legal education and with no federalism, no separation of powers, and no written constitution. The volume, however, leaves the impression that Morrison has not systematically employed evidence but has provided examples that best prove his case. Unanalyzed is the problem of the law-making activities judges can or do perform within a confining role. Except in the chapter on criminal law, the reader is not told how much freedom English judges have to make or alter policy in distinguishing precedent or interpreting statutes. Morrison refers only briefly to either the antagonism of English judges toward labor unions and the social welfare state or to Lord Devlin's views regarding a more active judicial role. His discussion of county courts appears to contradict itself. Morrison asserts that county courts interpret statutes and common law to make "almost all of the law relating to evictions and protection of tenants, etc. . . ." (p. 51). Of these courts he later observes that precedent severely limits the "possibility of argument for legal and social change" (p. 155).

One of the author's main objectives is to compare and contrast the English and American judicial systems. Because this comparison is not often explicit, the volume loses much of its intended comparative emphasis. (Indicative of this is that only about 12 of 296 footnotes refer to American judicial sources.) For example, in stressing that English judges lack the power of judicial review, Morrison fails to observe that American judges have virtually abandoned judicial review of Congressional socioeconomic legislation. Thus Morrison may leave the unintended impression that the formal power of judicial review is greater than it is.

Morrison's second comparative goal is to define the adjudicative, as distinguished from merely the judicial, process by employing structural-functionalism. Loosely using the terms "functional" and "functions," he does not develop systematically such a framework. What does it mean, for example, to compare the "functions" of the English and American judicial systems to assist "in determining the core meaning of adjudication" (p. 24)? Only in discussing administrative adjudication does Morrison apply functionalism, and then merely by observing that, in advanced societies, certain types of adjudication are "functional," even if not performed by courts.

Morrison's purpose is to provide a political analysis, not to write a text on English court structure and jurisdiction. Without inundating the reader with fine detail, he clearly describes both the court structure, including changes implemented between 1970 and 1972, and the legal profession. A fuller discussion of the political implications of the bifurcated English legal profession, however, would complement Morrison's laudable goal of emphasizing political analysis. Appointment to all but the lowest courts is reserved to barristers, and entry to the English bench and bar is more restricted than in the United States. Yet Morrison does not discuss the possible introduction of conservative social and political bias to the judicial system through the restricted English legal profession.

Miscellaneous flaws range from the author's moderately serious failure to discuss research procedures to the minor but inconvenient failure to include an index. Morrison relies heavily on conventional library resources and English legal materials as well as, apparently, his own direct observations for data. At times it cannot be determined whether statements result from the author's observations or from other sources. In discussing the lack of judicial review in England, Morrison also omits considering the lack of federalism as a possible explanatory factor.

Morrison's volume is the third in a Sage series, under the editorship of Joel Grossman, of eight projected volumes on politics and the legal order. Two of the remaining five volumes are to deal with the Indian Supreme Court and the Federal Constitutional Court of West Germany. If these volumes maintain Professor Morrison's political perspective, a foundation will have been laid for a truly political comparison of judiciaries.

JOHN E. STANGA, JR.

Wichita State University

A Japanese View of Detente. By Hisahiko Okazaki. (Lexington, Mass: Lexington Books, 1974. Pp. xi, 117. $12.00.)

Mr. Okazaki is a diplomat in the Japanese Foreign Ministry who served as chief of its Analysis and Research Divisions in the latter half of the 1960s. The original version of this book, in Japanese, was published in 1971 as *Kincho-Kanwa Gaiko* (Detente Diplomacy). It is unusual for a Foreign Ministry official to publish a book in mid-career. It is exceptional if such a book goes beyond the level of bland generalization. Fortunately, Mr. Okazaki is unusual – for his writing is straightforward and frank.

Despite the complexities and ambiguities inherent in the word "detente," the focus of this study is relatively narrow. "In this analysis, everything revolves around the problem of how to deal with Communist states" (p. x). This means that the author is only marginally concerned with Japanese-American relations. This subject has an extensive literature so that it is helpful to have a Japanese address himself, in English, to an entirely different set of problems. Nevertheless, it would also have been interesting to obtain the benefit of Mr. Okazaki's views on problems other than how Japan might best cope with China and the Soviet Union.

Diplomat that he is, Mr. Okazaki is careful to eschew making prescriptive policy statements couched within the framework of any grand designs. He is cautious, and prudent – and, in doing so, provides excellent evidence that he has devoted himself seriously to the study of post-World War II Russian foreign policy and even more so to that of the People's Republic of China. Regarding the Soviet's peace offensive in 1949, he asserts that it "should be more accurately described as tactics than as an actual detente policy" (p. 7).

After tracing the evolution of the PRC's foreign policy, he concludes that "in deciding its foreign policy, domestic motives have been far more important than the external environment for China, and much more so in China than in other countries. Therefore, it is dangerous to assume that China will change its attitude if we change our attitude" (p. 40). On this score, too, the author's views can be contrasted sharply with the efforts of some specialists in international relations, for whom the determinants of a nation's foreign policy seem to exclude its internal politics.

One of the most incisive sections of the book is the author's discussion of various convergence theories, from both sides of various fences. Convergence is possible, but before foreign policy planners become too enthusiastic, it would be best to juxtapose its prospects with the continued commitment to ideology, more on the Communist than the other side. In other words, given the existence of ideological education in the USSR and the PRC, there is also always the possibility of "a new surge of passion in the Communist countries, with considerable effect on East-West relations" (p. 77). He is, however, silent on the possibility of other forms of passion sweeping non-Communist societies.

In his chapter on Japanese foreign policy, Mr. Okazaki reflects his government's low posture vis-à-vis the major powers. Despite Japan's economic power, its capacity to influence questions of peace and war will remain smaller than that of China, and on questions of local conflicts, its weight will be less than that of India or Sweden. In a rare, and therefore all the more meaningful, excursion into the prescriptive realm, he concludes that "Japanese foreign policy should seek its national interest and national objective within limits, sometimes taking advantage of the existing international situation and future trends, rather than setting up an *a priori* national objective and expecting an ideal form of international society united to that objective" (p. 81).

Possibly the most important aspect of this book is that Mr. Okazaki has dealt with the national security of Japan within the broad framework of relations among the major powers of Asia. Furthermore, he has done so by interrelating political, economic, and defense issues. All too frequently, Japanese analysts have considered foreign policy within the context of bilateral rather than multilateral relations and have sought to separate politics and economics, in part because of bureaucratic separatism within its domestic policy making processes. Mr. Okazaki has succeeded in synthesizing these various elements. Hence, his effort charts new territory and is therefore particularly valuable.

The Atlantic Council of the United States is also to be congratulated for sponsoring this publication in English. Our only regret is that its price will preclude its extensive use in classrooms.

KATSUMI KOBAYASHI
HANS H. BAERWALD

University of California, Los Angeles

Le crisi della modernizzazione: L'Esperienza del Brasile e dell' Argentina. By Angelo Panebianco. (Napoli, Italy: Guida Editori, 1973. Pp. 241. L. 3600.)

This book offers us a comparative analysis of the crises of modernization as they took place in the two largest and most economically advanced states in South America: Brazil and Argentina. It is the author's hope that lessons derived from this comparative study may be employed to develop hypotheses which may in turn be applied to, and tested in, other Latin American political systems.

The central theme of the work is the response of the two political systems to such developmental crises as the crisis of state-building (subsuming the crisis of penetration and the crisis of integration); the crisis of nation-building (subsuming the crisis of identity and the crisis of legitimacy); and the crises of participation and distribution. More specifically, how have the ruling elites responded to these crises and what impact have their responses had on the development of the respective political systems? As we can see, Dr. Panebianco is relying very heavily on political development models furnished by Almond and Pye.

After an interesting historical introduction, in which he shows how different patterns of colonial experience helped to shape the early history of the two independent states in the nineteenth century, Panebianco then proceeds to discuss the relative tardiness of Brazil in dealing with the crisis of state-building – a crisis which Argentina had resolved by the 1880s, but which Brazil did not successfully overcome until the 1930s. Partly because of the existence in northeast Brazil of a vast underdeveloped region dominated by local clienteles, partly because the ruling elites of southern Brazil were not powerful enough to govern the country alone but had to seek allies and make concessions in the traditionalist North, Brazilian modernization lagged behind that of Argentina. Such indicators as literacy, urbanization, and per capita income are particularly revealing with regard to this socio-economic lag.

Consequently when the crises of distribution and participation erupted in the early 1930s, Argentina had already resolved the problems of state-building and nation-building, whereas Brazil had to cope with a veritable cumulation of crises. According to Panebianco, this difference in levels of development and in historical experience helps to account for the far greater susceptibility of the Brazilian military to foreign economic influence. It also helps to explain the different fates that befell the Peronist autocratic system in Argentina and Vargas's Estado Novo in Brazil. Both dictatorships were established in response to the economic depression of the 1930s, were eventually overthrown, and were replaced by more or less democratic regimes under the constant tutelage of, and threat of intervention by, the military. But as the author points out, the higher degree of social mobilization in Argentina has made it impossible for the military to block the revival of Peronism. In Brazil, on the other hand, the military has succeeded in implanting its own dictatorship, thus successfully resisting the efforts of the "national bourgeoisie" and the working class to establish an economic system relatively free of foreign domination. In fact, the author perceives the present Brazilian military regime as being based on almost total support for the interests of the "dependent bourgeoisie" (dependent on foreign, and especially American, centers of economic power).

At this point, as we can see, Panebianco is introducing a second model: a model of class conflict in a transitional society. Developing, transitional societies are in a state of economic dependence – a neocolonial relationship – vis-à-vis the more advanced countries. Certain entrepreneurial groups in such societies have a vested interest in attracting foreign investments and foreign imports, thus perpetuating the dependent relationship. These groups, described as "the colonial-fascist bourgeoisie" by some Brazilian writers, are given a more value-free title by Panebianco: "the dependent bourgeoisie." Other groups, such as "the national bourgeoisie" (medium and small businessmen who demand protection from foreign competition), the proletariat, and the intellectuals, tend to favor "a national path to development" (p. 167). The military and the urban middle class of white-collar employees and professional men tend to play the role of arbiters in this struggle between two competing approaches to the goal of economic development.

This book reveals both the virtues and the vices of macrotheory in political science. On the one hand, both the political development model and the class conflict model seem to impose a certain desirable order on a chaotic melange of political events and phenomena. The models seem to have a great deal of explanatory power – one senses that they fit the data to some degree, though to be sure, rigorous proof is out of the question. On the other hand, the reader cannot help feeling that much is being lost when politics is viewed through the powerful but narrowly focused lens of a macrotheoretical spyglass. The simplification of motives (largely economic); the reification of the actors (a series of usually monolithic social classes); the virtual absence of political culture, bureaucratic and military role perceptions, and purely personal motivations from the purview of our investigation – one wonders whether explanatory power is not being achieved at the price of excessive perceptual distortion.

RAPHAEL ZARISKI

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Social Democracy in Post-War Europe. By William E. Paterson and Ian Campbell. (New York: St. Martin's Press, 1974. Pp. 82. $6.95.)

As Western democratic political parties have converged ideologically, scholars have tended to examine parties universally, individually, or by country rather than by ideological family. William E. Paterson and Ian Campbell, political scientists from the University of Warwick, offer one of the few recent attempts to compare parties of a similar ideological persuasion. Their short book, written for the novice rather than for the expert, provides a brief but interesting survey of postwar developments in the social democratic parties of Western Europe. While the authors try to include all West European social democratic parties, they focus primarily on those in Britain, West Germany, France, and Italy.

Starting from the premise that the social democratic parties of Western Europe share a common concern with "reshaping society in the interests of the working class and other less privileged groups" (p. vii), the authors survey their historical developments, sources of electoral support, ideology, and foreign and domestic policies. They argue that apart from the common concern for the disadvantaged, the socialists of Western Europe lack a distinctive ideology or organizational style. A brief chapter on domestic policies even casts doubts on the socialist parties' commitment to improve the status of the working class.

Ideological convergence, experience in government, the acceptance of basically similar electoral styles, and the attempt to win the votes of each other's electorates have reduced the differences between the social democrats and their conservative rivals. Thus, a conservative French president, such as the late Georges Pompidou, could claim with a degree of accuracy that he too was a socialist. Indeed, the authors point to this evolution and its implicit threat of the loss of a social democratic identity as one of the main problems with which these parties must contend.

A book of only eighty pages that tries to cover social democratic parties in nearly a dozen countries of necessity can only offer superficial analysis. The authors do provide a good deal of interesting background information but can only hint at the even more interesting dilemmas and challenges that social democracy faces in the modern industrial society. For example, the ideological debates over nationalization and alternatives such as *autogestion* (worker self-management) are unmentioned. In other cases, important differences among these parties are glossed over. Many French socialists, for example, would angrily reject the social democratic label for their party. Social democracy is regarded disdainfully by them as an inferior and corrupted form of the pure socialism they advocate. In short, this book provides a brief introduction to European social democracy and highlights the need for a more comprehensive analysis of the recent developments and problems in the social democratic parties.

FRANK L. WILSON

Purdue University

Britain at the Polls: The Parliamentary Election of February 1974. Edited by Howard R. Penniman. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1974. Pp. 183. $3.00, paper.)

The British general election of 1974 was a two-act drama. The fall of the Tories happened in February, the somewhat reluctant acceptance of Labour came in October. Reviewing the play after the first act presents obvious problems. Nevertheless we have here a collection of capably written essays which give the reader much of the flavor of the first act. It is no substitute for Butler and Kavanagh and was certainly not intended to be. The essays are somewhat eclectic and thus disjointed. Nevertheless all of them are interesting, particularly Anthony King's overall view, Austin Ranney's discussion of candidate selection, and Richard Rose's article on the polls, a sensitive subject after the 1970 debacle.

Several common themes run through the various essays. The authors generally agree that the events of 1974 are symbolic of a broader and deeper crisis in the British political system, indeed perhaps of a morbidity in the system. Britain last had two general elections in 1910 in the midst of the turmoil which surrounded the displacement of the House of Lords and the elements that it represented in British society. There is no similar agreement among scholars on what is now happening. It would appear, however, that the political system is failing this time rather than changing. Conversely, it is possible that the society is failing and that fact is finally having an impact on the political system. In any case, the estrangement between society and system is certainly the most striking aspect of the events of 1974. That estrangement is reflected in the decline in support for the major parties. In February the Conservatives and Labour obtained 75 per cent of the popular vote, down from an average of 91 per cent in the previous eight elections. A second indication, although geographically limited, is of course the dramatic increase in Celtic nationalism, most particularly the advance of the Scottish nationalists. It has been a curiosity of recent European politics that as national identity has weakened somewhat it has been replaced in some cases more by a reawakened regional identity rather than by a European allegiance as many had hoped would be the case. Further, the estrangement between the political parties and the society is reflected organizationally in the decay of Labour party organization over the past twenty-five years, ably described here by Dick Leonard and in the increasing turmoil and dissatisfaction in the process of candidate selection. As Austin Ranney notes, the old days of automatic re-adoption of MPs as candidates are pretty well over. This dissatisfaction also manifested itself in the high Liberal vote, the Liberals serving as an electoral way-station to nowhere; this situation allows the voter to fulfill his civic duty without having to support either of the major parties.

All in all, it means that the British political system, admired by many and studied by a few, is not operating as the textbooks have led us to believe it should operate. It is a system in which the electoral process if nothing else is at least supposed to produce a clear-cut winner. That has not in fact happened in three of the last five elections. It is a system in which the leadership is supposed to be able to provide firm, purposeful direction. That is also not happening. The Tory campaign slogan in February, 1974, about "Who runs Britain?" remains to be answered. Clearly, it was not the Conservative government. Finally the system is supposed to operate in a manner which places a political premium on moderation. In fact, Tory politics in the 1970s have consciously embraced the politics of social confrontation and rejected the politics of accommodation practiced in the 1950s. That attitude was reconfirmed by the replacement of Edward Heath with Margaret Thatcher. Similarly the guard of moderate trade-union leaders who supposedly insulated and protected Labour leaders has apparently vanished.

In politics, morbidity is and always has been a particularly fascinating topic. Consequently this may be a particularly interesting time to be studying the British political system. This is not a terribly good book, but it is written about an extraordinarily rich period in the political history of Great Britain.

ROBERT C. FRASURE

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English Local Government Reformed. By Lord Redcliffe-Maud and Bruce Wood. (London: Oxford University Press, 1974. Pp. vi, 186. $9.75.)

Now that the dust has settled, it seems evident that the changes brought about by the Local Government Act 1972, implemented in 1973–74, have been effected in a smooth and reasonable manner, as is the custom with changes in the governmental structure in England. Although the new two-tier system is an advance on the former pattern, certain problems remain and it may take time and further modification to adjust the relationships between the two new levels of authority. Many officials in local government feel that a unitary system, with only nominal power being given to district councils, would be more effective than the present division of responsibility.

The main architect – or should it be midwife? – of this first comprehensive reorganization of English local government since the nineteenth century was Lord Redcliffe-Maud, Master of University College, Oxford. In turn, he was chairman of three committees inquiring into the work of local government, his most significant contribution being as head of the Royal Commission which reported in 1969, a report which led to the 1972 Act. Therefore, he is uniquely qualified to comment on the structure of local government in England at this time of transition. It is more than forty years since he first produced a text on local government, and his combination of academic study and knowledge gained from the detailed investigations of his governmental inquiry days has produced a concise yet highly informative account of the way in which services are provided and monitored in the localities of England (and Wales, also covered by the 1972 Act though hardly mentioned in the book). His co-author was Assistant Research Officer on the Royal Commission and now teaches at the University of Manchester.

Unlike, say, the USA, England has a highly unified political structure. Federalism, though occasionally aired as a possible solution to pressures from the regions and the major national groupings (the Scots and Welsh) in the largest of the British Isles, seems remote from a system where power is still heavily vested in Westminster and Whitehall. As the authors state, "local government *in modern England* is the creation of Parliament" (p. 10). What Parliament giveth it can also take away, but what concerned both central and local government officials, elected and appointed, during the post-1945 years, were the obvious flaws that had emerged in the structure of local government by the mid-twentieth century. As the authors point out in Chapter 3, boundaries were out-of-date, many authorities were far too small (this review is being written in what was once *the smallest* county borough *in* England, an independent local authority of only 33,000 people), the system was too complex and supposedly dependent on national government; public interest in local government was minimal.

The 1972 Act, according to the authors, was designed to eradicate at least some of these disadvantages. Like the report of the Royal Commission, it reflected a degree of compromise in the aims to be maximized. Large authorities (in population terms) might achieve economies of scale but would become remote from the ordinary voter. An optimal size of about a million people was suggested and, though mostly amalgamation was the order of the day, occasionally large county councils (e.g., Glamorgan in South Wales) were split up. The Royal Commission had recommended the bisection of Kent, but this was not accepted in the Act as passed through Parliament. London had been reorganized into a two-tier system in 1963 and three new "conurbation" areas (W. Midlands, Merseyside, Greater Manchester) were established by the 1972 Act. The rest of the country retained something like the old county council as the senior local authority with a junior "tier" of district councils complementing them and together constituting a much simplified version of the older and more complex system; the county borough disappeared entirely, for example, as a unit of local government.

The authors review the working of this new framework, its division of responsibilities between each level, the work of its officers, the financial structure and its relationship with both central government and the English courts of law. This is a matter-of-fact, rather cool book. We are not taken behind the scenes to any great degree, not even into the debates which must have occurred in the Royal Commission during Redcliffe-Maud's relationships with the leading politicians and civil servants of the day. What we are left with is a sound basic work of reference for all those who wish to know how the English govern themselves in their localities. It remains for others to suggest more radical alternatives to the present structure designed to carry the country into the next century.

ALEC BARBROOK

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Separatism Among Indian Muslims: The Politics of the United Provinces' Muslims, 1860–1923. By Francis Robinson (New York: Cambridge University Press, 1975. Pp. 469. $27.50.)

Research on Indian political history has tended to focus on Imperial policies and a few key persons, events, or organizations. During the last decade, however, a new trend has developed, with attention to subnational patterns and a re-evaluation of the indigenous elements involved in politicalization. This fresh study of Muslim politics reflects such new concerns. Francis Robinson gives a connected picture of seven decades of Muslim activism in one region, the United Provinces in North India. He attempts to deal with a range of major issues such as the following: Why and how did religion define political identity and consequent alignments? Who was instrumental in that process, and how did the new politicians operate? Finally, how did British rule contribute to the patterns, and what does the interaction between ruler and ruled tell us about the building blocks of Indian politics?

Robinson begins his study with a short sketch of Muslim conditions in the U.P. around 1870. After demonstrating the existence of an Urdu-speaking elite that combined leaders from both the Hindu and Muslim communities, he traces how economic and political threats disintegrated that rough grouping into communal factions. In the areas of education and employment, Muslims began to define themselves in terms of religious identification. The U.P. Muslim leaders played a major role, in moving their co-religionists from collaboration with government to a severe attack on the British in the form of the *khilifate* movement (c. 1914–1923). Throughout the period, distinct factions among Muslims interacted in an effort to formulate alternative strategies to the predicaments created by a complex political system. Although the *khilifate* agitation ended in failure, experience emerging in the decade of intense political mobilization contributed to new communal demands and ultimately the cry for a separate nation.

Despite frequent lapses in argument and the interjection of references recognizable only to specialists, Robinson has succeeded in providing an overview of Muslim elite politics. While admitting that the book will be a landmark in terms of detail and theory, however, one must add quickly that underlying problems mar the presentation and raise questions as to its long-range importance in re-interpreting Indian politics.

The rich source materials available for study of Muslim dynamics either have been used incompletely or misused. For example, Robinson taps few if any Urdu documents, and therefore an entire segment of historical documentation tends to be ignored. Moreover, in an effort to cover ground, Robinson does not treat several important published works, unpublished theses, or even a major newspaper on U.P. politics, *The Pioneer.*

Reliance on British records as primary sources on Indian politics also endangers the validity of Robinson's conclusions. On the one hand, he has not systematically examined the mass of proceedings generated by a communication network stretching from London to the district office. Information on critical turning points such as separate electorates are based essentially on viceregal correspondence and India Office files, for example, while the Home Political Proceedings in New Delhi remain almost ignored. On the other hand, Robinson appears to accept the judgments of British officers who commented on Indian trends. Official views on financing of Muslim papers, motives, and in-fighting within factions may have been accurate, but to rely extensively on intelligence reports and scattered British correspondence in such areas is suspect. Accumulating research on colonial decision making suggests strongly that the world-view of the bureaucrat colored his perception of what was happening within India.

Finally, specialists in other regions will find the assumption that U.P. Muslims played a central role in all-India Muslim politics at best a thesis that requires buttressing. Robinson often treats U.P. politics in isolation without taking into consideration apparently divergent patterns in neighboring provinces. Accumulating studies on communalism have not been integrated into the overall picture of Muslim politics. Consequently, Robinson has dealt with "politics of United Provinces Muslims" and at the same time raised questions about how local patterns related to all-India events. A syncretic study of "separatism among Indian Muslims," however, remains to be written.

N. GERALD BARRIER

University of Missouri-Columbia

The Last Exodus. By Leonard Schroeter. (New York: Universe Books, 1974. Pp. 432. $10.95.)

Although the Jewish dissident movement in the USSR has been prominently featured in the

Western media for some time now, this is the first comprehensive description and analysis of that remarkable and unanticipated phenomenon. Fortunately, this pioneering effort is a very good one. Leonard Schroeter, an American lawyer and civil libertarian who served as legal adviser to the Attorney General of Israel, has written a detailed survey of the movement among Soviet Jews in the late 1960s which has so far led to the unprecedented emigration of more than 100,000 Soviet citizens. He does not pretend to write a definitive history – it is too early for that – nor does he attempt to generalize about protest movements or even about political dissent in a Soviet-type system. His modest aim is to discover and explain the forces which created the movement, its strategies and tactics, its successes and failures, and its relationship to other dissident movements in the USSR. Schroeter's method was to interview extensively, both in Israel and the USSR, many of the leading figures of the movement and to read the large volume of Jewish *samizdat* material, some of which has not appeared publicly in the West. He succeeds admirably in integrating the life histories of individuals with the history of a movement, and tells a moving story of human individuals against a background of large social forces.

Though some Soviet Jews expressed their support for Israel and had tried to immigrate there even in the "black years" of Stalinism, a nationwide movement whose aim was *aliyah* (immigration to Israel) was formed only in 1969. Borrowing their tactics from the political dissidents ("democrats"), the Moscow Jewish activists organized an All-Union Coordinating Committee which included representatives from groups in Leningrad, Riga, Kiev, Minsk, Kharkov, and Tbilisi. In its geographic range, social and intellectual diversity, quality of leadership, and foreign support, this movement was uniquely advantaged among the variety of dissenting groups that have become a part of the Soviet scene. Schroeter describes the specific characteristics of the Jewish groups in several cities and shows how their strengths complemented one another. Riga Jews provided the Jewish knowledge and Zionist ideology which gave the movement its theoretical base; Georgia introduced the element of messianic nationalism; Leningrad supplied a cadre of militant activists whose courage was an inspiration to the others; Moscow "supplied the nervous system and the connective tissue." It was in Moscow "that the movement found expression and made its contacts with the West. . . . Without the response in the West . . the movement might have been ignored, neglected, and thus to a considerable degree rendered ineffectual" (p. 85).

As one reads the individual stories, two general types of Jewish activist emerge. The first is a person who usually comes from a Russified family, at some point questioned the cultural and political orthodoxies of the system, was drawn into the democratic movement, and then – in many cases after the invasion of Czechoslovakia – decided either that the struggle for political reform was hopeless or that the fight for *aliyah* was more germane. The second type, more frequently encountered in the Baltic areas, had engaged in Zionist activity as early as the 1940s and 1950s, had been imprisoned for such activities, and had emerged even more profoundly alienated from the system and more determined to get to Israel. Ironically, it was in the prisons and labor camps that many of the democratic dissenters got their first exposure to Zionist ideas.

The links between the democrats and the Zionists present certain problems to both groups. Both were obliged to consider the question of whether *aliyah* would weaken the struggle for human rights in the Soviet Union, as well as whether it was feasible and desirable to fight for both aims simultaneously. Schroeter sympathizes with both movements and is particularly sensitive to the delicate moral and tactical issues involved in their relationship. He is also able to detail the operational methods of the dissidents, the secret transcripts of court proceedings involving them, and their reflections on their own political activities.

Schroeter is an admitted partisan, though not a blind one, and he adopts the view of some of the more militant elements in the movement. He is critical of the Israeli government and of the Western "Jewish establishment" for deliberately keeping silent about many developments within the USSR and he disagrees with Israel's refusal to support political dissidents in the USSR. Perhaps he does not give sufficient consideration to Israel's role as a state with international responsibilities and does not take seriously enough the constraints within which she must operate, particularly in regard to the Soviet Union. Nevertheless, some of his criticisms seem well founded and there are some striking ironies which he brings to light. For example, those who attempted to hijack a plane from Leningrad in 1970 had written a "Testament" which explained their actions and aims, but the KGB as well as the Israelis suppressed its publication, each for their own reasons; so the Testament appears here for the first time in English.

For students of Soviet politics this book provides a detailed, documented, cogent discussion of the most successful of the Soviet dissident movements. For all people concerned with human rights it is a valuable introduction to a story which is still being written.

ZVI GITELMAN

The University of Michigan

Guide Politique de Picardie. Aisne. Oise. Somme. By Michèle et Jean Sellier. (Paris: Tema-Editions, 1973. Pp. 379. No price indicated.)

Most French political studies have focused on patterns of national politics and institutions such as the distribution of power between the executive, the legislature and the bureaucracy; political culture and behavior; elections and parties; and selected public policies. Only a minority of American social scientists have dealt with French local and regional politics, and even fewer have been concerned with the linkages between national and local politics. The latter will undoubtedly be disappointed by this *Guide Politique de Picardie.*

To summarize briefly the contents of this work, the reader will find a detailed historical description of electoral returns in Picardie since 1945 as a basis for discussing the tribulations and electoral fortunes of national political parties in the region. Another chapter examines, somewhat cursorily, the nature of the regional institutions which were set up by the Fifth Republic in the early 'sixties within the framework of its policy of administrative *déconcentration.* A final chapter is devoted to an effort to identify a specifically regional pattern of politics. Two appendixes provide concise demographic, social and economic data about each of Picardie's electoral constituencies as well as some biographical information about local political notables.

The Selliers' key argument is that regional economic and social conflicts are not translated into political ones. They are stifled by the predominance of a homogeneous agricultural population which enjoys a privileged political status because all other social groups, while representing a numerical majority, remain fragmented. Thus, there is no regional pattern of politics distinct from the mainstream of national politics. The capital overshadows the three *départments* of the region. The *Commission de Développement Economique Régionale* has no significant political power. In Picardie, only national parties count and the repeated successes of Center parties (e.g., the Radicals under the Third Republic and the Socialists and the Gaullists under the Fourth and Fifth Republics) are, in turn, presented as an outgrowth of a delicate and stalemated equilibrium at the regional level.

All of this is at best highly informative although it should be noted that most of the empirical material might also have been found in a telephone book, the *Bottin Administratif, Who's Who in France,* or the *fichiers* and publications of the National Foundation of Political Sciences in Paris. The main interest of the book lies in the authors' concluding and interpretative chapter. The points raised there may indeed very well be accurate. Unfortunately, none of them is adequately supported by the data presented in the body of the discussion. Specifically, I do not see how such sociological statements can be derived from a narration of electoral campaigns and returns. From the perspective of modern political analysis, this work suffers from a formalistic, historical, and static approach. The discussion is also limited by an unduly narrow definition of politics, understood primarily as conflictual processes taking place in an open and public arena. Consequently, the authors' conclusions are unaccounted for and unexplained.

To summarize, this is a well-documented sourcebook which presents useful though not necessarily original nor in-depth information. As a study of regional politics in France, it is cast in a traditional intellectual mold which develops only limited and occasional insights into the nature of Picardie's politics while failing to integrate its prodigious mass of factual material into a cogent explanatory framework. Hence a general impression of superficiality and ramblingness. The definitive work on Picardie's political life remains to be written.

JACQUES FOMERAND

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Southeast Asia: Documents of Political Development and Change. Edited by Roger M. Smith. (Ithaca, N.Y.: Cornell University Press, 1974. Pp. 606. $19.50.)

Here is a solid volume that contributes greatly to the area specialist's documentation on political affairs by presenting what Southeast Asians themselves have been saying. The collection will promote the tendency for political scientists to take more seriously how the political actors in developing societies view their affairs. The book is organized by country, appropriately treating together the two Vietnams and also Malaysia-Singapore. The editor for each country begins with a brief and generally balanced historical account and provides continuity by thoughtful and succinct introductions to each document. The collection is useful for country-specific study, not for comparative analysis, because of the lack of common themes and common organization. One regrets that Dr. Smith did not pose some of the common problems – governmental effectiveness, military domination, ethnic pluralism, foreign intervention – so that the different national approaches could be compared. The index alone cannot perform comparisons because the topics are touched upon in passing and the index is insufficiently conceptual. The lack of a bibliography is critical, because it is needed to direct the reader to other key documents and commentaries by Southeast Asians.

As is usual in a collective effort, there is great disparity in the quality of the selections, reflecting the intensity and direction of the individual contributor's current research as well as the general availability of documents. Seven of the nine contributors are the same regional experts who wrote the country chapters of Kahin's 1959 classic, *Governments and Politics of Southeast Asia.* By any criterion the section on Indonesia is outstanding. Herbert Feith and Alan Smith drew the first half of the documents from Feith's and Castle's superb collection, *Indonesian Political Thinking* (1970). The selections since Gestapu convey the richness of the Indonesian political fabric – poems of protest, General Suharto's interview with the student press, the courtroom confession of the top Communist leader captured, voices from exile. Since Indonesian political discourse has never taken place in English, the translations for this collection are particularly valuable. The other contributors have generally limited themselves to official translations of official pronouncements and to the tiny English language press.

In the Philippine section David Wurfel also broadly interprets his task, bringing together a variety of views under the themes of ideology, elections, policies, and martial law. Similarly, Norman Palmer conveyed the complexity of the ethnic issues in Malaysia by presenting a diversity of views, although one wishes for voices from the East Coast and East Malaysia. Unfortunately, the editor and the other contributors construe their task more narrowly, presenting only official documents. The sections on Burma and Vietnam both suffer from concentration on documents more than ten years old and reliance upon official documents previously published in English. Both sections contain a welcome surprise, a previously unpublished document in the private possession of the contributor – for Burma a Shan criticism of the quasi-federal constitution, and for Vietnam a province chief's 1958 recognition of the weaknesses of the Diem regime. The Burma section begins appropriately with excerpts from Silverstein's recent collection of Aung San's speeches, but proceeds through the four alternating civilian and military regimes presenting only the government viewpoint. An opposition voice is badly needed; General Ne Win's long speech rebutting the "thirty-three wise men" should have been complemented by at least snatches of the "wise men's" conclusions which provoked the general.

This collection makes a lively and substantial contribution in presenting Asian perspectives on their political affairs. This work should be read by graduate students and Americans working in Southeast Asia to increase their understanding and empathy.

DOROTHY H. GUYOT

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Politics of the Indian Ocean. By K. Rajendra Singh. (Delhi: Thompson price, Ltd., 1974. Pp. 252. Foreign Price, $14.00.)

This book purports to explain "big-power rivalry" in the Indian Ocean in terms of strategic deterrent weapons systems. Regrettably for the author of this detailed but largely irrelevant analysis, the presence or absence of strategic weapons in this area is not really important for the U.S. and the U.S.S.R., let alone for such extraregional powers as Great Britain, China, and Japan. Certainly, public evidence indicates that there have been SSBN cruises in the Indian Ocean, but there are no data – certainly none presented in this book – to demonstrate that either the U.S. or the U.S.S.R. regards the area as having strategic nuclear importance. Nevertheless, starting from this defective premise, the author proceeds to discuss littoral reactions to an imaginary strategic nuclear presence. One suspects that his main interest is to legitimize an Indian nuclear program, begun at about the time this book was written (1971–74).

There are numerous errors of fact and interpretation here, beginning with the view that the U.S. was trying to "compete" with Great Britain for naval superiority in the mid-'fifties (p. 23), on through the misguided judgments that Japan has military designs on the Indian Ocean (p. 119) and that China will soon present a naval threat to India (p. 115). Dr. Singh has apparently settled for a Cassandra-like role, seeing military threats and challenges to the Indian Ocean area from nearly every direction. But reality is difficult enough for the Indian Ocean policy makers without inventing new problems.

While there is a substantial amount of information on the political history of the Indian Ocean (drawn largely from readily available Western sources), the chief interest here is tone and style. A note of anti-Americanism permeates this book. There is also a vague and rather naive belief in the integrity of the Indian Ocean "community": that by pulling together, the littoral states can keep outside powers away. This view is commonly found among Indian analysts (although not in many other littoral states), who have long argued that the region's difficulties stem from outside intervention and involvement. But it ignores the very real damage which littoral states have inflicted upon each other – most recently the victimization of South Asia by the oil-producing states of the Persian Gulf – and the fact that American and Western interests in South Asia largely derive from interests and objectives stemming elsewhere.

As Dr. Singh does note in his final chapter, the politics of the Indian Ocean area have taken a new turn following the 1973 oil crisis. We can hope that in his next work he will elaborate on this theme and perhaps include his ideas on the nuclear balance in South Asia in light of the recent Indian nuclear explosion.

STEPHEN P. COHEN

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A New Bill of Rights for Britain. By Frank Stacey. (North Pomfret, Vt.: David and Charles, 1973. Pp. 168. $10.50.)

Picture a contemporary Western country whose government practices in one of its regions a policy of internment without trial of suspected terrorists, combined with recently proved and officially acknowledged "systematic physical ill-treatment" in the course of interrogations. Picture the same country where a random sample study by a legal scholar of 134 convicted defendants found that of 57 who asked to speak to a lawyer when arrested, no fewer than 42 were denied this right by the police. Picture finally this country where a reputable organization of one of the largest racial minorities alleges "systematic brutalization" against their people by the police in the nation's capital.

Franco's Spain? Greece under the colonels? Chile after Allende's overthrow? No, the country is the mother of liberal democracy and still widely regarded as its foremost practitioner: the United Kingdom. And the above facts are but a few presented by Frank Stacey in making the case for a British Bill of Rights. Stacy sets out cogently and succinctly the deficiencies of the British political system in making citizens aware of their rights and in providing machinery to secure these rights not only in the broad sense of personal security, privacy, freedom of expression, assembly and association, and freedom from discrimination, but also in terms of citizen rights vis-a-vis social services and administrative tribunals. Although the discussion in each of these areas is "by intention, illustrative rather than exhaustive" (p. 149), the presentation is cautious and balanced, and certainly upholds the contention in the preface that British "standards of fairness and humanity . . . are not always as high as we suppose they are."

What the book does not do, however, is convince the reader that a Bill of Rights would remedy the situation. The main reason for this lies in a lack of hard analysis either of the causes of the problem or of the success those polities with a Bill of Rights have had in avoiding the problem. In his opening chapter, Stacey suggests that big government, big business, concentrated newspaper ownership, and the continuing unequal access to the law between rich and poor are leading various politicians and jurists to challenge the classical view of the British Constitution – the assumption that the common law as interpreted by the Courts and guarded by Parliament adequately guarantees individual liberties. But Stacey nowhere examines what a Bill of Rights would accomplish in stemming those larger social forces which he all too briefly addresses as the real cause of the problem. Stacey notes the activist role of the American Supreme Court in the postwar era in contrast with its conservatizing role in the earlier period. But this surely only begs the question since it suggests that it is not a Bill of Rights or judicial review *per se* that are important but rather the social forces at work beneath them. Again, in his second chapter, Stacey cites examples of Commonwealth countries which have adopted a Bill of Rights. But beyond thereby showing that it is possible to tack a Bill of Rights onto the

Parliamentary system, he does not examine how meaningful such an exercise is. To take one example which Stacey relies on heavily, the Canadian Bill of Rights adopted in 1960 is generally regarded here as having contributed little to the protection of civil liberties; it certainly did nothing to prevent the Canadian government suspending all liberal freedoms during the FLQ crisis of October, 1970 by invoking the War Measures Act in peacetime, thereby using, as one Canadian politician put it, "a sledgehammer to crack a peanut."

Despite these failings, Stacey's book does make a contribution, especially when the author makes concrete suggestions for new mechanisms to protect individual privacy and access to benefits in the social services. (Unfortunately, he is often too timid in these proposals, as when he cursorily dismisses the idea of a universal and free legal aid system along the lines of the National Health Service.) Moreover, implicitly at least, Stacey recognizes that the greatest protection for a free society lies in a politically active population aware of the worth of basic freedoms and prepared to defend them. His discussion of the 1972 Industrial Relations Act, which he correctly perceives as having posed "a threat to the continuance of free trade unionism" (p. 120), shows that it was the steadfast defense of its freedom of association by the organized working class in Britain which laid the basis for the Act's eventual repeal. A contribution to the question of how this defensive action might be transformed into a positive movement to overcome those increasingly dangerous social forces which Stacy sees as undermining basic freedoms, would, however, be more to the point than a new Bill of Rights for Britain.

LEO PANITCH

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Elites and Power in British Society. Edited by Philip Stanworth and Anthony Giddens. (New York and London: Cambridge University Press, 1974. Pp. xiii, 261. $15.50.)

This book is the product of a conference at Cambridge in 1973, and consists principally of a series of empirical studies of elites in British society. It begins with a theoretical discussion by Anthony Giddens outlining a possible conceptual basis for the study of elites in the British class structure, and concludes with a rousing plea by John Rex for studies in the future which concentrate not simply on elite recruitment, structure or decision making but on the general framework of state and economy within which elite groups are structured and recruited. As with most collections of this kind, the result is a mixed bag, yet the overall quality of individual contributions and general editing is high. The book certainly provides political sociologists with valuable empirical evidence and some food for thought.

Empirical and conceptual study of elite groups in British society has only recently received extensive academic attention, and it remains largely the province of the sociologist. The reasons for this need not concern us here, though they are not explained simply by the preoccupation of students of British politics with understanding the role of class in British society and politics. Indeed, as is argued in the preface to this book, the vocabulary of class theory is not necessarily incompatible with that of elite theory. If one can no longer talk of a capitalist ruling class in Britain, this does not preclude the existence of a governing class, or even a ruling status group. Indeed, John Rex suggests (in modified Marxist terms) that Britain has an elite with a corporate identity, which shares power and authority built on cultural legitimacy. While this book demonstrates that important empirical evidence can be collected on recruitment to elite positions, and on the structure and cohesion of elite groups, it underlines the difficulty in moving from such evidence to an evaluation of how much control over government allocations is exercised by the elite groups analyzed. Yet this latter question is of central importance to political sociologists in Britain. Considerable progress has been made toward understanding the significance of the transmission of political and cultural values through elite educational institutions, and the possible effects of this on political decisions, but much more needs to be done. Any serious attempt to determine who gets what, and how, within the British political system involves more than speculation on the implications of the rise to power of Mrs. Thatcher to the leadership of the Conservative party, or the effect of a capital transfer tax on Britain's wealthy elite.

This book provides illuminating information about the nature of elites in British society, but it falls short of a stated goal of furthering our understanding of the actual use of power, and conflicts over power, among the members of elite groups. The empirical contributions include two useful pieces synthesizing and updating earlier important political studies – William Guttsman on the British political elite, and R. K. Kelsall on recruitment to the higher civil service. Several contributions discuss different aspects of new research concerned with wealth and economic power, tracing interconnections between the City and industry, the transmission of wealth between generations, and the social and educational characteristics of wealthy property owners and economic leaders. R. E. Pahl and J. I. Winkler, in perhaps the most valuable conceptual contribution in this group, warn also of the dangers of presuming that office or position is synonymous with control or influence, at least within the economic elite.

Other contributions deal rather sketchily with the higher education of those holding elite positions, especially University Vice-Chancellors, and with Church of England bishops as an elite. The overall impression is that, while it is still useful to have a certain type of education and family background in order to become a member of the academic, economic, and even the political and administrative elites in British society, this does not mean that thereby one will acquire a position of influence over political decisions. The evidence may sustain Rex's view of the continued existence of a ruling status group, but there are indications that such a group may be exercising a less decisive voice over political decisions.

In this context, the contribution by Christopher Hewitt – a case study of the distribution of political power in the United Kingdom during the 1944–64 period – seems unsatisfactory. While concerned with a major problem, the extent to which elites may exercise disproportionate influence on governmental allocations, Hewitt too consciously accepts a pluralist interpretation of policy making backed by a limited definition of political decision makers and the political elite, and concludes that the policy process is not explicable in terms of the nature of the political elite. However, if political events in Britain since 1964 have led to a diminution in the role of class as an indicator of political support, and in the influence of certain class-related attitudes and groups over the "institutional mediation of power," there seems to be a strong case, as R. E. Pahl and J. I. Winkler advocate, for more direct study of the ethos and action of elite groups and their effect on political decisions. Regrettably, a contribution by Bruce Headey on cabinet ministers, now part of a book, was not included. This would have provided more specific evidence of the degree to which elite recruitment patterns and formal authority may be related to political power. It would also have given an insight into the relative influence of legitimating, selecting, and initiating factors on policy formulation and implementation in British politics.

J. D. LEES

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China and Pakistan: Diplomacy of an Entente Cordiale. By Anwar Hussain Syed. (Amherst: The University of Massachusetts Press, 1974. Pp. 259. $12.50.)

The central theme of this concise and highly valuable study is the interaction of a small state with other components of the international political system, in particular the Great Powers, in its quest to seek protection against the possible aggression of a hostile neighbor. The book is broader in scope than its title suggests, as it not only discusses the development of friendly relations between Pakistan and China but also examines the involvement of the United States and Soviet Union in Indo-Pakistan affairs, the impact of that involvement on Sino-Pakistan relations, and the impact of Pakistan's internal political dynamics on its foreign policy. The sources of data employed in this study include Chinese press and radio reports, the Pakistani newspapers, the National Assembly of Pakistan debates, the author's discussions with key Pakistani government officials and diplomats, and his lengthy interview with Prime Minister Bhutto.

The author contends that Sino-Pakistan relations have passed through many stages. Following Bandung and Suhrawardy's visit to China, goodwill and cooperation between the two countries increased. The Chinese understood the Pakistani viewpoint and need to participate in SEATO and align itself with the United States. Until 1961, however, the relationship between the two countries was, for the most part, restricted to cultural exchanges and trade. The Sino-Indian border clash, the subsequent decision of the United States to arm India, and continued Soviet military and diplomatic assistance to India convinced Pakistani policymakers of the desirability in broadening the scope of its relations with China. The border and air agreements between the two countries during the early 1960s were steps in this direction.

To acquaint the reader more fully with the "context" of Pakistan's China policy, the author has examined Pakistan's relations during the past two decades with India, the United States, and the Soviet Union. Indo-Pakistan relations, he believes, have been marked by the mutual perception of deep-rooted hostilities. The American-Pakistani alliance, he argues, collapsed because both sides had different objectives in becoming a member of the alliance and had, furthermore, different expectations from each other. The U.S. wanted the support of Pakistan for the containment of China. Pakistan's main concern, however, was its hostility with India. Although the U.S. significantly contributed to the economic development of Pakistan, it was at no stage prepared to alienate India. Pakistan's relations with the Soviet Union, the author believes, "have been difficult, from time to time, tense, and, at their best, cooperative in a selective businesslike fashion" (p. 50).

According to Professor Syed, the suspension of American military assistance to Pakistan during the Indo-Pakistan War of 1965, and the reluctance of the Soviet Union to supply weapons to Pakistan on a large scale for fear of alienating India, forced Pakistan to look to the Chinese for arms and eventually economic assistance. Chinese support for Pakistan during the 1965 war was appreciated by the people of Pakistan, who were hopeful that in the event of an Indian attack on Pakistan, the Chinese would intervene in their behalf. During the 1971 war between India and Pakistan, however, the Chinese were faced with a dilemma. I agree with Professor Syed's view that the Chinese were not prepared and perhaps lacked the military capability to break the Indo-Soviet alliance, and that Pakistan itself was in a very vulnerable position as a large portion of its population had revolted and challenged the legitimacy of the federal government.

The author has also examined the interrelationships among Pakistan, India, China, the United States, and the Soviet Union from a futuristic perspective. He believes that "while India insists on being the dominant power in Southern Asia the prospect of Indo-Pakistan amity will remain bleak" (p. 160). Although the United States does not benefit from India's rise to a dominant position in South Asia, the dimension of American interest in the solidarity of Pakistan will nevertheless, be, "modest." The U.S. will continue, however, to take an interest in the stability and economic development of Pakistan. The Soviet Union will continue to put pressure on Pakistan to weaken its ties with the United States and China. The author believes that in the event of an Indo-Pakistan war, the direct intervention of China on behalf of Pakistan cannot be assumed. Pakistan, therefore, "can not invite Soviet hostility except at her peril" (p. 173). However, relations between China and Pakistan have matured to the extent that they can "sustain" themselves even without the hostility of a common enemy.

Students of South Asian politics and foreign behavior of small states will welcome this book because it focuses on many pertinent issues. Though the analysis presented in this volume is, for the most part, historical and descriptive, the author should be commended for presenting a highly vivid, readable, and objective assessment of the development of close ties between China and Pakistan. The material presented is reliable and its organization outstanding.

G. SHABBIR CHEEMA

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Palace and Politics in Prewar Japan. By David Anson Titus. (New York: Columbia University Press, 1974. Pp. 360. $20.00.)

The mid-1970s is an exciting period for specialists on Japanese politics: after many years in which scholars advanced relatively few new concepts to explain the Japanese political system and concentrated instead on exploring the ones that already existed – consensus in decision making, modernization, democratization, government-business cooperation, seemingly unique labor relations, and others – today there is growing interest in reconceptualizing how the Japanese govern themselves. This new emphasis includes analyses of conflict in Japanese society (and consensus as a mode of conflict resolution rather than as a cultural trait), bureaucracy, international linkages, Japanese competitiveness, and many other hitherto neglected subjects. This trend holds out the promise that whereas Western generalizations about industrialized open societies used to be "applied" to Japan, the Japanese case will now contribute to improving general theory. David Titus's important book reflects this new desire to theorize, while it also supplies previously unavailable pretheoretical information on a key institution of the Japanese system.

Despite the imperial institution's obvious centrality to prewar politics (and its continuing importance as an unchanging pole of Japanese national identity), it has never been studied. Until the occupation changed its status and created conditions which allowed scholars such as Titus access to imperial records, no one could even discuss it. Titus now offers us a wealth of new information and also introduces several novel and challenging concepts to explain how the overall system worked.

Areas of new information include the following: First, Titus clarifies the diverse imperial prerogatives contained in the Meiji constitution of 1889 to show that the Emperor was not personally sovereign but rather a "transcendental prisoner" (p. 14) and that the imperial will that ratified every act of the prewar Japanese government was not the will of a person but of an institution which held in trust the supposed interests of a lineage. Titus also documents the disasters, confused responsibilities, and incoherent priorities that resulted from this brand of so-called absolutism.

Second, Titus offers a new periodization of the separation, unification, and reseparation of power and authority that Japan underwent in its century of modernization. The shogunal period was characterized by the Emperor's ritualistic ratification of *faits accomplis*. This was followed by a period of oligarchic control (1885–1912), a period of transition to leadership by career bureaucrats representing the institutions of imperial prerogative (army, navy, Diet, ministries) outside the palace (1912–21), a period of bureaucratic leadership at the emperor's side by the constitutional monarchists (1921–36), the replacement of the constitutional monarchists by the traditionalists (1936–40), and a period of bureaucratic leadership by the traditionalists (1940–45).

Third, and most important, Titus places all Japan specialists in his debt with a detailed and insightful analysis of four officials: the Imperial Household Minister, the Grand Chamberlain, the Chief Aide-de-camp, and the Lord Keeper of the Privy Seal. Particularly the last named official appears in every history of the international politics of the 1930s and 1940s, but few authors and even fewer readers ever understood, until Titus's book appeared, whether he was a mere figurehead or one of the key decision makers. Through a detailed analysis of the career of Kido Koichi, chief secretary to the Privy Seal (1930–33), director of Peerage Affairs in the Imperial Household Ministry (1933–37), and the last Lord Keeper of the Privy Seal (1940–45), Titus exposes how the Privy Seal performed his most important function: political broker to the throne on cabinet formations.

On the theoretical side, Titus will be applauded for his treatment of Japan's "privatized decision-making process" (p. 155), for his elucidation of what he forthrightly labels "bureaucratic fascism" (p. 192), and for his hypothesis that "the imperial government was the greatest single instrument for the privatization of conflict in the prewar Japanese community. Govenment in prewar Japan was a great structure for contracting the scale of open conflict" (p. 312). Less happy is his misplaced criticism of Maruyama Masao's threefold typology of political personalities: the powerless but authority-laden "shrine," the illegitimate but powerful official, and the violence-prone outlaw. Refusing to understand that Maruyama's original statement was more an aphorism than a scientific assertion, our author tries – interminably – to operationalize it. The results are about as persuasive as would be an attempt to operationalize "a stitch in time saves nine," and only produces a caricature of quantitative social science methodology. This is, however, only a small part of an important book, and even it points to the importance of the official bureaucrat in the Japanese political system, as Maruyama originally intimated.

For readers more interested in the historical than the systematic role of the emperor, Titus is convincing on one of the major controversies of the postwar world. From the leftist critics of the occupation to David Bergamini's direct attack on the emperor (*Japan's Imperial Conspiracy*, New York, 1971) – which is not mentioned by Titus – critics of Allied policy have contended that the Japanese Emperor should have been tried as a war criminal. The foreigners most knowledgeable about the militarist and wartime periods supported the decision of MacArthur to spare the Emperor, but there has always been a grain of suspicion that they may have been too close, or hoped to be closer, to Japanese ruling circles. Titus's documented conclusion is therefore of considerable interest: the "palace acted as a break on extremism throughout its prewar existence" (p. 333).

CHALMERS JOHNSON

University of California, Berkeley

The Asbestos Strike. Edited by Pierre Elliott Trudeau. Translated by James Boake. (Toronto, Canada: James Lewis & Samuel, 1974. Pp. 382. $15.00, cloth; $5.95, paper.)

The Asbestos Strike is more than the anatomy of a particular industrial conflict. It provides an understanding of the fusion between economic and political forces, and it affords some insight into the development of political leadership. All this and more is done in what technically is a book of essays written nearly twenty years ago and only this year translated from French into English.

The book was edited in 1956 by Pierre Elliott Trudeau. He was then described as a lawyer, economist, and co-editor of the Quebec review, *Cité Libre*. He is now, of course, Canada's Prime Minister. As editor, Trudeau arranged the essays into a mosaic that gives perspective to the French Canadian character, for the book deals pre-eminently with the quest for Quebec identity in an industrial age. The Asbestos Strike was a climactic event in the accommodation of French Canada to industrialization.

It was on February 13, 1949 that 5,000 asbestos miners at Asbestos, Quebec struck and remained on the picket line for 120 days. They struck in defiance of Quebec law, which required conciliation and arbitration. What was more dramatic, they struck through Quebec's Catholic trade union, the Canadian and Catholic Confederations of Labour (C.C.C.L.). The workers, underpaid relative to the rest of Canada and victims of the industrial disease of asbestosis, refused to yield to the twin pressures of government and business. In the process they caused their union, the C.C.C.L. – originally formed by the Church to ward off international "radical" labor organizations – to become somewhat radicalized itself. The struggle of the C.C.C.L., initially unnoticed, was long and violence-ridden.

According to Trudeau that struggle discredited the myth that the Quebec character would be saved by turning away from industrialization. For years, wrote Trudeau, the Church propagated a "social doctrine" that required return to the land, small business, the cooperative movement, the Catholic trade union movement, and corporatism. Workers were to accept their lot; their reward was to be in heaven; radical "foreign" unions, such as the Knights of Labor, were to be turned aside, an elite of the Church, government, and business would lead the people. In the Asbestos Strike the people asserted themselves; they wrenched themselves and their institutions into a world that included rather than excluded technological advance.

The strike marked a watershed. Within Quebec it tended to galvanize the forces of organized labor. It forced moderation by "big" business. Or put another way, "big" business began the process of integrating French Canadians into their Quebec enterprises. And, most importantly, the Quebec government began to retreat from the jingoism of "Buy Quebec Only," from forms without substance and corresponding opportunism. Government began to concern itself with the practical well-being of its citizenry.

Those who wrote *The Asbestos Strike*, in their own way, were decision makers in 1956. They then represented a powerful intellectual elite consisting of journalists, economists, lawyers, and academicians. Three of the nine contributors, however, were affiliated with trade unions. It can fairly be said that the strike helped to shake awake Quebec's intelligentsia and cause it to address the problems of reality. Not the least among these intellectuals was Pierre Elliott Trudeau, who was an on-the-spot observer of the strike and was for a short time "detained" by a hostile Provincial police force in Asbestos, Quebec.

When the strike ended, it seemed for some, including Trudeau, that there was a danger of slippage, of a return to mythology and servitude. Against such a danger Trudeau responded in words that perhaps help us to understand better his motivation and those of his Liberal Party:

> After fifty years of servile and stupefied silence, have the mass of industrial workers but recently acquired the right to speak, only to hold their tongues with greater eloquence? At the very moment when Quebec society feels that it may catch up after generations of backwardness, are the gains of recent progress to be converted into a sterile conservatism, or into the traditional *je me souviens?* It is a matter of urgent necessity that we reject these counsels of quietude. We no longer live in a static world: we must go forward with the caravan of humanity or perish in the desert of time past (p. 345).

This book has obvious interest for Canadians, for it defines some of those concerns that motivate their present Prime Minister. Beyond that, the book should have wider readership as a study of a revolution that may not yet be complete.

DANIEL JAY BAUM

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Military Role and Rule: Perspectives on Civil-Military Relations. By Claude E. Welch, Jr. and Arthur K. Smith. (North Scituate, Mass.: Duxbury Press, 1974. Pp. 294. $4.95, paper.)

This is an excellent new book in the field of civil-military relations. Professors Welch and Smith have combined thorough research, sound methodology, and an interesting style to investigate the growing phenomena of military intervention to gain political control of national governments. Through five case studies of military intervention into politics (Thailand, Nigeria, Peru, Egypt, and France) the authors investigate the applicability of twenty propositions which they either drew from the literature or proposed themselves. The authors are concerned with describing and analyzing internal and environmental factors affecting military intervention within the five countries studied – both theoretically and behaviorally. The book has three major sections: (1) a theoretical framework composed of the twenty propositions and a typology of civic and praetorian systems; (2) the case studies (all but France being "praetorian societies," p. 7), and (3) the testing of (1) with (2) and presentation of some appropriately temporized findings about the factors conducive to military intervention and the impact of military rule on the national modernization process.

This is a scholarly nonpolemic evaluation which avoids definitive conclusions, simplistic prescriptions, or sweeping value judgments. The message, in sum is that: (a) military power tends to fill civilian political leadership vacuums; (b) the garrison-managerial state is ascending in importance even though possibly a transitory phenomenon (p. 262); (c) the "political roles of the armed forces vary in accordance with factors peculiar to each state" (p. 3); (d) "outright military intervention in politics arises primarily from the limited or declining legitimacy of civilian political institutions. . ." (p. 4); and (e) "strong civilian control over the armed forces emerges gradually through complex historical processes, reaching its fullest expression in the military's voluntary acceptance of subordination. . ." (p. 4).

The importance of the book, however, lies not so much in the findings, which present no surprises, as in the research and organization of information which aims to answer the question: "How do these selected propositions hold for the countries studied at this time?" From this point of view the book represents a basic civil-military status report rather than a set of opinions subject to later serious challenge. It is worth mentioning what the book does not do. I found the first ten pages (p. ix to p. 8) to be somewhat misleading. This portion poses a wide range of questions that are not analyzed in the text (sometimes not even addressed) and indicates a broader scope of investigation and applicability of lessons learned (e.g., p. x) than the text supports. I had the feeling that the authors may have neglected to re-edit these first ten pages once the work had been completed and the complexity of the subject had been fully exposed. For instance, the book does not address the totality of civil-military relations, as occasionally implied, but only that portion dealing with policy control of some national governments. The reader should skim these first pages with the knowledge that the remainder of the book is a solid and coherent research and writing effort.

This Welch and Smith book is a welcome indication of the reversal of the trend of declining academic research emphasis in military matters characteristic of the Vietnam War years. I recommend it for undergraduate or graduate courses (including ROTC) in government, politics, civil-military relations or, for that matter, for any reader interested in the role of the military in nation-building and in control of, or influence over, national policy.

COLONEL ROBERT M. KRONE

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Governing an African City: A Study of Nairobi. By Herbert H. Werlin. (New York: Africana Publishing Co., 1974. Pp. ix, 308. $22.50.)

In colonial times, the day-to-day government of Nairobi was largely carried out by its officers. Trained in the tradition of British local government, they formulated policy for their council, meeting weekly together to work out the advice they would present. The officers and most of the councillors were British, regarding Indians with suspicion and Africans with paternal superiority. Indians were represented on the council, but Africans scarcely took part in local government, their affairs being considered the responsibility of the District Commissioner for Nairobi. Within its narrow limits, city government ran smoothly enough. There were scandals over shoddy municipal housing for African workers, but with its broad avenues festooned with bougainvillia, its parks and garden suburbs, its hotels and shopping arcades, it was a comfortable city for its European masters.

All this changed with the independence of Kenya. British officials now confronted an elected African council, inexperienced and poorly educated, but led by a mayor determined to assert the prestige of his office. Within a few years, these officials gave way to African replacements. At the same time, the powers of city government were enlarged. Formally, the structure still conformed to British conceptions of local government, but behind this facade the government of Nairobi was disintegrating under the tensions of transition. The heart of Herbert Werlin's study is his account of this transition.

He describes how relationships within city government broke down in mutual distrust. The African councillors were, naturally enough, unsure of themselves, and equally naturally, unsure how far to trust the advice of British officials. They would make impossible demands, where they would have been wiser to accept the officials' practical judgment, yet fail to make policy when officials were looking for a lead. The replacement of British by African officers did not generally resolve the mistrust: for the African newcomers were insecure and lacked the authority of experience. The same pervasive mistrust extended to relationships with the Ministry of Local Government, where a similar pattern of capricious and arbitrary intervention repeated itself. The outcome was muddle and growing incompetence. The city employed more and more people, yet the work got farther and farther behind, while political favoritism became more and more extensive.

Werlin interprets this transition in terms of "elasticity of control." He argues that neither centralization under national government, nor decentralization to free the city from the interference of the Ministry, would resolve the problems. Rather, control should be elastic, dividing responsibility at every level as people and institutions proved their competence. But this seems only to restate the truism that good administrators know how to delegate. Werlin's study is a useful account of the problems of political change, but its analysis seems preoccupied with the symptoms of managerial breakdown, rather than its underlying causes. It ignores the political setting within which a new basis of trust had somehow to be established. His account does not bring out the crucial problem of creating a politically effective and responsive government in a city where most people considered their real home to be elsewhere.

Charles Rubia, Nairobi's first African mayor, understood that at independence, the government of Kenya's capital city was a potentially powerful political base. It controlled a large revenue, and represented the nation to the tourists, industrial and commercial investors from overseas on whom Kenya depended for most of its development capital. At the same time, it controlled access to housing and a large number of jobs in the city for its African constituents. But if this potential was to be realized, the mayor would have to assert the authority of his office, and especially his standing with Kenyatta, whose endorsement was ultimately necessary for any effective exercise of power. Hence Rubia's insistence on the dignity of his office – his quarrels over precedence, his abortive purchase of a Rolls Royce – represented an attempt to transform the government of Nairobi from its British roots to a system, rather, of political patronage under a dominant mayor, whose standing would equal that of the ministers of national government. Such a system, though it would have shocked colonial officials, might have used patronage to integrate a reliable and effective pattern of relationships between officers and councillors, city and national government, politicians and their constituents.

But as Werlin shows in his discussion of housing and petty trading, the City Councillors did not develop a coherent patron-client relationship with their constituents. Very few people in Nairobi regard the city as their home. To the Africans, it is a center of opportunity, where they hope to earn enough to buy or develop a farm in their homeland. They identify so little with the city that few bother to vote in council elections, in some wards as few as one per cent. The European population is transient, consisting of tourists, or academicians, managers, and professionals on short-term contract. The Asian population were truly natives of the city, but as time went by, it became clear that there would be no place for most of them in an African nation. The only identifiable African community with roots in the city was its oldest African settlement, Pumwani; and Pumwani was regarded by the new African leadership as an affront to civic pride – a slum to be cleared as soon as possible.

Hence there was no powerful political constituency, for whom the development of the city itself was a crucial concern. In these circumstances, patronage did not evolve into a politically responsive strategy towards the needs of an urban African population. City government could be milked for jobs, contracts, licenses, housing allocations: but the benefits went largely to a political elite, who inherited much of the indifference and contempt toward the struggling African workers of their European predecessors. These factors surely underlie the problems of government that Werlin describes, and his analysis seems a little superficial for lack of a fuller treatment of them.

PETER MARRIS

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The Moral State: A Study of the Political Socialization of Chinese and American Children. By Richard W. Wilson. (New York: The Free Press, 1974. Pp. 290. $12.95.)

The Moral State is Richard Wilson's second book on the political socialization of Chinese elementary schoolchildren. The first, *Learning to Be Chinese* (Cambridge, Mass.: M.I.T. Press, 1970) provided a comprehensive account of what and how children learn about their political system in Taiwan, the political symbols they respond to, and their attitudes toward authority.

In *The Moral State,* Wilson has developed further some of the themes introduced in his prior work, in particular the impact of shaming punishments on the moral development of children and on their attitudes toward groups and authority. The concepts of guilt, shame, and moral development are discussed in some detail, with special reference to the work of Piaget and Kohlberg. The bulk of the book, however, is devoted to a presentation and discussion of the results of interviews and a questionnaire administered by Professor Wilson to 3rd, 5th, and 7th grade children in urban Taiwan, Hong Kong, New York City's Chinatown, and Princeton and New Brunswick, New Jersey. These questions were designed to test the degree of universalism/particularism and autocentricity/heterocentricity of the children's responses to situations involving moral considerations. (Wilson defines these terms as follows: "Autocentric: Where particular others are *not* cited as the authority for behavior: orientation to rules or principles. Heterocentric: Where particular others are cited as the authority of behavior: orientation to others" [p. 127].)

Given the strength of the family system in Chinese society and the particularistic nature of the traditional Chinese social and political system, we might expect intuitively that Western schoolchildren would prove more autocentric in their responses. But this is *not* the case. The Taiwan schoolchildren were the most autocentric and the American schoolchildren the most heterocentric, with the Hong Kong and Chinatown children scoring in between. Wilson argues convincingly that these results reflect the nature of the learning environments to which these children have been exposed:

> We believe that these prominent differences in the various learning areas are related to the explicit use of shaming in school education in Taiwan and Hong Kong, to morals training in Chinese schools, to the relative importance of autonomous peer groups in American society and especially in American schools, and to the inability of children in Chinese societies (particularly Hong Kong and Chinatown) to maintain a firm sense of identity and commitment to secondary level membership (p. 242).

The group orientations of Chinese and American schoolchildren, Wilson shows, differ sharply. The Chinese children consider teachers and other authority figures to be key members *of* the group, exercising primary influence over group norms. To defy the leader is to defy the group as well, suffering all the disapproval and sanctions this implies. In contrast, the American schoolchildren see adult authority figures as standing outside the group itself. The group then "deals as an entity with (outside) leadership. In such a case the group, through the force of its own internal codes and sanctions, buffers the sanctions and codes of the leadership" (p. 246). Thus the autocentric norms emphasized by the national leadership can be more readily transmitted in the Chinese setting. Instilling in the young a sense of loyalty to groups larger than the family or village has been an important educational goal in Taiwan as well as in the People's Republic of China and other modernizing societies. By the time they reached the seventh grade, however, the Chinese schoolchildren questioned by Wilson seemed already to be turning away from the autocentric values they had learned earlier, perhaps in reaction to their perception of a gap between these values and the actual behavior of adult authority figures who are supposed to embody them. Other research suggests that cynicism may begin to develop at this point.

Wilson has been careful to place his study in an appropriate theoretical framework. His questionnaire and research design are ingenious, and his interpretations insightful. The presentation of the data he has collected leaves something to be desired, however. Rather than showing the children's precise responses to his questions, he has given only the percentage selecting *pairs* of responses he has designated as "autocentric," "heterocentric," etc. A reader who might wish to classify the responses in different ways is thus prevented from doing so. A master table in the appendix might also have made it unnecessary to present two graphs in the text for each question, one for the "autocentric" and one for the "heterocentric" case. Only nonresponses keep these pairs of graphs from being mirror images of each other. The times at which the surveys were administered are not noted, nor are the responses of the black schoolchildren who comprised about a quarter of the U.S. sample given or discussed. In addition, Wilson sometimes immerses himself too much in the jargon of educational psychology, making the reader work to distill his meaning.

There is considerable evidence to suggest that the political culture in Taiwan and Hong Kong, studied by Wilson and others, is similar in many respects to that encountered by the Chinese Communist Party when it took control of China in 1949. A number of the mass campaigns since then, including the Great

Proletarian Cultural Revolution and the struggle against the ideas of Confucius and Lin Piao, have been viewed by some scholars as a determined effort to change critical aspects of this political culture. *The Moral State* provides important insights into the nature of these political attitudes, and the responses of those who hold them to government-directed efforts to alter them. Despite the minor shortcomings in presentation noted, the book is also a welcome contribution to the field of political socialization, and should be especially useful to those whose acquaintance with that field has been limited largely to Western societies.

SHELDON APPLETON

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Theory, Law and Policy of Contemporary Japanese Treaties. By Gerold Adams. (New York: Oceana Publications, Inc., 1974. Pp. xvi, 270. $15.00.)

The purpose of this book is according to the author, first "to examine thoroughly Japanese treaty-making and practice" and second, "to determine the role of Japanese treaties in Japanese foreign policy (p. 2). Although Japan has been involved with treaties, since she joined the international society through the 1854 Treaty of Kanagawa, the time span of this book is limited to 1952–1970. The sources used are the texts of treaties, related official documents, and Japanese and non-Japanese writings. Twenty-nine tables and three charts to illustrate the article are useful, together with "the calendar of Japanese treaties 1951–1970." While Adams's book seems to be influenced by Jan F. Triska and Robert M. Slusser, *The Theory, Law and Policy of Soviet Treaties,* 1962, not only in its title but also in the construction of its contents, his methodology of comparing Japanese treaty practice with world treaty trends by using computed data is novel (pp. 10–11).

In general, Adams's book provides a pertinent outline of the Japanese attitude toward the law of treaties from both aspects of theory and law. According to the author's finding, however, Japan is not very different from Western countries in "treaties and the constitutional system" (Chapter II) or in "the conclusion and termination of treaties" (Chapter III). The only exception is that even the exchange of notes may be subject to ratification in Japan (pp. 197–198).

In discussing the role of Japanese treaties in Japanese foreign policy, Professor Adams, classifying treaties into "political treaties" (Chapter V), "economic treaties" (Chapter VI) and "assistance, functional and social agreements" (Chapter VII), analyzes the contents skillfully. More specifically, by making full use of computed data on 568 bilateral treaties and 146 multilateral treaties Japan concluded or entered into between 1952 and 1970, he throws light on the treaty trends of Japan based on the content, taking into account the time and partner factors as well. Thus, the book vividly depicts the process by which Japan gained a footing in international society, (a) establishing the legal foundation for future relations with former enemy and occupied states by "political treaties" during the first period (1952–1960), (b) expanding "economic and social agreements" and increasing treaty relations with the less developed states and Soviet Bloc states during the second period (1961–1970), and (c) joining "multilateral treaties" (Chapter IX), in particular, economic and functional throughout the above first to second period. While the author further discusses the Japanese treaty trends from two interesting aspects, i.e., "Japan and the East Asian Region" (Chapter VIII) and "unequal treaties" (Chapter X), he points out that the Japanese increasing influence in the East Asian and international community is based not so much on political as nonpolitical or economic treaty relations (pp. 97–98, 103–104, 161–162, 193, 200).

This book is the most comprehensive study of Japanese treaties yet attempted. The statement supported by the novel methodology is persuasive and suggestive. Besides being enlightening on the law of treaties of Japan, it provides the studies on international relations of Japan with useful fundamental materials. I wish, however, that the author had referred to more constitutional topics: there are some problems the author does not mention (e.g., the meaning of "prior or subsequent approval of the Diet" in the conclusion of treaties); some points are dealt with in greater detail theoretically in Japan (e.g., the place of treaties in national legal order); and there are some problems on which the government view is expressed although this book merely introduces various theories on them (e.g., the proclamation by the Emperor, relationship between treaties and the Constitution, etc.). Moreover, though Adams's book is said to have made use of documents associated with the International Law Commission and the Vienna Conference on the Law of Treaties "in order to establish the more theoretical arguments and aspects of the Japanese view of the law of treaties" (p. 9), I think it would have been better to amplify the plan more positively. While this is work of codification, it is an interesting problem what kind of role Japan played for harmonizing stability and change in the treaty order at the Vienna Conference on the Law of Treaties as a member of Asia and advanced nations. In this connection, in discussing the role of the Japanese treaties I wish the author had referred to the so-called three diplomatic principles of Japan: (a) the United Nations-centered policy, (b) cooperation with the free nations, and (c) adherence to the position as a member of Asia.

Furthermore, the book contains some minor problems in description. For instance, although it is written that Japan made reservations only to three multilateral treaties (p. 177) there are some others (e.g., Agreement Establishing the Asian Development Bank, Article 56, Paragraph 2; Convention of Road Traffic, Annex 1). With respect to the inequality in the 1951 Security Treaty between Japan and the U.S. (p. 187), the dominant Japanese opinion is that it enforces an inequality, since the U.S. is not obligated to defend Japan, while Japan is obligated to provide military bases. The revised Treaty of Mutual Cooperation and Security (1960) made the provisions equal. Also, his treatment of the contents of some treaties is so stereotyped as to lose sight of the particular significance to Japan (e.g., resource conservation agreement, p. 135).

As to the study on the law of treaties of Japan there has been a long blank since K. Matsudaira, *"Le Droit Conventionnel du Japon,"* 1931, was published, although its approach is somewhat different. This book is a valuable effort – both skillful and painstaking – to fill the gap. This book also shows clearly that Japanology in the U.S. has been extended to include the field of international law. For the future, I hope the author will expand his treatment of remaining problems he pointed out (p. 203) and that he will further explore the other fields of international law involving Japan.

HISASHI UCHIDA

Tokyo Metropolitan University

The Banks of Canada in the Commonwealth Caribbean: Economic Nationalism and Multinational Enterprises of a Medium Power. By Daniel Jay Baum. (New York: Praeger Publishers, 1974. Pp. 170. $16.50.)

Professor Baum's book examines the activities of a small group of powerful Canadian corporations – the chartered banks – which have only rarely been analyzed as political and social actors. In the major portions of his book, Baum discusses the history of Canadian banking operations in the Caribbean, the socioeconomic situation of the islands and the dominant place of Canadian banks in that socioeconomic picture, the weakness of island laws which regulate banking activity in the various Caribbean states, the attitudes of diverse segments of the Caribbean community toward the Canadian banking establishment, and the role of foreign aid in promoting regional economic development. In his concluding chapter, Baum addresses what he considers to be the key point of his volume – a condemnation of the dual standard with respect to responsible corporate behavior which he feels the Canadian banks epitomize and the Canadian government condones. Canada, a country so deeply concerned about the impact of the multinational enterprise on its own society that it has promulgated guidelines for good corporate behavior and established a review agency to screen takeovers and new investment, has taken no steps to insure that its domestically-based multinational enterprises – the banks – "conduct themselves as 'good corporate citizens' in developing nations" (p. 136).

The major weakness of Baum's book is the relationship he imputes between the Canadian banks and the Canadian government. The banks are variously described as "a mechanism for the achievement of national policy" (p. 1), "an extension of the Canadian state" (p. 50), and "appendages of the Canadian government" (p. 156). There is no doubt that the Canadian government does maintain tight control over domestic banking activity through the Canadian Bank Act. It is a big leap, however, to move from acknowledging regulation of bank charters and assets to perceiving the banks as instrumentalities of the government. It is a further jump to suggest that "in a legal and realistic sense, the federal government has the power to control banking behavior in the Commonwealth Caribbean" (p. 19). That the banks are not agents of the Canadian government or institutions for the attainment of national policy is very clear, for example, from efforts made by the Liberal administration of Prime Minister Pearson through the Bank of Canada during situations of domestic economic stress in 1965 and 1968 to influence loan policies of the Canadian banks to prevent the transfer of large sums of money outside the country. Attempts to control the overseas operations of Canadian banks, whether realistic or not from Baum's perspective, would smack of the same kind of extraterritoriality that so antagonizes Canadians. Morality was not acceptable to Canadians as an excuse for external regulation of the economic activities of foreign subsidiaries located in Canada.

Unquestionably, Canadian banks in the Caribbean could be more responsive to the problems of developing economies and to the growing nationalism of the region. In his condemnation of the banks' conduct, however, Baum seems to imply that they should abandon traditional business principles for innovation, if not uncertainty. After describing an offer by one of the banks to the Bahamian government to provide financial assistance for the completion of a stalled housing project, an offer to which the government did not respond, Baum writes, "The matter was dropped, for the banks were unwilling to do anything more. They were unwilling . . . to write a blank check or induce the government to complete the project" (pp. 61–2). For good corporate citizenship to be a criterion for evaluating behavior, the banks must be apprised of the host government's expectations.

Baum's efforts would have been more successful if he had considered in greater detail some of the interesting proposals he merely mentions, for example, that the banks actively participate in Canadian aid schemes in the Caribbean, and devoted less space to hackneyed themes such as the transmission of values through aid or nationalism versus business rationality. Baum conducted many interviews before writing his book, yet he does not appear to have discussed his subject with Canadian officials responsible for drafting either the Bank Act or aid programs for the Caribbean. In short, Baum has highlighted a fascinating quandry in Canadian domestic and foreign policy. Too frequently his discussion of the dilemma becomes entangled in his own web of preferences and confusions.

MAUREEN APPEL MOLOT

Carleton University

Problems and Approaches for Solution of an Association Between Fiji/Tonga/Western Samoa and the European Community. By Klaus Billerbeck. (Berlin: German Development Institute, 1974. Pp. 70. DM 5.)

"Fiji, Tonga and Western Samoa in particular, have succeeded in creating a developmental concept which only partially corresponds to Western ideas of economic and social development and which gives priority to the protection of South Pacific culture," the author tells us (p. 37). Given their distinctive character, limited economies, and the range of possible relationships with other countries of the South Pacific, future cooperation between these three associable states and the European Community becomes the "subject under inquiry."

During the course of Billerbeck's thorough but tedious exercise, the various Pacific regional organizations come in for brief description and their compatibility with Economic Community membership for consideration: in their order of age, South Pacific Commission, Pacific Islands Producers' Association, South Pacific Forum, and the latter's economic arm, South Pacific Bureau for Economic Cooperation. Similarly, the prospects of industrial cooperation, Fiji's sugar problem, and arrangements for copra products of all three are examined. My tentative conclusion: that all theoretical approaches to EC associated status for the three countries are not viable, and a Pacific regional association agreement offers the most promising solution.

NORMAN MELLER

University of Hawaii

In the Human Interest. By Lester R. Brown. (New York: W.W.Norton, 1974. Pp. 190. $6.95.)

Lester Brown, well-known writer on world food problems and President of the new Worldwatch Institute, is a prolific professional worrier. Understandably in the World Population Year he has churned out a book about the dreadful consequences of too rapid global population growth and how such growth can first be reduced and then halted by the year 2015. Since he is also worried about and writing books on food, energy, and environmental catastrophes there is no doubt considerable efficiency in seeking to weave all these themes together. The result is a picture of a tired, overpopulated, mostly underfed, polluting people making a mess of the one planet that is theirs so far to possess. Yet with the help of a "new social ethic," "workable world order," "better leadership," and fewer people breeding less often it can somehow be all put right.

Lester Brown means well, and his writings on the green revolution played an important role in first publicizing the possibilities of the hybrid seeds. But in adding world population, with wide helpings of energy, ecology, and sundry other topics to his plate he leaves his readers with a starchy diet largely devoid of intellectual proteins. This hasty potpourri skips from topic to topic (three pages for abortion laws around the world) and is calculated to infuriate the knowledgeable and to confuse the layman. What passes for evidence in support of gross generalizations comes sometimes in percentages rather than absolute figures or is very selectively culled. Widely known contrary evidence, especially on relationships between fertility rates and socioeconomic variables, is conspicuously absent. This is a hard-sell attempt to plug voluntary world zero population growth by 2015, which is at least thirty-five years before leading demographers consider it to be feasible.

The case for rapidly stabilizing world population rests on a series of alleged causal links between population growth and global food, energy, and mineral resources, as well as global politics, economics and ecology. Although the advantages of reduced fertility to individuals, families, and nation-states are now well-established and widely accepted, they do not add up to an argument for world ZPG. Hence Brown has taken on an intellectual task of the first order, has equipped himself with a broad spectrum of disciplinary tools, but has ultimately failed to convince this reader that what he is advocating is either desirable or practical. One reason for this failure is that in making fertility responsible for so many of the planet earth's problems he has made a chain of shaky, specious, and often invalid causal links. More may not equal better, but neither does fewer.

The introduction sketches the population explosion and ties it to global shortages of energy (undocumented), and food. The basis for population projections is ignored, particularly the importance of youthful age distributions and child-bearing in perpetuating rapid increases. The rich are warned that because of the politics of energy scarcity, there will be no easy technological solutions and standards of living may fall or stagnate. Chronic global food scarcity for the foreseeable future is predicted, with the United States asked to contribute to a permanent multilateral world food reserve managed by the FAO. A host of ecological horrors, from overfished seas to eutrophied lakes, is ticked off and rather unsuccessfully tied to population growth. Neither the pollution of Lake Erie nor the drought in the Sahelian region of West Africa really have much to do with human fertility, nor with one another unless you buy the Brown thesis that we are all dependent on common and fast shrinking global resources.

A strict one-worlder, Brown in his chapter "The Global Politics of Resource Scarcity" wants us to shift from expanding to redividing the world economic pie. The rich are to stop growing or at least to slow down consumption so that the entire human race can have an "acceptable" level of living. Americans should start by driving smaller cars, having two or less children, and substituting vegetable for animal fats, e.g., margarine for butter and vegetable for animal proteins. The Third World should pay careful attention to the "Chinese model" although Brown is careful not to endorse it or to discuss its ideology.

Part II of the book focuses on fertility and how to stabilize world population at 5.8 billion by 2015. There is much misinformation on the alleged mass discontent of Third World migrants, and a facile plea that education, health services, employment and status for women, and much better income distribution can rapidly reduce fertility. The problem is that, as in China, these reforms on a national scale would require a societal upheaval. Even then it is prudent to anticipate that a generation may be needed before fertility falls rapidly after infant mortality has been drastically cut, and other social services provided.

Two grossly optimistic chapters discuss population policies and contraceptive methods. Contrary to Brown's assertion, there is no safe, reliable, inexpensive contraceptive available or in sight in the research labs. Most governments have neither the means nor the will to push rapid fertility reduction. His strategy for stabilizing developed country populations by 1985 and the world by 2015 is a pastiche of universal literacy, universal family planning services, mass employment for women, accelerated labor-intensive rural development, and large-scale transfers of food and other resources from rich to poor countries. The alternative is a Malthusian scenario of rising Third World death rates from famine with "food scarcity, continuously rising prices, and growing political instability. Governments would change hands with increasing frequency" (p. 177).

Population growth is a serious problem meriting sober treatment which this book does not provide. Through overstatement it weakens a good case.

AARON LEE SEGAL

Cornell University

American Economic Aid to Thailand. By J. Alexander Caldwell. (Lexington, Mass.: Lexington Books, 1974. Pp. 248. $16.00.)

Caldwell's book provides a comprehensive survey of the American aid program to Thailand since its inception. As a survey it can hardly fail to be interesting, given the inherent interest of the subject: the attempt of the United States to lend a hand in modernizing the Thai economy and administration in the hope of promoting a successful resistance to Communist insurgency. The usual pitfalls of such cross-cultural and politically motivated assistance were there, and some of the pits were predictably fallen into: an ethnocentrism on both sides, differing goals for the same programs; short assignments and an abysmal level of area expertise and Thai language skills in the AID mission; the inadequate attention devoted by the mission to developing a high-quality Thai staff; more fundamentally, concentration on infrastructure and bureaucratic development rather than on basic educational and political development.

But what is perhaps noteworthy is that despite all these shortcomings some American AID projects did succeed to a remarkable degree. Caldwell, who was based in the AID mission in Bangkok for two years, with access to individuals and files, attempts to "identify past determinants of success and failure in the American aid program in Thailand," using "a combination of some of the analytical tools that have been developed for general studies of international aid with some of those created for the analysis of specific projects . . . to . . . permit a fruitful and hitherto unexploited mode of enquiry into the issues posed by international development assistance" (p. 2). If this seems a rather overblown description of an analytical method which depends mainly on a commonsense evaluation of goals, methods and results, its findings are nevertheless valuable. Aid programs which work are on the whole those involving transfer of relatively simple technology; the need for such technology was clear to the Thai officials involved. Projects requiring institutional change succeeded less frequently. The success of institutional development efforts was heavily influenced by the quality of Thai leadership, the degree of interdepartmental cooperation needed, and the perception by other agencies of the importance of the aided institution's role.

Funding patterns of American aid through the 1950s and 1960s, both the country mix of aid and the project mix within a given country, clearly reveal the extent to which resistance to further Communist advances, especially in Asia, was uppermost in the minds of those who approved budgets, if not to the local administrators of aid programs. Since economic progress at the village level was seen as essential in building villager loyalty to the government (an assumption which Caldwell questions), there was no necessary conflict between the aim of "counter-subversion" and that of economic development. For example, the building of a network of paved highways in Thailand's northeast no doubt contributed to meeting both objectives. The regional balance of aid projects was thereby distorted, however. In Caldwell's view, inadequate attention was given to aspects such as quality of villager-official relationships, the effective extension of administrative control over threatened areas, and the nation-building potential of the Thai primary education system. Despite continued efforts, an effective, coordinated system of village security units was not achieved. Some of these shortcomings were no doubt inevitable, given the corruption of the military government and the patronage involved in key provincial government appointments. This inherent weakness of the system receives little attention from Caldwell, who chooses to stress rather the stability and basic competence of the Thai civil service. Since the rapacity of governors and high officials in certain *changwats* was no secret, I would have preferred to see more analysis of how much these factors hindered the success of programs which in a different context might have succeeded.

The AID mission in Bangkok is inordinately fond of abbreviations in place of the full name of various agencies or organizations. Thus Caldwell's book is heavily and rather confusingly spiced with USOM's, RTG's, NEARC's, ARD's, PAS's and so forth. To a degree this can be forgiven, but not in the case of the CT's (Communist terrorists) who are first referred to with precisely that abbreviation on p. 127 and then only once more (as CT's) on p. 144, by which time the reader has almost certainly forgotten who they are. Apart from such quibbles Caldwell's book is a sensible and perceptive treatment of the strengths and weaknesses of one of the United States' major aid programs, as evaluated by an American "insider." Caldwell has done a thorough job, and his book deserves careful reading by aid donors, administrators, and recipients everywhere.

GAVIN W. JONES

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Documents on Canadian External Relations 1909–1941. Seven Volumes. By the Canadian Department of External Affairs. (Ottawa: Information Canada, 1967–1974.)

This collection of documents is similar to the U.S. series, *Foreign Relations*, begun in 1861 and published under similar titles since then with special volumes supplementing the annual editions. The Canadian collection begins in 1909 because it was in this year that the Department of External Affairs was established. The purpose of the Canadian collection is to make public a selection of documents which tells the "basic story" of the formulation and implementation of Canadian external relations.

It is a fascinating story which documents the uniqueness of the Canadian international experience and belies assumptions that Canada is "just like" the U.S. Indeed, the difficulties Canada has encountered in registering and maintaining an international presence are perhaps not fully appreciated by Americans. Canada was isolated in North America with the U.S. and subject to the influences and vagaries of overwhelming U.S. power. It was simultaneously attempting to reconcile this North American position with its position in the British Empire, while engaged in a drive for autonomy from Great Britain. Moreover, Canada was subject to all the domestic necessities of a fragmented pluralistic society spread across a vast geographical expanse. In short, Canada accomplished an extraordinary feat by taking its recognized place in the international community, and herein lies the significance of this collection of documents.

The years 1909–1941 are thus crucial formative years in the Canadian international experience. Canada's independence can be formally dated with the 1931 Statute of Westminster, and it had no embassies abroad until 1943, although its legations date back to 1927. Canadian diplomats during this period were more cautious than innovative, and the documents in this collection reflect a diplomatic competence coupled with a sense of national purpose that was more secure in the knowledge of what Canada did not want than what it wanted. This collection very well captures the drama of the Canadian foreign policy experience, including the evolution of Canadian autonomy and the Imperial Conferences, Canadian relations toward the U.S., Canadian reaction to international events ranging from World War I to the 1922 Chanak incident, and the Canadian approach to international organizations and specific countries other than the U.K. and the U.S.

The editors of the volumes have been given maximum leeway in surveying and publishing documents; their selection of documents generally reflects solid historical judgment; and the organization of the volumes is, while rather uneven, generally useful. The overall purpose of the collection has clearly been achieved, and it stands as an invaluable research tool and teaching device for those interested in Canada itself as well as those interested in comparative examinations of "nonsuperpowers" i.e., political scientists as well as historians. For example, the historical themes involved in Canada's attempt to reconcile and balance conflicting North American and international interests continue to this day. Indeed, these historical themes have a surprisingly contemporary ring given the current effort of the Canadian government to assert "autonomy" from the U.S. by attempting to invoke such counterweights as a strengthened link with Europe to offset the preponderant U.S. influence on Canada.

Unlike the U.S. series, *Foreign Relations,* which was compiled by resident historians in the State Department's Historical Office, the Canadian collection was edited by professional historians working with the Department of External Affairs. Although not so indicated, Volume I of the Canadian collection, which covers the years 1909–1918, was under the supervision of D. P. deT. Glazebrook, who apparently was one of the prime movers in the creation of the entire collection. Volume II was compiled by R. A. MacKay, and covers the Paris Conference of 1919; Lovell Clark's Volume III covers 1919–1925; Alex Inglis's Volume IV covers 1926–1930, and his Volume V covers 1931–1935; John Munro's Volume VI covers 1936–1939; and David Murray's Volume VII covers 1939–1941. Canada currently has a thirty-year rule governing the release of public documents, although this might be altered to twenty-five years, and work is presently underway on future volumes in an attempt to bring the collection up to the middle 1940s. David Murray is preparing a second volume covering 1939–1941, John Hilliker's two volumes will cover 1942–43, Eric Gillmore's two volumes will cover 1944–45, and Donald Page will prepare separate volumes for 1946 and 1947.

This collection is not definitive, nor do the editors claim that it is. The inclusiveness of these volumes is affected by the variable number of years covered by the separate volumes (ranging from nine years to one year), and by the fact that while the editors attempt to include other relevant departmental files and collections of papers, they concentrate on those of the Department of External Affairs. Because we do not know what documents were available to the editors, we cannot tell how representative their sampling is. In general, the reader must trust the editors' "historical objectivity," and in general this trust is well placed. It should be acknowledged, however, that had other editors compiled these volumes, there would have been differences in emphases and inclusions. I can cite four examples of important documents that should have been included but were not: the July 1919 Christie Memo on the Paris Peace Conference and its effect on Canada as an "international person," the 1935 Skelton Memo on Canadian policy towards the Italian-Ethiopian crisis, the Skelton defense brief which preceded Prime Minister King's February 1937 House of Commons statement on defense policy, and the August 1939 Skelton Memo which contained the major Canadian statement of war aims.

It is not the quality of the editors or their judgment that raises questions about this collection, but the guidelines under which they were operating. The lack of specific criteria of selection is disturbing, and this coupled with the absence of a general editor raises questions about the continuity of criteria. For example, Volume I emphasizes policy implementation, Volume IV states that there is a "greater emphasis" on the formulation of policy (p. ix), and Volume VII states that "basic guidelines" have been altered because the Department of External Affairs recently prepared a revised set (p. ix). If these revised guidelines are observed, the criteria of selection will, one hopes, be less variable with future volumes. Another major problem appears to have been resolved with Volume VII. Somewhat inexplicably, the first six volumes do not indicate the source of the documents, an omission that is corrected in the last volume by indicating the departmental file number or the name of the collection from which each document was taken. An additional useful step would be the appointment of a general editor for the entire collection to insure standardized criteria and format. Finally, the collection could be usefully expanded by commissioning such supplemental volumes as a "catch-up" edition covering the period from Canada's 1867 Confederation to 1909, a special edition consisting of the Prime Ministers' foreign policy papers, and with future public availability of documents, supplemental editions on such dimensions as Canada's role in the origins of the UN and NATO.

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Population Dynamics and International Violence: Propositions, Insights and Evidence. By Nazli Choucri. (Lexington, Mass.: D.C. Heath and Company, 1974. Pp. 281. $18.00.)

This volume by Professor Nazli Choucri constitutes a rare effort to explore in a systematic way the connection between outbreaks of violence in international relations and the population dynamics of the system. A strong tie between the two has long been suspected, and scholars interested in the problem have repeatedly signalled the presence of possible causal links. But suspicions never progressed to proof; in the past forty years less than half a dozen systematic studies have appeared, and much of this terrain has remained *terra incognita.* Such past inattention to the problem is highly regrettable and not easy to explain in view of the fact that the observation of the mechanisms that mediate the effects of population dynamics on political behavior generally, and violent behavior specifically, also afford a rare opportunity to observe behaviors critical to the working of any social order. This book represents a first step toward regaining lost ground; it carries on an informed exploration of a critically important corner of the field, and equally laudably, it presents its material in such a manner that the bulk of the findings form a natural point of departure for new research.

This volume is divided into three parts. The first portion of the work is based on an exhaustive study of the literature dealing directly and indirectly with the relations between population dynamics and international violence. The literature is extensive, and in these materials the study of population and violence seems to have been tackled in two ways. One set of scholars saw the relationship as indirect; population became critical only in conjunction with the movement of other socio-economic and political variables in creating major shifts in the distribution of international power which, in turn, were alleged to have caused international violence. A second set of scholars, on the other hand, saw direct causal links between demographic change and the use of force in international relations. In their view a massive population size, or high population densities, or high population growth could and did induce one nation to attack neighbors with abundant space and natural resources and sparse population of their own. Professor Choucri quite rightly takes a highly disapproving view of such oversimple notions. And, not surprisingly, it is the first approach rather than the second that produces the small number of useful propositions that result from the survey of writings in this field.

One should also note that the dissatisfactions Professor Choucri has with the literature she reviewed are not peculiar to the field of political demography and will sound familiar to any one who has read widely in any of the social sciences. The main defects she lists seem to be as follows: The literature is redundant, since authors repeat ideas and observations also found in every other book on the subject. Fundamental relations between violence and population dynamics are set down loosely and vaguely, with insights unsupported by evidence, and with most discussion dealing with the experience of one country at one point in time. Quite frequently, no propositions at all could be constructed out of such materials, making it understandable that, at the end of the book, in her own discussion of research imperatives, Ms. Choucri suggests that future research should be scientific, empirical, cross-national, longitudinal, and relevant. These are certainly commandments that ought to be heeded.

A second portion of the volume focuses entirely on empirical materials gathered from an examination of violence in the developing world and seeks to explore systematically whether population changes have influenced the beginnings or the outcomes of violent conflicts. The set of propositions formulated from the theoretical literature in Part I are then confronted in Part II with the evidence generated from a sample of forty-five cases of violent conflicts in the developing world since World War II. "[This] analysis," Ms. Choucri writes, "in no way constitutes a test of these propositions in the conventional sense; but it does provide a systematic means of assessing the extent to which these propositions are consistent with our empirical observations of population and conflict in the developing areas" (p. 81). The choice of the modernizing world as the proper setting to test propositions drawn from a body of writing concerned for the most part with the behavior of large European industrial countries is to permit one to evaluate the applicability of propositions beyond the confines of the experience on which these propositions were based. Interestingly enough, many of the propositions that were culled out of the theoretical literature survive the confrontation with the evidence quite well. The coda of Part II presents an empirical propositional inventory that should be considered a major contribution of this volume.

Part III consists of a review of major themes, a section of concluding statements, and a short chapter given over to discussion of issues of policy and research in the field connecting demographic and political behavior. The discussion of what research ought to be done is much too short. As a rule, my complaints go the other way, lamenting that there is too little research being done and too much talk about what research ought to be like; but in this case it would have been good to read a more elaborate defense of Ms. Choucri's views. I am doubtful of her position that what is needed in this corner of the field of political demography is research on public policy. Some such research is obviously required; but should the bulk of questions to be asked have the policy angle of vision? Much, of course, depends on how policy research is defined. If the definition of policy research is narrowed down to the usual terms to the study of how elites and particularly governmental elites make decisions in the population field and how such decisions are implemented through governmental institutions, then I would argue against such research having a high priority. For I believe that a concentration of research on governmental decisions to deflect unwanted population trends would prove counterproductive until we know more about how the system works, i.e., about the connections between political and demographic behavior. On the other hand, a broad definition would include the exploration of a wide range of connections between demographic and political behavior and beliefs of both mass and elites. Specifically one would be interested in how the aging, physical turnover, and social and geographic movement of a population affects the political system, as well as the influences running the other way. If the broad definition is what Professor Choucri has in mind, I would certainly subscribe to the policy research goal she sets in her conclusions. And in spite of a careful reading I am not certain where she stands on this fundamental issue.

Two or three additional comments are in order. This book represents a beginning. The knowledgeable reader will be struck, I think, by what the author has chosen to omit from this volume. Why, for example, did she not attempt to view the problem from the opposite angle of vision? In the title the author recognizes that population dynamics and international violence affect *one another*, yet she explores the flow of influence moving only in one direction, from population dynamics to violence; why not also explore the affect of violence on population dynamics? Certainly the other face is clearly intimately related to the question that Ms. Choucri has set out to study. Also, ironically, most of the literature exploring the evidence of the influence of international violence on population is the very kind of research the author advocates. Or, again, why is it that the author has not really paid attention to what has been written on governmental programs to increase fertility? Some of this work too has obvious connection with and offers important clues to the phenomena Ms. Choucri sought to explore, particularly the capacity of governmental systems to deflect demographic trends.

But having considered these omissions, the reader will also be struck how well Ms. Choucri has done what she has set out to do, and how much the intellectual capital developed in her collaboration with Robert North has gone to enrich the work here. There can be little question that our knowledge of the problem of population dynamics and international violence is now ahead of where it was when Professor Choucri began her work.

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The Warsaw Rising of 1944. By Jan M. Ciechanowski. (New York: Cambridge University Press, 1974. Pp. xx, 332. $19.50.)

The study of Polish history during World War II is definitely underdeveloped. Works that have appeared in Poland showed not only bias in the selection of themes, but also demonstrated deficiencies of scientific objectivity in the treatment of chosen topics. In the West, the situation was not ideal either. With a few exceptions, scholars were rarely attracted by the recent Polish past; research difficulties further hampered exploration of the field, while preconceived notions sometimes spoiled the effects of rare ventures. Historians of Polish origin, especially those who found themselves abroad as a result of World War II and were seemingly predestined to do work in this area, were, for the most part, unable or unwilling to tackle this highly dramatic and controversial period of Polish history. Consequently, even the Warsaw Uprising did not receive, until recently, the systematic treatment it deserves.

And yet, it was an event of significant historical importance. It not only greatly affected the fate of Poland, but also marked — according to Churchill and Kennan among others — a breaking point in the wartime cooperation between the Anglo-American Allies and the Soviet Union.

Was the insurrection necessary? Was it diplomatically and militarily prepared? Was such a horrible hecatomb justified?

Ciechanowski's answers to all these questions are decidedly negative and his argumentation is difficult to refute. His painstaking reconstruction of diplomatic and political incidents, often oddly startling, demonstrates that the chief characteristic of the uprising was the extraordinary confluence of rationality and irrationality in the Polish decision-making process. The leaders in Warsaw displayed a penchant for overreacting, while those in London failed to exercise sober judgment and judicious procedures. As a result, they forfeited all hopes for establishing a *fait accompli* of their desires.

Though Ciechanowski does not pretend to formulate any generalizations, his searching inquiry underscores the fact that leaders shaping history are not always making calm and calculated decisions. Sometimes, they confuse, defy, and even destroy the purposes they claim as their justification. To this, one could add in reference to the uprising that the underground movement was by its very nature endowed with dynamics of its own which tended to create self-fulfilling, but very dangerous, solutions. The command to start a popular insurrection in Warsaw must be seen also in this context.

Surfacing through Ciechanowski's critical study is the inexorable question of whether an agreement between Poland and the Soviet Union, even concluded on Stalin's terms, would have been respected by the Kremlin. The author has no answer to this query, taking refuge in the limp conviction that something had to be done. Perhaps, there is no clear-cut reply, and probably several alternatives were possible. Certainly the following cannot be excluded: Stalin would have accepted the Polish offer and promised to respect Polish national interests only to violate them after occupying Poland by the force of arms.

It should be noted that the subject Ciechanowski has taken for his own was long sacrosanct in Polish emigre circles, especially among former AK leaders who are now grouped in London around the "Polish Underground Study Center." As might be expected, his book caused much bitterness among them. Even though his study is marked by careful scholarship and impressive documentation, it was judged by them as biased, distorting the truth, and generally harmful.

These views appear too harsh. It might be that Ciechanowski's argumentation expresses more than he intended and than he realizes. Probably, it would be less depressing and embarrassing not to know the evidence he brought out. But more important than these reservations is the fact that his is a courageous and thought-provoking contribution to the discussion of a persistent dilemma of Polish foreign policy: idealism or realism?

Ciechanowski scorns political idealism because it exposes the Polish nation to the danger of biological extinction. But his critical attitude does not conceal his respect for the spirit of sacrifice and the essential devotion to freedom demonstrated by all actors of the Warsaw tragedy.

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Latin America and the United States: The Changing Political Realities. Edited by Julio Cotler and Richard R. Fagen. (Stanford, Ca.: Stanford University Press, 1974. Pp. 417. $17.50, cloth; $4.95, paper.)

This volume presents the revised drafts of papers and commentaries delivered at a 1972 conference in Lima funded by the Social Science Research Council and the American Council of Learned Societies and hosted by the Institute of Peruvian Studies in Lima. Edited by a distinguished Peruvian sociologist and by an equally distinguished North American political scientist, the volume focuses on the political economy of inter-American relations. The introductory essay by Cotler and Fagen provides as much integrative tissue as could possibly be demanded of a volume that includes twenty-three diverse contributors coming from intellectual traditions previously noted for speaking past, rather than to, each other. Indeed, given the fundamental differences which the conference proceedings reveal, the introductory essay stands as a monument to rational discourse in the face of formidable temptations to lapse into the cynicism of despair.

As a compendium of research to be judged by the criterion of having included articles likely to be of lasting import, however, this book falls short. I see little here that either provides a new conceptual breakthrough or states a research problem in a fashion likely to prove seminal. Yet such conclusions must be tempered with the acknowledgement that *all* of the Latin American contributors to this volume are sensitive to analytical problems which North American social scientists have only recently begun to understand. My belief is merely that not many of the articles will prove to be "classics." The classics of *dependencia* literature remain in Spanish in a variety of sources. With a few exceptions, to be mentioned below, the Latin American contributions exhibit serious limitations, as do the North American contributions.

Indeed, many articles seem to be characterized by the recycling of concepts or of data already in the public domain. The recycled *dependencia* concepts are not necessarily more or less useful as a result of recycling. And the three "bureaucratic politics" contributions by North Americans are admittedly derivative of the conceptual framework first introduced by Graham Allison, the utility of which has been debated at length by non-Latin Americanists. The recycled data, however, have not always been accessible to other scholars. Provision of such data is a valuable service. But the value of scarce data does not obviate the unfortunate fact that, for those who wish to analyze U.S.-Latin American relations from the perspective of *dependencia*, data-gathering remains a most difficult task. How does one collect data *systematically* on the political constraints imposed by a condition of dependence on the United States?

It is perhaps indicative of the difficulty of acquiring such data that the most empirically useful contributions by Latins are from those who happen to be affiliated with atypically strong institutional support systems. Anibal Pinto's article on "Economic Relations Between Latin America and the United States" obviously profited from his association with the Economic Commission for Latin America. Having access to the most recent trade, investment, balance of payments, and capital flow data increases the credibility of his assertion that "Latin America has provided substantial support to the United States in its recent balance of payments crisis, a support that has far exceeded the benefits to Latin America deriving from direct U.S. investment." The documentation of these trends does not guarantee their interpretation from a *dependencia* perspective by those not previously so inclined. The documentation does, however, lend an aura of empirical respectability to a contention which, without data, will strike unsympathetic readers as mere polemic.

Similarly, Luciano Martins, writing on "The Politics of U.S. Multinational Corporations in Latin America," would seem to have profited from the institutional support derived from association with the University of Paris. This is not to suggest that Martins would have been incapable of challenging the ideology of "multinational corporations as saviors" had he not been in Paris, merely that the difficulties of acquiring data *from within Latin America* would have made his case much less compelling.

Both of these exceptionally meritorious articles illustrate a number of propositions consistent with dependency theory itself. Where a condition of dependence exists, conservative authoritarian governments are likely to prove convenient to the metropolitan neocolonial power and, hence, to be sustained. The persistence of such authoritarian regimes further weakens the capacity of indigenous social scientists to analyze the international forces operative within their societies. Hence to argue that many of the contributions by the Latins are empirically weak is no damning indictment; it is a mere statement that confining conditions associated with national dependence impair the quality of social science which might otherwise be produced. At their worst, the Latin Americans contribute an example or two of the "what I am thinking about topic X at time Y" variety, of the sort that many "political development theorists" produced in the U.S. in the 1960s. As I see it, the Latins have an excuse, while the North Americans didn't.

So much for sweeping judgments and the sociology of knowledge. One question remains about the Latin American contributions: What else is especially worthy of note? For sheer intellectual rigor, I recommend Guillermo O'Donnell's commentary on Ernest R. May's article on U.S.-Argentine relations. For descriptive materials which might prove new to some (they were to me), I suggest the articles by Olga Pellicer de Brody and by Carlos Estevem Martins on Mexican-U.S. relations and on Brazilian-U.S. relations, respectively. For evidence that Project Camelot is gone but not forgotten in the 1970s, see the article by John Saxe-Fernandez.

In conclusion, this book makes a twofold contribution to scholarly discourse. First, there is a symbolic message conveyed by the joint publication in English and in Spanish, by the co-editorship of distinguished Latin and North American social scientists, and by the format of Latins commenting on research by North Americans in a highly critical manner. The symbolic message is quite simple: Latin American social science must be taken seriously by North American scholars. It cannot be discarded as irrelevant and trivial because it employs an alien and sometimes seemingly polemical set of concepts. The research agenda proposed by Latin American social scientists is as important as, and probably more important than, any alternative agenda constructed by their North American counterparts.

Second, this is a book which the profession of political science, as practiced in the United States, needs more than the subfield. Any good Latin Americanist will have been attuned to the issues discussed in Cotler and Fagen long ago. For that reason this book will appear on the bookshelves of competent Latin Americanists regardless of what I say in this review. The book is perhaps most needed by the international relations scholar who, excepting the Cubas and Chiles, has never really encountered any reason to believe that Latin America is terribly consequential. This book might help such individuals to understand that the eyeglasses through which the Third World is coming to view the United States have been ground by Latin American oculists and that these lenses focus on irreconcilable conflict between incompatible national interests not because of imperfections in the grinding. If we have not previously so believed, it is because of the lenses *we* wear. There is some bona-fide empirical theory in that neo-Marxist conceptual language which deserves our attention.

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The Western Mediterranean: Its Political, Economic, and Strategic Importance. Edited by Alvin J. Cottrell and James D. Theberge. (New York: Praeger Publishers, 1974. Pp. 282. $17.50.)

Published in cooperation with The Georgetown University Center for Strategic and International Studies, of which Dr. Cottrell is Director of Research, this is a volume of the Praeger Special Studies in International Politics and Government. It is based on papers presented at The Conference on the Western Mediterranean at Palma, Majorca, during September 23–24, 1972, in which thirty-three experts from seven countries participated in discussions of current developments and future prospects in the Western Mediterranean basin. The papers focused in the littoral states of the Western Mediterranean – North Africa (the Maghreb and Libya), Spain, France, and Italy, and the manifestations of change which, hitherto, seemed somewhat overshadowed by the challenges and developments within the Eastern Mediterranean.

As Dr. Cottrell observes in his introduction, the Inland Sea, from one end to the other, has seen the rise and fall of empires and has been the cradle of three of the world's major religions – Judaism, Christianity and Islam. It has retained great political, commercial, and strategic significance with the passage of the centuries, but for a variety of reasons, Western attention has been focused on the Eastern Mediterranean since the end of World War II in 1945. In the immediate postwar era attention centered on the Arab-Israel conflict, as it still does, as well as upon Soviet pressures on Greece and Turkey (especially during 1945–1947), the Suez conflict in 1956, American intervention in

Lebanon in 1958, and the rapid Soviet substitution for Western interest in the Eastern Mediterranean. The background of violence and crisis sustains a strong Western Mediterranean. Events in the Western Mediterranean attracted much less attention, a situation which now appears to be changing somewhat. While the problem of political development appears to be a key issue in some states, it is also true that Soviet naval interest is now actively interested in the Western Mediterranean, and Soviet policy is directed toward acquiring political influence in the Maghreb, and especially in Algeria, to reduce Western influence and deny bases to the West.

Essentially, the papers are presented under six basic themes, the first of which deals with the political and strategic aspects of the Western Mediterranean. Dr. Curt Gasteyger, Deputy Director of The Atlantic Institute for International Affairs in Paris, discusses the challenges and changes in the Western Mediterranean. Professor P. J. Vatikiotis, of the University of London, analyzes East-West Arab relations. Part II treats of oil trade and development, with Philip H. Trezise, formerly Assistant Secretary of State for Economic Affairs and currently Senior Fellow at the Brookings Institution, leading off with a discussion of economic relations among the Maghreb states, the EEC, and the United States. K. S. MacLachlan and R. M. Burrell, of the University of London, lucidly discuss the role of Algerian and Libyan oil and gas in the Western energy supply, in a chapter well buttressed with tabular data. Part III treats of the northern littoral states, with Professor Edward Kolodziej, of the University of Illinois, discussing the French position in the Western Mediterranean, especially in the post-de Gaulle era. Stefano Silvestri, of the Instituto Affari Internazionali, analyzes the Italian position, and Benjamin Wells *(New York Times)*, discusses Spain and the Maghreb.

Part IV surveys the southern littoral states along the North African coast. Charles F. Gallagher, Director of Studies of The American Universities Field Staff, treats of the Maghreb and Europe, while Michael Wall analyzes politics in the Maghreb, and Waldo Dubberstein, Professor of International Relations, the National War College, Washington, D.C., discusses the intriguing subject of Libya's relations with the United States, the United Kingdom, the USSR, France, Africa, and the Middle East. Part V, written by Dr. Cottrell and Air Marshall Stewart Menaul, Director-General of The Royal United Services Institute for Defense Studies, London, discusses the very special problems of Gibraltar and Malta – the future of Gibraltar and the position of Malta in the processes of change in the Western Mediterranean.

Part VI, which concludes this volume, deals with external powers and their impact on the Western Mediterranean. It begins with a chapter on U.S. security policy in the Western Mediterranean – Spain and North Africa – by Dean Edmund C. Gullion, of the Fletcher School of Law and Diplomacy, former Ambassador to the Congo Republic. Walter Laqueur, of Tel Aviv University, writes on the USSR and the Maghreb. Dr. Cottrell and Wynfred Joshua, of the Stanford Research Institute, close the volume with an analysis of American-Soviet rivalry in the Mediterranean. They insist that Soviet policies have deliberately sought to link the intra-Arab split and the Arab-Israeli conflict with Soviet-American rivalry. They also note that the USSR experienced both a major political setback in Egypt in 1972 and a loss of stature vis-a-vis the U.S. in the Middle East as well. Nevertheless, we are told, the Soviet naval position in the Middle East has changed little, although the American position in the Mediterranean steadily deteriorated in the last ten years and can hardly be restored. They caution against the USSR becoming predominant in the Mediterranean, and call for strengthening southern NATO members against Soviet coercion. They also believe it imperative to protect Western Europe as the frontier of the U.S., a point which they feel justifies the continued American presence in the Mediterranean. They also contend that tangible evidence of American concern, preferably reflected in an improved Sixth Fleet capability, would help to convey to the southern NATO members that there is still an alternative to a neutralist or pro-Soviet position. It would go far, they believe, toward maintaining a political environment which would permit the Mediterranean nations to choose their own destiny. How much the events of 1974–1975 in the Mediterranean area have modified these views is a question.

The volume, with its different and disparate contributions, performs a useful service in stressing the significance of the Western Mediterranean. The authors are well aware, of course, that, in the last analysis, the West cannot be separated from the East, whatever the different problems involved. No novel interpretations or judgments and few unknown facts or developments are presented. Aside from notes to chapters, there is no bibliography. The volume should be read carefully along with many other volumes dealing with the many other aspects of the problems discussed. There are fifteen tables, largely dealing with oil, which, however, do not bring the story beyond 1971.

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The Yom Kippur War: Israel and the Jewish People. Edited by Moshe Davis. (New York: Arno Press, 1974. Pp. 362. $9.00.)

This is an unusual and uneven book in both form and substance. First, while being a volume about the 1973 Arab-Israel War, it does not deal at all with the military aspects of that confrontation, its subsequent diplomatic and political aftermath, or the ensuing oil boycott. Rather it treats Israel's position on the domestic scene, but defines that home front broadly to cover the entire world Jewish community.

While somewhat novel in this approach, the book indicates an increased concern of late with the long neglected domestic theaters of the Arab-Israel conflict. This new concern is reflected in the popular press and media, which are intrigued by the political influence of American Jews vis-a-vis Israel (see, for example, "American Jews and Israel," *Time*, March 10, 1975, pp. 14–28; or Stephen D. Isaacs, *Jews and American Politics*, Garden City: Doubleday, 1974), and by the more recent impact of Arab investments on local economies and politics.

Similarly, political scientists concerned with international relations in the Middle East have become increasingly aware of domestic sources of foreign policy in the Middle Eastern states themselves (as, for example, Michael Brecher, *The Foreign Policy System of Israel*, New Haven: Yale University Press, 1972). The book here under review adds another, transnational dimension to the consideration of the impact of the Arab-Israel conflict on domestic politics and of domestic politics on foreign policy vis-a-vis that conflict.

The volume's second unusual, and less engaging characteristic is its methodology, or more precisely, lack thereof. The work of twenty-seven contributors, the book makes no pretense at methodological sophistication or even organizational rigor. The contributions vary in size, quality and type: a few analytical articles by respected academics; numerous impressionistic accounts and reminiscences by on-the-scene observers; interviews with participants in pro-Israel activities; public statements on the war by various groups; letters from prominent intellectuals, etc.

The key to this methodological mess lies in the origins of the volume. It is the product of a specially convened seminar on World Jewry and the Yom Kippur War held at the Institute for Contemporary Jewry at the Hebrew University of Jerusalem. Its aim was to gather as much documentary and oral evidence as possible. As such, the book gets its overall significance more as a source of (or as a guide to further) evidence on a major political event than as a definitive analysis thereof. What it lacks in coherence – which could have been somewhat remedied by more rigorous editing – it makes up in spontaneity. One gets the sense of immediacy, of the desire to record initial impressions before the dust settles and time is available for leisurely hindsight.

The foregoing is not meant to imply that the book lacks an organizational framework entirely. It is divided into two parts, the first consisting of brief analytical and survey articles on the response to the war by the Jewish communities, by intellectuals (Jewish and non-Jewish), by public opinion and by governments in particular geographic areas: the United States, Canada, Latin America, Great Britain, Oceania and South Africa, Western Europe, the Soviet Union and Eastern Europe, Asia and Africa, and Israel itself. The second section, more like an appendix, presents a small, representative selection of the oral documentation gathered in each of these regions. The first section is far more significant for political scientists.

But even that section is uneven. The American Jewish reaction occupies one-third of this section, perhaps as befits the world's largest and most politically powerful Jewish community. By far the most significant of the five articles on the U.S., and indeed in the book as a whole, is that of political scientist David J. Elazar.

Elazar sees the response of the American Jewish community to the Yom Kippur was as fourfold: political action, fund raising, volunteering, and public identification.

The political activity of the organized American Jewish community is of two types: (1) direct political lobbying in the executive and legislative branches, and (2) "community relations," i.e., cultivating positive public opinion on Israel via the media and extensive intergroup alliances. Each is conducted by a bewildering variety of Jewish organizations and agencies, which Elazar outlines succinctly.

Despite the sophisticated and diverse political machinery, Elazar concludes that the chief consequence of the Yom Kippur war on the American Jewish community has been "the increased centralization of power and authority," particularly in the hands of the fund-raising local Jewish federations (p. 31).

Critical of the growing centrality of fund-raising, Mordecai Waxman, in his article on the Conservative Jewry movement's reaction to the war, urges greater emphasis on the explicitly political tools of public opinion molding and intergroup alliance building in order to build favorable American public opinion for Israel.

Norman Lamm's essay on the Orthodox community's reaction to the war discerns the differences of opinion between Orthodox Zionists, Orthodox anti-Zionists, and Orthodox non-Zionists, while Alfred Gottschalk's report on Reform Judaism's active pro-Israel role emphasizes the transformation that Reform has undergone since the days of its own anti-Zionist origins.

While the overall impression left by the contributors is that of the solidarity of the Jewish people behind Israel, there is at the same time a thread of isolation and abandonment, particularly by intellectuals, which runs through the book. Veteran Zionist intellectual Marie Syrkin decries this phenomenon, which she attributes as much to the "battle fatigue" of pro-Israel intellectuals, both Jewish and non-Jewish, as to the increasing militancy of Israel's antagonists on the New Left, Old Right, Third World and elsewhere. Recent events, such as the UN General Assembly's condemnation of Zionism – Jewish nationalism – as a form of racism, only serve to reinforce this sense of Jewish aloneness, and to make another Middle East war, and another volume such as this, all the more likely.

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West German Trade with the East: The Political Dimension. By Robert W. Dean. (New York: Praeger Publishers, 1974. Pp. xvi, 269. $17.50.)

It has become a commonplace to suggest that a pervasive shift of emphasis, and relevance, has taken place in world politics: away from the primacy of military-strategic elements of power toward the primacy of economic elements. This shift (illustrated most recently in the increased political clout of OPEC countries, most of which are deficient in military power) has affected every important set of relations among the major groupings in contemporary international politics – between the developed and underdeveloped nations; between the two nuclear superpowers; among the United States, Western Europe and Japan; among Western and Eastern Europe and the Soviet Union. Indeed, it seems as if in many instances of current international problems the issue is not only the resolution of such "technical" questions as arms control agreements, trade negotiations, monetary reform, energy policy, etc., but also a tentative re-examination of the meaning (and sources) of political power in the last quarter of this century. Many specific conflicts of interest among nations at the same time seem to develop into explorations of ways to gain the most efficacious mix of military-strategic and economic instruments for pursuing essentially political purposes.

The West Germans, from the inception of the Federal Republic, have always been acutely sensitive to the political uses to which economic and monetary policies can be put. For although German rearmament provided the most dramatic push for Germany's rapid economic and political reconstruction in the context of the Western alliance, the successive Bonn governments fully realized that they could not translate their political purposes into military-strategic language (as did France, especially under de Gaulle) because of a prohibitive combination of political, legal, and psychological restraints stemming from Germany's defeat in World War II. By necessity as well as by choice, the Bonn republic consolidated its political position within the Atlantic alliance, and within the Common Market, by largely economic means.

The dimensions of this process that were directed toward West Germany's partners in the West have been described and analyzed rather extensively (although a definitive study remains to be written), but the dimensions of Bonn's economic policy toward the Soviet Union, Eastern Europe, and East Germany have not received such sustained and comprehensive consideration. It is for this reason that Mr. Robert W. Dean's thoughtful and balanced account deserves attention and praise. Mr. Dean, who is Senior Analyst for Poland and Czechoslovakia, East European Research and Analysis Division, Radio Free Europe, introduces the subject matter with a broad examination (in chapter 1) of the background factors and problems of East-West trade in general and West German trade with the East in particular: the reasons for the Communist states' interest in dealing with the West; the difficulties and inefficiencies that arise from the nonconvertibility of Eastern currencies and from the differing economic organization and trading practices of the participants; and the overall political, diplomatic, and legal posture of the successive West German governments toward the Warsaw Pact states. In subsequent chapters, the book deals with the politics of trade relations between Bonn and the German Democratic Republic (chapter 2), between Bonn and the Soviet Union (chapter 3), and between Bonn and the Eastern European states (chapter 4). A final chapter examines the role that the Brandt governments assigned to trade as a political instrument for furthering West Germany's rapprochement with its Eastern neighbors and the Soviet Union.

As the chapter outline indicates, Mr. Dean's study is logically organized (especially since Bonn's political stance toward East Germany, Eastern Europe, and the Soviet Union was at times highly differentiated). There can be no question about its timeliness, since several West European countries as well as Japan and the United States have been increasingly interested in trading arrangements with the Soviet Union to ease their energy supply problems and other economic or monetary difficulties. In general, Mr. Dean argues his case cogently – the central hypothesis being that "trade has been in large part a function of the political relations (or lack of them) and antagonisms that have characterized relations between West Germany and the Soviet bloc," and that "the significance of Eastern trade as perceived by the West German government has been largely political. . ." (p. xiii). The evidence for this thesis is marshaled diligently and responsibly, although there seems to be an excessive reliance on newspaper accounts while one misses among the references such "classics" as Samuel Pisar's *Coexistence and Commerce: Guidelines for Transactions Between East and West* (New York: McGraw-Hill, 1970), which is still the indispensable vademecum for Western traders with the East.

There remains a real question, however – one that is not even fully raised by Mr. Dean: to what extent can East-West trading arrangements in general, and West German economic dealings in particular, be explained as a largely politically motivated instrument intended to help implement foreign policy objectives? It is perhaps natural for governments to view trade (and monetary) relations as such an instrument, and most Western governments have been as sensitive to such opportunities as have been those of the Eastern bloc; but this does not necessarily mean that powerful economic forces (especially multinational corporations) share a corresponding sense of the primacy of politics over economics, or that they find it impossible to circumvent politically inspired governmental guidelines for external trade and monetary transactions. In that respect, Mr. Dean's study is somewhat narrowly focused, concentrating on the political role trade has played in Bonn's relations with its Communist neighbors; hence one misses an extensive consideration of the economic interests within Germany that did not always share the prevailing governmental view about when, how, or if one should trade with the East. The politics of economics have an internal as well as an external dimension.

These observations are not intended to detract seriously from the overriding merits of this study – one should not fault an author for not doing what he didn't set out to do.

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Aid Performance and Development Policies of Western Countries: Studies in U.S., U.K., E.E.C., and Dutch Programs. Edited by Bruce Dinwiddy. (New York, Washington, and London: Praeger Pubishers in association with the Overseas Development Institute, 1973. Pp. 148. $11.00.)

The Charity of Nations: The Political Economy of Foreign Aid. By David Wall. (New York: Basic Books, Inc., 1973. Pp. 192. $7.95.)

The volumes by Messrs. Dinwiddy and Wall provide different perspectives on development aid at a time when it has become increasingly difficult to support politically, particularly in the U.S. Wall purports to provide a "political economy of foreign aid" as one study in a series on "The Political Economy of International Relations" edited by Benjamin J. Cohen. The book's eight chapters deal with the definition of aid; the motives of donors in maintaining aid programs; the domestic and international economic and political implications of aid; developing nations' need for aid; the criteria for the distribution of aid. Each of these topics, and related issues, is treated with clarity and in a fashion easily digested by laymen and college students alike, the audiences to whom Wall directs his arguments.

The audience of the Dinwiddy volume, published as an Overseas Development Institute Review, is much different – aid administrators and private consultants, for example, those whose professional activities are intimately involved with the details of development policies. Five of the seven chapters were written by members of the ODI staff. These deal with recent (about 1970–71) trends in British aid and development policies in the context of Western development policies generally, with some of the possible consequences for developing nations of British entry into the EEC, and with the effect of growing population pressures on Western-oriented development strategies. A sixth chapter, by Dick van Geet of the University of Amsterdam, surveys the philosophy and practice of Dutch development assistance programs. The remaining chapter, by James Howe and Robert Hunter of the Washington-based Overseas Development Council, surveys the future of the American aid effort and development policy, particularly in light of the New Economic Policy instituted by President Nixon in August, 1971.

The Dinwiddy volume contains a wealth of detailed information on the Western aid programs; similarly, considerable detailed information is provided on the way in which the structure of various Western trade schemes affects the transfer of goods between rich and poor nations. Judged against the interests of the book's audience, such detailed analyses may well be useful. But judged against the interests of the academic social scientist in accumulating generalized knowledge, the detail of the Dinwiddy essays too often borders on case-specific information unrelated to any larger frame of reference. Central themes running through several essays are discernible, but the questions being asked are so dissimilar that the answers can hardly be evaluated in a comparative context. About the only consistent themes are: (1) that data on donors' aid efforts grossly overstate the "real aid" (grant equivalent) of bilateral aid flows; (2) that overlapping administrative jurisdictions frequently work at cross-purposes, thus obscuring an otherwise coherent development strategy; (3) that export credits and other governmentally supported private investment activities have become increasingly important facets of Western policies toward developing nations; (4) that preferential trade schemes, while potentially useful vehicles for resource transfers, are presently quite meaningless to developing countries given the protectionist practices of developed nations; and (5) that aid, trade, and development policies generally pose intensely political problems, the solutions to which are frequently generated in response to a multiplicity of domestic considerations that find developing nations themselves far down the list of relevant considerations.

The Charity of Nations demonstrates similarly the author's appreciation of the fundamental political nature of aid programs. Wall notes early (p. 38) and emphasizes frequently thereafter that "different people can support the same program for different reasons." Unfortunately, however, he appears to reject the notion that a multiplicity of political interests provides the broad base of political support necessary to sustain Western aid efforts. The central thesis of Wall's work is that aid is (should be?) the means for effecting a redistribution of world income: "*. . . the tax burden imposed on rich countries in order to sustain their aid programs involves a sacrifice in welfare which is less than the potential gain in welfare which the less-developed countries derive from such programs.* This is my political judgment. . . . [I]n order to hold to [this] political judgment . . . I have to assume that the aid programs of the rich countries are sustained by taxes on the incomes of people who are rich by international standards. I also have to assume that aid funds are used to improve the lot of the poorer sections of the world community. Insuring that these assumptions actually operate is one of the major problems of aid administration in donor countries" (p. 6).

Virtually all of Wall's analysis is grounded in this political judgment. Much of it is very persuasive (e.g., his discussions of the rising debt burden of developing nations; of how much aid developing nations need and their capacity to absorb it; and of the meaning of "development" and the role of aid in the development process). Despite the clarity of these arguments, Wall's normative political judgment does not stack up well against empirical evidence, including much of that amassed in the Dinwiddy volume and also much of the argument Wall himself presents; for example, on p. 78, Wall states: ". . . while aid programs can operate more or less independently of the state of public opinion [in donor countries] (because of their relative unimportance in the spectrum of domestic political issues), they *are not* independent of domestic battles which are totally unrelated to the economic situation of the mass of inhabitants of the Third World."

Income redistribution may be *one* of the reasons why *some* of the people support aid programs. But a combination of security, political, and economic considerations, along with humanitarian ones (including redistribution), is more likely to please more of the people more of the time. Moreover, the security, political, and economic rationales, individually or collectively, are likely to be far more potent political rationales than humanitarianism given their salience to those policy makers whose political clout is ultimately necessary to sustain foreign assistance programs. Although Wall persuasively argues that the official objectives of aid programs, such as security and development, contain contradictions and generally defy precise measurement of the contributions that aid makes to their realization, this does not detract from the political value of the rationales themselves.

If we use the goals of the social scientist as a standard of evaluation, Wall's analysis can also be criticized for his unabashed dislike of systematic, broad-based inquiry. This is apparent in several places, but especially in Wall's critical evaluation of economic growth models. The validity of development models such as Rosenstein-Rodan's "Method of Computing the Capital Inflow Requirements" of poor countries may be questioned. But labeling the model "pseudoscientific mysticism" (p. 162), largely because the data necessary for an accurate estimation of the model's parameters are lacking, is bound to irritate those who have invested substantial professional and intellectual energies in scientific methodology, its assumptions, and its presumed payoffs. More broadly, Wall strongly criticizes Rosenstein-Rodan for the value judgments implicit in his method of calculating the needs of developing nations. However, the logic by which Wall is permitted value judgments and Rosenstein-Rodan (and others) denied them is difficult to find.

Especially disappointing from this viewpoint is the lack of a coherent theory tying Wall's analysis together. We appear to be moving into an era when political-economic issues will increasingly replace political-military issues as the dominant concerns over which states compete. This will make it increasingly important for political scientists to master the often technical nature of economic interactions of the sort discussed in the Dinwiddy volume as well as the broader question of political-economic interfaces of the sort discussed by Wall. (Actually, the relative lack of attention Wall gives to trade as a "charity" mechanism is somewhat surprising. As Howe and Hunter point out in their essay on U.S. aid performance [p. 63]: "about 75 per cent of the foreign exchange available to LDCs comes from exports, compared with only 15 per cent from aid and 10 per cent from private capital.") One hopes this can be accomplished within a framework that is simultaneously parsimonious and theory-rich. If "political economy" is to provide such a framework, however, a better case will have to be made for it than can be found in *The Charity of Nations.*

Wall appears to define political economy as economic analyses supplemented by political judgments (which he would no doubt label normative judgments), and the explication of the political aspects of economic questions arising out of the use of taxpayers' money. Clearly absent is a set of logically related propositions that might be called a theory of political economy — or even a "framework" that would enable one to know what is and what is not political economy. Wall's political economy, as he clearly states, is nothing more than a set of *beliefs* (pp. 167–168) about the desirability of alleviating the social and economic distress characterizing the lives of so many peoples of the world, about the responsibility of the rich nations in making available the resources by which this goal might be enhanced, and about the necessity of restructuring the whys and ways currently used to transfer resources from rich to poor. Regardless of how persuasive one may find Wall's analyses and normative conclusions, this is hardly the basis for either a theory or a framework for the analysis of politico-economic issues. If anything, it contains the seeds of being so all-encompassing as to open itself to the same kinds of criticisms leveled at the concept power and the "power politics" framework so long used for the analysis of politico-military interactions.

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Africa and International Organization. Edited by Yassin El-Ayouty and Hugh C. Brooks. (The Hague: Martinus Nijhoff, 1974. Pp. 250. Guilders 45.)

This collection of essays on Africa and international organization emanates from a symposium held at St. John's University in 1970. The gathering included a number of distinguished African diplomats as well as American, African, and Arab scholars whose credentials are evidenced by the generally high quality of the essays. This volume does make a noteworthy contribution to our understanding of why African states have turned to collective action in attempting to solve their many political, economic, and social problems.

Its merits aside, the editors would have served the general public better had they remembered that symposia are organized to serve purposes which may be quite distinct from those one hopes to accomplish in compiling a general reader. The volume, in the first place, is somewhat repetitious with respect to certain themes which may be relatively inconsequential in the total scheme of African or world politics. At least six of the essays, for example, touch on the efforts of African states to develop machinery for compulsory arbitration and judicial settlement of political disputes.

Conversely, limiting the volume to the participants in the symposium has resulted in a number of glaring deficiencies. As one illustration, the collection would have profited immensely from an incisive "inside story" of the functioning of the African bloc in the United Nations, to complement the brief scholarly paper by Thomas Hovet, Jr. Hovet does an admirable job of describing how the African third of the UN membership has so drastically altered the manner in which the world organization proceeds, the issues it confronts, and the way African behavior affects the non-African members. Unfortunately, the more dynamic testimony which would have put flesh on Hovet's essay was not provided by two of the "working diplomats" in the symposium, Chief S. O. Adebo, the former Permanent Representative of Nigeria to the UN, and Angie E. Brooks-Randolph, the Liberian diplomat who served as President of the General Assembly during its 24th session. Surprisingly, the editors neglected to include essays which would have demonstrated the importance African leaders attach to FAO, WHO, UNESCO, and the other UN specialized agencies. It would certainly, too, have been of interest to explore the role Nigeria has been playing in OPEC (the Organization of Petroleum Exporting Countries) and the impact that organization's price manipulation has had upon development planning in every African state – including Nigeria! Indeed, the symposium was generally weak in its treatment of the Economic Commission for Africa and other UN technical assistance programs. This is one of the few areas where African states have generally encouraged non-African interest in African problem solving.

Moving to the positive side, there are several noteworthy contributions on the role of the Organization of African Unity (OAU). The first is an unimpassioned analysis by Immanuel Wallerstein of the historical development and the scope of authority and effectiveness of the OAU. It is a good critique of what regional organizations can and cannot be expected to do, given the strength of the idea of national sovereignty in independent Africa. The article by James O. C. Jonah goes even further and contrasts the OAU with the UN in the maintenance of peace and security in Africa. Jonah broadly examines the range of political, economic, legal, and military questions involved. A complementary essay by Zdenek Cervenka traverses much the same ground as Jonah, but it does more sharply focus on the role of the OAU in the peaceful settlement of disputes.

One of the primary theses of the three essays on the OAU is that the OAU since the mid-1960s has been remarkably successful in displacing the UN and other non-African or mixed groupings as the legitimate forum for the discussion and solution of African problems – particularly where the use of military force is contemplated. The handling of the Congo crisis by the UN, the return of British and French paratroopers in the mid-1960s at the request of newly independent governments beleaguered by army mutineers, and other events created real fears about the reimposition of Western colonial rule. Thus, extracontinental military intervention was to be avoided at all cost.

In two areas, however, African states in the United Nations have welcomed non-African assistance. The first takes the form of keeping up the drum-beat against the remaining vestiges of white supremacy in Africa. The African bloc in the UN has been very skillful, as Yassin El-Ayouty points out in his essay on "Legitimization of National Liberation," in getting the rest of the world community to bring constant pressure against the Portuguese, the Afrikaners, and the white Rhodesians. Support, both direct and indirect to liberation movements, as well as political and legal harassment of the white regimes by the UN and its agencies have been helpful in undermining the Portuguese and in compelling South Africa to alter its public image.

Where the African bloc has attempted to get the non-African states to give more explicit evidence of their commitment to effective nonviolent liberation, the balance sheet is somewhat muddied. I refer to the efforts of the African bloc to get the members of the United Nations to employ economic sanctions against a recalcitrant member of the world community. The essay by Leonard Tawayena Kapungu on the Rhodesian crisis is an excellent analysis of the limitations involved in implementing such a course of action. Perhaps the essay could have been strengthened, however, by reference to the other major attempt by a world body to apply economic sanctions in the solution of an African problem, namely, the futile efforts of the League of Nations to halt the Italian invasion of Ethiopia in 1936.

In addition to analyzing the UN and OAU, the symposium dealt with subregional international organizations in an essay by Boutros Boutros-Ghali on the Arab League and an article by Yash Tandon and Ali Mazrui on the East African Community. Although Boutros-Ghali perhaps exaggerates the effectiveness of the Arab League in liberating African colonies and in integrating Arab states, he does note the curious way in which the competing organization, the OAU, has on occasion been more effective than the Arab League in the discussion of situations involving states in the Arab Maghreb. The authors of the essay on the East African community do an excellent job of analyzing why the concept of East African closer association has followed – to paraphrase Lenin – the path of "one step forward, two steps backward" since independence. The development of national rather than regional economic plans, and the diverse ideological stances taken by Kenya, Tanzania, and Uganda have led to the abandonment of the interstate cooperation in a wide range of fields. The competitive advantage enjoyed by Kenya clearly leaves Tanzania and Uganda behind in any efforts at economic cooperation. It is a sobering piece which dramatizes the point made by Thomas Franck in the lead essay of the symposium. This is the realization that only extraordinary acts of political self-denial on the part of African leaders will alter the remarkably persistent political boundaries left behind by the European colonial powers.

Although one of the essays does not actually address itself to the main theme of African involvement in international organization, it is nevertheless worthy of attention: I refer to the essay by L. Gray Cowan on "Education and National Integration," which sets forth very cogently why education stands so high on the priority list of every African state. Also somewhat out-of-place is the article by Abdulrahim Farah on South Africa's apartheid policy. Although the author gives an adequate – albeit sometimes emotional – analysis of the essential elements in the system of apartheid, Farah should perhaps have concentrated instead on the intriguing story of this most persistent of General Assembly agenda items and its implications for the handling of other so-called "domestic" questions within the UN.

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The Struggle for Cyprus. By Charles Foley and W. I. Scobie. (Stanford, California: Hoover Institution Press, 1975. Pp. vii, 193. $7.95.)

The authors, two journalists intimately familiar with the events in Cyprus during the period of the Grivas-led EOKA movement against British colonial rule from 1955 until independence in 1960, present in *The Struggle for Cyprus* an interesting account filled with anecdotes involving some of the major participants in the struggle, particularly the leader of EOKA. The incidents described provide a fascinating study of the role of accident, tactics, and strategy of one particular guerrilla movement interacting with its enemy, the British colonial forces.

The book, an historical narrative, focuses primarily on the terrorist activities of EOKA; this severely limits the book's usefulness since EOKA is presented in a vacuum. No effort is made to discuss the context within which EOKA operated, either in Cyprus or the international scene. Nor is EOKA's ideological stance other than its goal of *enosis* – union with Greece – discussed. Colonel Grivas was a reactionary whose targets were not only the British but Greek traitors who for him included all leftists. EOKA represented a retrogressive nationalism, opposed to the secularism and modernization of British rule. Its goal was a return to the *status quo ante* in which the Church and traditional values prevailed. The commercial elite, the Communist Party, and the labor organizations were grudging supporters of EOKA.

The authors, however, do discuss Grivas's relations with Archbishop Makarios, whom the British considered the leader of the broader *enosis* movement and whom they exiled in 1956. Recently, it has become widely known that when Grivas revived EOKA in 1971, supported by the Greek dictatorship at the time, its specific objective was the overthrow of Makarios. Foley and Scobie make clear the less well-known fact that conflict and tension between the two men persisted throughout the struggle against the British. While often agreeing on goals, Grivas and Makarios differed significantly on strategy and tactics. Although the book is based largely on Grivas's perspective, the authors provide insight into the conflict between Grivas and Makarios, as Makarios accepted an independent Cyprus in 1960.

Except for one brief chapter, "Civil War," which discusses the intercommunal conflict during June, 1958 when negotiations on Cyprus were in progress (Turkey and the Turkish Cypriot leadership demanded partition of the island) the authors ignore the Turkish Cypriots. TMT, the Turkish Cypriot equivalent to EOKA, is mentioned only once. Although the Turkish Cypriots and TMT are not the focus of this study, this omission distorts the context of EOKA's operation. Both EOKA and TMT terrorized their own communities as well as each other. In addition, their relationships to Greece and Turkey raise questions regarding the economic and security interests of the U.K. and the U.S. Discussion of the policies of the U.K., the U.S., Greece, and Turkey are beyond the book's scope; but for that very reason, its usefulness is limited to those who are already familiar with Cyprus and are concerned with a detailed discussion of the activities and personnel of EOKA in the 1950s. The book contains references to Nicos Sampson and to other EOKA figures who led assassination squads directed at British officers as well as Greek leftists, and therefore provides interesting indirect insights into the evaluation of terrorists. Sampson subsequently engineered the coup and the assassination attempt against Makarios in July 1974 and was briefly "President" of Cyprus.

One other theme of the book is EOKA's vacillation between urban terrorists and mountain guerrillas; Makarios favored the former, Grivas, the latter. EOKA originated in towns and remained viable there, whereas the mountain guerrillas were eliminated by the British in 1957. Implicit throughout, but never extensively analyzed is the relationship between a guerrilla movement and political actors. Grivas felt, as the authors make clear, that he was undermined and destroyed not on the battlefield, but by the political agreements which led to an independent Cyprus.

The authors carry their account through to 1974. However, the period after the granting of independence in 1960, including an epilogue on the coup against Makarios in July, 1974, is summarily handled. In contrast to the treatment of the earlier period, this part is a superficial overview, reflecting a less intimate familiarity with events.

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American Policy Toward Laos. By Martin E. Goldstein. (Cranbury, N.J.: Fairleigh Dickinson University Press, 1973. Pp. 347. $15.00.)

Despite its title, and there is no subtitle, *American Policy Toward Laos* covers in detail only the period from mid-1954 through mid-1962. The preceding and succeeding eras, as well as Laotian society, are treated in a perfunctory and idiosyncratic manner. But even within the narrow time span chosen, the book is far from satisfactory.

Professor Goldstein is counted among the "good guys." He opposes colonialism; regrets the deaths and uprootings of war; prefers a decent, non-Communist regime to either Communist authoritarianism or corrupt, rightist military and aristocratic incompetence; looks askance at errors and waste of American aid; and deplores the flow of drugs which, under the protection of U.S. guns and aircraft, has run from Southeast Asia to the streets of America. But laudable values alone do not justify a purportedly academic study.

I believe that Dr. Goldstein is attempting to delineate how U.S. policy toward Laos was determined by American ideological/strategic thinking, by events in Laos and the remainder of Southeast Asia, and by the believed intentions and actions of both America's allies and her enemies (especially the Soviet Union, China, and the DRV). Also, obversely, to ascertain how much events in Laos were affected by American policies.

What emerges is a chronology of Laotian events and of U.S. policy declarations and interventions, with guesstimates about their interrelationship. Events are given in detail, but the author has had no first-hand experience with Laos, the Laotians, or any of the other actors in the drama. Professor Goldstein appears ignorant of several important on-the-scene accounts of what was happening in both Washington and Laos.

The author derives his understanding of actual American policy largely from official statements or the memoirs of officials. He avoids an in-depth discussion of the processes of American policy formulation, although he is aware that at crucial times there was disagreement within and between the many organs of government. While the Soviet Union, China, and the DRV are unarguably important in Washington's calculations, the first receives mention based on few and shallow sources, while the last two remain spectral. Little explored are either the contemporary development of the war in South Vietnam (other than for four pages pertaining to the post-1962 period) or considerations of Thai security. And judgments are generally hedged into meaninglessness: "With the exception of a very few dishonest officials, all Americans involved in Laos acted with the interests of the United States in mind. If we can fault them for anything, it may be that they acted not wisely but too well" (p. 281).

Generalizable hypotheses are not Goldstein's forte, although he does unwisely devote one brief chapter to them. There, for example, in a search for conditions under which compromise can be reached in a combative political situation, he borrows approvingly from another scholar the conclusion that one preliminary condition must be the existence of communication between the parties. And as an avowedly generalizable and testable hypothesis, he offers the following: "An agreement, by major powers that sustain conflicting factions in an internal war, to seek a compromise settlement contributes significantly to the likelihood of such a settlement" (p. 286).

The writing, in quest of profundity and clarity, is replete with sophomorisms and aggravating and often misleading similes and metaphors. About the course of history: "In the topography of history, there are no canyons, only slopes. Events issue from one another and give rise to yet newer occurrences" (p. 20). Or, about Laotian society: "If one were to raise a pane of glass over one's head and smash it on the floor, the scattered distribution of glass splinters would bear a striking resemblance to the social structure in Laos" (p. 27). And so on.

The bibliography has great gaps, even if one accepts the dubious proposition that works in English are adequate for the study.

Experts in Indochinese affairs or American foreign policy will find little of interest in *American Policy Toward Laos,* except, perhaps, for its lengthy precis of Congressional reports on aspects of the American aid program, and its statistics on the amounts and categories of aid. The interested nonexperts will discover far better points of entry in Bernard Fall's several works, only some of which are cited, and in such uncited books as Hugh Toye's *Laos: Buffer State or Battleground* (London: Oxford University Press, 1968), Adams and McCoy's *Laos: War and Revolution* (New York: Harper and Row, 1970), and Marek Thee's *Notes of a Witness: Laos and the Second Indochinese War* (New York: Vintage Books, 1973).

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The United Nations in a Changing World. By Leland M. Goodrich. (New York: Columbia University Press, 1974. Pp. 280. $12.95.)

This is a useful survey of the organization and activities of the United Nations written by a noted authority in the field. Professor Goodrich has devoted a lifetime to the study of the United Nations and its predecessor, the League of Nations. He has written several books on the subject; the best known is his commentary on the Charter of the United Nations, written in collaboration with Edvard Hambro and Anne Simons. The purpose of the present work, as the author puts it, is "to look at the Organization in its historical context; to recall how it was originally conceived; to examine the changes that have taken place as the result of the changing world environment, the expanded membership of the Organization, the attitudes of its members and the demands made upon it; and to consider what its likely prospects are for the years immediately ahead" (p. vii). After introductory chapters in which he compares the League of Nations and the United Nations and in which he treats the UN as an "evolving political system," he deals subsequently with problems of membership and representation, the structure of power and influence, the role of the Secretary-General and his staff, the maintenance of peace and security, peace-keeping operations the protection of human rights, problems of colonialism, the role of the UN in the social and economic field, and the demands of modern technology. A final chapter is devoted to prospective developments of the United Nations.

Professor Goodrich is at his best when he describes the UN as it actually works, drawing upon his considerable knowledge and experience. Somewhat disappointing to the professional political scientist are his references to topics with a more theoretical relevance, such as "influence" or "functionalism," which remain unsystematic, vague, and inconclusive. Repeatedly, he refers to the "influence" which the permanent members of the Security Council can exercise (pp. 61, 65, 77) without ever making clear what this influence consists of or how it can be observed. Similarly, he refers to the use that can be made of "functionalist theory" (p. viii) or the work of scholars on the process of political integration (p. 255), but he does not apply those theoretical concepts to his subject.

Professor Goodrich on the whole evaluates the record up to the present rather positively. Indeed, some of his pronouncements may strike his readers as somewhat naive. Thus one would like to know why he has "reason to believe that members of the United Nations have been substantially influenced in their policies and actions by the purposes and principles set forth in the Charter and by the interpretations given them by the organs of the United Nations" (p. 29). The same applies to the "common values" which are allegedly adhered to by all members of the UN. Maybe so. But I would hardly consider it significant evidence in this context that "even the opposing parties in the cold war professed fervently their commitment to peace, protested that they had no aggressive intentions, and justified their armaments solely on the ground of self-defense" (p. 28).

The author is realistic in assessing the role of the major powers which he considers essential in the further development of the United Nations. He particularly emphasizes the role of the United States and notes with regret that the Nixon administration has paid less attention to the United Nations than its predecessors have. This may be because, as the membership of the United Nations has increased, the United States has found it more and more difficult – and often impossible – to have its views prevail as they had during the first ten years of the existence of the organization. This is probably a more crucial reason for the waning enthusiasm of the United States than the causes listed by Professor Goodrich on p. 262: the financial crisis, disillusionment over peace-keeping, dominance of the General Assembly by members whose votes outrun their contributions, and the adoption of resolutions without thought to their implementation. Yet, given these problems, it is not clear how Professor Goodrich can conclude his book by urging the U.S. government, "in spite of its disappointments, disapprovals and occasional defeats" to make clear its commitment to the UN, and to provide "strong responsible leadership" in the organization (pp. 271–272). The author fails to make clear why the "true interests" of the United States would in the long run best be served by such a policy.

The book is written from a pro-UN perspective. One finds this occasionally reflected in the analysis; for instance, the author describes the choice of Kurt Waldheim as Secretary-General rather positively (p. 88) without acknowledging that Waldheim was chosen partly because there was little danger that he would develop into a "strong" Hammarskjöld-like figure. On the whole, however, Professor Goodrich is sober in his evaluations, and he cannot be accused of too much optimism about future developments. The book is both a useful introduction for beginning students and a handy survey for the nonspecialist.

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The Political Economy of Agrarian Change. By Keith Griffin. (Cambridge, Mass.: Harvard University Press, 1974. Pp. xv, 264. $11.00.)

This book represents an elaboration and extension of the author's earlier monograph for the United Nations Research Institute for Social Development (UNRISD). See Keith Griffin, *The Green Revolution: An Economic Analysis* [Geneva: United Nations Research Institute for Social Development, 1972]). The author's purpose is to evaluate the economic, social and political implications of the "green revolution" – of the introduction of new, high yielding varieties of rice, wheat and other cereal crops in the rural areas of the LDC's, particularly in Asia.

The book is dominated by two contradictory theses. The *first* is that data on production trends are interpreted to imply that there has been no "green revolution" in cereal production in the developing countries of the tropics. Griffin argues that the most that can be claimed is that the introduction of high yielding varieties have enabled some countries to offset the effects of rapid population increase to maintain existing levels of per capita food production and nutrition.

The *second* is that the green revolution has had severely regressive effects on social order and economic relationships in rural areas. According to Griffin: "The new technology has led to changes in crop pattern and methods of production. It has accelerated the development of a market oriented, capitalist agriculture. It has hastened the demise of subsistence oriented, peasant farming. It has encouraged the growth of wage labor, and thereby helped to create or augment a class of agricultural laborers. It has increased the power of landowners, especially the larger ones, and this in turn has been associated with greater polarization of classes and intensified conflict" (p. xiii).

The major weakness of the book stems from the author's attempt to maintain these two theses. He never really faces up to the problem of how the secondary impacts of a barely perceptible "green revolution" could be so severe. The major strength of Griffin's study stems from the use of a carefully articulated model of dialectical interaction between technical and institutional change as the organizing theme for the process of agrarian change. Economic and technical changes are viewed as subversive of traditional institutions in peasant societies. He concedes that the new high yielding varieties are themselves technically neutral with respect to scale. The concentration of economic and political resources in rural areas associated with technical changes is attributed to the effects of neutral technical change in an environment characterized by nonneutral institutions. Bias in the performance of rural institutions directs the new income streams generated by technical change into the hands of the local elites. The effect is to further enhance the resources available to the rural elite to influence economic policy.

There is no question that terms such as the "green revolution" and "miracle rice" were employed by publicists and by the agricultural science lobby in many countries to imply more dramatic rates of change than has been revealed by the trends in agricultural production. Yet in Gapan, Nueva Ecija (Philippines), where the new high yielding rice varieties developed at the International Rice Research Institute (IRRI) were first introduced in 1966, sixty per cent of the area was planted to high yielding varieties three years later. This was a more rapid rate of adoption than achieved by Iowa farmers following the introduction of hybrid corn. Nor can there be any doubt that the social returns to the investment in the international crops institutes that developed the new rice and wheat varieties has been high by any standard. Dana Dalrymple has estimated that by 1972–75 the new wheat and rice varieties were adding an annual increment of more than 16 million metric tons to wheat and rice production in non-communist Asia. (Dana C. Dalrymple, "Development and Spread of High-Yielding Varieties of Wheat and Rice in the Less Developed Nations," [Washington: Economic Research Service, U.S. Department of Agriculture, FAER 95, 1974].)

There is evidence to support Griffin's contention of rising tension in rural areas. My own review of much of the same literature that he draws on leads me to place much greater emphasis on the history of political and economic organization and the effects of rising pressure of population against limited land resources as a source of this rising tension. Indeed, in those areas where the progress of the green revolution has been most rapid the evidence indicates that all classes have typically shared, though unequally, in the new income streams. In a bitter commentary on Griffin's use of Philippine evidence, Gelia Castillo presents data from the Philippines to show that the introduction of the new varieties has resulted in greater income inequality but higher income for most farmers. Before the new varieties were introduced there was greater equality but lower income for most farmers. (See Gelia T. Castillo, *All In a Grain of Rice* [College, Laguna: Southeast Asian Regional Center for Graduate Study and Research in Agriculture, 1974].)

The challenge which Castillo (*Ibid.*, p. 220) offers to Griffin – "Does the pursuit of equitable income distribution . . . lie in keeping farmers equally unproductive and poor?" – will have to be faced with increasing intensity by LDC governments and by technical and financial assistance agencies during the next decade. Griffin's answer is that technocratic and reformist strategies of rural development are inherently biased against the smaller peasants and landless laborers. In his view only a radical strategy, buttressed by socialist ideology will be capable of mobilizing the human and physical resources in rural areas to improve both productivity growth and equity in the partitioning of the income streams that become available as a result of technical change.

Although he does not attempt to specify the details of an appropriate Socialist model of rural development Griffin does suggest that elements of a solution can be found in the experience of both the Peoples Republic of China and the Republic of China (Taiwan). This implies that social scientists should begin to bring significantly more rigor to the analysis of problems of institutional transfer and innovation that Griffin has brought to the issue of technological transfer and innovation.

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Basic Documents of Asian Regional Organizations. 4 Volumes. Edited by Michael Haas. (Dobbs Ferry, N.Y.: Oceana Publications, 1974. Pp. 1544. $140, set.)

Asia is far behind Europe, Latin America, and Africa in regional integration. The colonial legacy, a host of bilateral disputes, intervention by the superpowers, varying sizes and uneven stages of economic growth among Asian countries – all these inhibit far-reaching integration in this continent. Yet Asia has seen a steady proliferation of intergovernmental organizations committed to promote regional cooperation in economic, social, cultural, political and military fields. Very little is known about these organizations even in the countries that are members of some of them. I can testify from personal experience that it is extremely frustrating to collect documentation on these organizations because most Asian governments are reluctant to give researchers mimeographic copies of relevant documents. At times we are told that a document exists, but it cannot be traced. Professor Michael Haas and the Oceana Publications must be congratulated for producing a set of four volumes, covering 1514 pages of useful documentation on non-UN intergovernmental organizations.

Patterned after Louis B. Sohn's *Basic Documents of African Regional Organizations*, this compendium is an annotated documentary survey of some two dozen intergovernmental organizations ranging from highly controversial military pacts to vegetable research centers. A rough idea of the content may be obtained by glancing through the twelve chapter headings organized more or less chronologically: The

Colombo Plan, Military Organizations (SEATO and The Five Power Defence Arrangement), Asian-African Organizations (Asian-African Legal Consultative Committee, and the Afro-Asian Rural Reconstruction Organization), Asian Productivity Organization, Asian-Oceanic Postal Union, Asian Parliamentarians Union, Asian and Pacific Council, Asian Development Bank and Related Organizations, Association of Southeast Asian Nations, Organizations of The Ministerial Conference for the Economic Development of Southeast Asia, Educational Organizations, and Commodity Producers Organizations (Asian Coconut Community, etc.). The documents in each chapter are preceded by a brief note on the origin, organization and evolution of organizations covered. The value of the compendium is enhanced by the inclusion of a selected bibliography in each chapter.

We may not agree with the editor's optimism about removing misunderstandings about these organizations or view these as "potential building blocks toward a united Asia," but there is no doubt that the compendium under review is the first comprehensive reference work on Asian regional organizations, and, perhaps, will remain so for a long time. Students of Asian affairs, journalists, and diplomats will find these volumes a source of handy reference material as well as of brief introduction on nearly all non-UN agencies. The usefulness of this set could have been enhanced if the editor had offered a bit more annotation on the context and diplomatic background of some of the Declarations and communiques of organizations such as SEATO, ASEAN, and ASPAC. But perhaps this is expecting too much from a work of this kind.

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International Systems: A Behavioral Approach. Edited by Michael Haas. (New York: Chandler, 1974. Pp. xii, 433. $14.00.)

After Singer's *Quantitative International Politics* (New York: Free Press, 1968) and Russett's *Peace, War, and Numbers* (Beverly Hills: Sage, 1972) this book is the third to deal explicitly and extensively with the behavioral and scientific approach to the study of international politics. Yet its aim is different from that of its predecessors. While Singer and Russett presented an overview of the results of empirical research in international relations, Michael Haas's intention is rather to provide a synthesis of theoretical propositions based upon findings of three decades of behavioral research in the field. Continuing the explorations in the territory of quantitative international politics, Haas draws the first "map" of the new discipline. In particular he wants to demonstrate that the use of scientific methods has indeed produced substantive knowledge about international relations; that the behavioral approach provides both a solid base for future research and some answers to policy relevant questions. Haas is hence less concerned than Singer and Russett with problems of measurement and data analysis. In fact, methodological issues are only slightly touched upon in Haas's introduction on scope and methods of international relations and practically absent from the other contributions. Rather, the emphasis is clearly placed on theoretical problems, and Haas's final chapter constitutes a plea for development of new theories of international politics.

Haas's intention to survey the kind of substantive propositions that quantitative international politics has already produced is, I think, certainly sound. Some might argue that such an effort is premature given the youthfulness of the discipline. An inventory of propositions, however, is useful both for the scholar wanting to build his research on past efforts and to the student interested in evaluating the achievements of behavioral perspectives in international politics. Haas's collection of articles is potentially a valuable reference and textbook.

Is Haas's ambitious program realized in this book? *International Systems* presents a whole array of interesting subject areas, some of which have only recently been brought to the attention of the behavioral researcher. This is particularly the case for the problems of foreign policy calculation (chapter 3 by Lloyd Jensen), international nonalignment (chapter 5 by Nazli Choucri), international statecraft (chapter 6 by Werner Levi), international law (chapter 10 by Irvin White), and international administration (chapter 11 by Haas). Jensen tries to assess how nations evaluate their moves at the international level and what determines these evaluations. He stresses the importance of power maximization and illustrates it by studying the motives behind ratifications or nonratifications of the nuclear nonproliferation treaty. In chapter 5 Choucri selects several nonaligned countries to present the attitudinal and perceptual components of nonalignment, emphasizing the influence of the cold war. Her conclusions are based upon content analysis of speeches by these nations' leaders as well as on investigations of the countries' voting behavior in the

United Nations and their trade patterns. In chapter 6 Levi examines the principal tools of statecraft in foreign policy which, in his eyes, are diplomacy, propaganda, and pressure. He points out relationships among these three and considers the increasing weight of propaganda and pressure. In chapter 10 White assesses the impact of the international legel system, especially the International Court of Justice. Haas, in chapter 11, deals with the questions of implementing policies and decisions of international organizations, pointing out bureaucratic problems arising from inefficiency, infighting, and poor relations with the recipients of the programs.

In addition to these newer areas of research, more traditional subjects appear: socialization in international relations both by individuals and countries (chapter 2 by Michael Haas), alliances with an emphasis on the theory of public goods (chapter 4 by John D. Sullivan), and communication and integration (chapters 7 by W. Philips and 8 by Haas). International decision making (chapter 9 by Louis Kriesberg), deterrence (chapter 12 by John Raser), and conflict resolution (chapter 13 by Haas) also represent classic research topics.

One important area is absent, namely conflict and war studies. These are only occasionally treated within the chapters on socialization, alliances, deterrence, and conflict resolution. Omission of an explicit contribution on conflict and war is all the more regrettable because this aspect of international relations has the longest research tradition as well as one of the richest theoretical contents. Indeed, conflict research, particularly arms race models, has provided international relations with some of its more sophisticated formulations. Without mention of this important subfield, has Haas fully achieved his goal of presenting the substantive results of behavioral research in international politics?

Neglect of war and conflict analysis is symptomatic of a general weakness of the book. A basic characteristic of wars and conflicts is their longitudinal aspect. Very well emphasized by some of the contributions in Russett's *Peace, War, and Numbers*, the dynamic dimension is often lacking in *International Systems*. Rather, the results of behavioral research are presented here as bivariate or multivariate propositions in the static if-then mode. But the relationships elucidated by these statements are rarely integrated into coherent dynamic schemes.

The conception that different behavior characterizes different moments in the evolution of a process is certainly present in Haas's article on socialization and in the contributions by Jensen, Choucri, and Sullivan – and these four are perhaps the best chapters in the book. But somehow the connections between these developmental stages is missing and no conceptualization of the dynamics of international behavior appears. For instance, the momentum gathered by an agreement like the nonproliferation treaty is in itself an important element of foreign policy calculations: some states will join only when many other states have already ratified. Had Hensen considered these kinds of feedback mechanisms, his article would have gained in structure and clarity. Similarly, in Choucri's contribution on nonalignment, one would have liked to see evaluatons of the effect of the disappearance of cold war issues on the international system. Didn't the recent energy crisis show that the nonaligned bloc gained some momentum? Raser's article on deterrence also lacks a dynamic perspective. Raser does not provide any clear representation of the mechanisms by which the nuclear balance of terror evolves over time. He does not discuss the problem of its stability in relation to technology. Also, linkages between deterrence theory and behavioral models of arms-races like Richardson's are missing.

These weaknesses lead to a relative absence of rigor in the elaboration of the theoretical constructions surrounding the propositions presented in *International Systems*. Its value as a textbook is therefore reduced because familiarity with the issues is necessary to understand and evaluate the various arguments and propositions. This lack of clarity is reflected paradoxically in the last chapter by Haas. In my eyes, a theory is a formally consistent body of propositions, rich in substantive content, and largely empirically verified. What Haas calls theories are to me no more than broad frameworks without specific substantive content or with one so general that it becomes trivial. This is especially true for what he calls genetic, field, and equilibrium theory.

Despite these last harsh criticisms, I think that *International Systems* has some inherent virtues in presenting the scholar and the student with a broad overview of the knowledge accumulated by the behavioral approach. Its coverage of relatively new domains is particularly valuable. I have suggested some areas that could be improved upon, realizing full well that *la critique est aisée, l'art est difficile.*

URS LUTERBACHER

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The United States and Western Europe: Political, Economic and Strategic Perspectives. Edited by Wolfram F. Hanrieder. (Cambridge, Mass.: Winthrop Publishers, 1974. Pp. 311. $8.95, cloth; $5.95, paper.)

Each contributor to this thoughtful volume believes it essential that the grand alliance between the United States and Western Europe be maintained in the face of the continuing Soviet threat.

Hans J. Morgenthau makes the point most clearly in the introductory chapter: "the Soviet Union is engaged in a political campaign to diminish and if possible, to expel the American presence in Europe" to strengthen its position (p. 4). "Only the American presence has prevented" the Red Army from dominating Europe. Moscow, he says, seeks to emasculate NATO and to isolate and eventually co-opt the Federal Republic of Germany. "The main threat to our interests," says Morgenthau, "lies in the risk that Western Europe may be absorbed, nation by nation, into the Soviet orbit" (p. 7).

Professor Morgenthau also warns against overestimating the strength and cohesion of Western Europe. Since Western Europe is "far from being a political or military entity" comparable to the United States, the Soviet Union, China, and Japan, and since it is "an economic entity only in a limited sense," he says, we still live in an essentially bipolar world (p. 6).

Some of the other contributors do not concur with Morgenthau's geopolitical analysis, but agree on the significance of NATO and insist that the alliance needs bolstering.

Professor Hanrieder sums up three themes that run through most of the thirteen essays: (1) In U.S.-European politics there has been a shift "away from the primacy of military-strategic elements of power toward the primacy of economic elements"; (2) there is a "growing interdependence among traditional actors of world politics," especially "among highly industrialized countries"; (3) ". . . a new convergence of international and domestic political processes is underway, with consequences that are most likely irreversable but are neither fully understood nor perhaps fully acceptable" (p. viii).

The first theme persists through the collection, though with its overtones of economic determinism it is hardly held as an article of faith by all contributors. At least Morgenthau still believes that power and interest are the prime determinants of interstate politics. The second theme is widely accepted as self-evident. The view that "foreign policy is an extension of domestic policy" is fashionable, but it tends to underplay the fundamental security components of interstate politics.

Kenneth N. Waltz says the United States "should continue to guarantee Western Europe's security, not only through the deterrent forces that we maintain for our own sake, but also through the earnest of our interest in Europe provided by the presence of our troops," but he questions the necessity of having "more than a thin trip wire force" (p. 36).

Taking exception to Morgenthau, Arnulf Baring maintains that though we may "long for the days of the frozen bipolarity" of the Cold War, only an "optimistic observer can call the model of the world we are heading for polycentric. It is, in fact, chaotic" (p. 55).

Calling for a fresh recognition of "the continuing centrality of the nation-state in modern political life," David P. Calleo insists that we "Americans can hardly expect to escape the burdens of imperialism until we can learn to accept the legitimacy of other nation-states" (p. 78).

Several contributors address directly the interplay between U.S. economic and security policies toward Western Europe. Since the essays were written in the first half of 1973, they reflect the tensions between Washington and the Common Market as well as the balance-of-payments problems that assailed the relationship during what the administration ironically chose to call "the Year of Europe." Benjamin J. Cohen expresses the mood of several contributors when he advises Washington to demonstrate that it is "prepared to adjust to the new" economic realities in Europe, adding, "the old bargain has come unstuck. A new one, signifying real equivalence, is needed" (p. 133). William Diebold, Jr., David A. Walker, and Edward L. Morse also address different facets of the economic issue.

Several authors focus on ways of strengthening the Atlantic alliance. Paul Y. Hammond of RAND notes that desirable changes in NATO are unlikely to occur "without major initiatives from its more powerful members" and that U.S. leverage had been eroded by a Watergate-weakened President Nixon.

Robert E. Hunter, an adviser to Senator Edward M. Kennedy, calls for Washington to express greater concern for the "non-military aspects of the Alliance," to engage in "common efforts to avoid a trade war," and to recognize "that Atlantic 'security' in the economic sense must involve other nations, including Japan –

and when it comes to energy issues, even oil-producing states" (p. 250).

The complexities of "mutual balanced force reductions" in Europe are examined by John N. Yochelson. Robert Gerald Livingston's concluding essay ends on a hopeful note: "The collective transatlantic political will is still strong" because it "rests on shared values and on a long and often glorious history of cooperation" (p. 292).

This collection is frequently repetitive and tends to overemphasize the economic elements at the expense of the gut political forces of power, interest, and ambition. Largely absent are references to the internal political weaknesses of our NATO partners and to Soviet penetration into Western Europe. Unless I missed it, there was no mention of KGB activity in Britain and Portugal; the extent of Communist influence in strikes in Britain, France, and Italy; Communist penetration in the French Armed Forces, or other subversive Soviet activity in Italy, Greece, and Portugal. These are serious omissions in any book dealing with the state of the Atlantic alliance.

ERNEST W. LEFEVER

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Defeat of an Ideal: A Study of the Self-Destruction of the United Nations. By Shirley Hazzard. (Boston, Mass.: Little Brown and Company, 1973. Pp. 286. $8.50.)

This is a scathing and sometimes bitter analysis of United Nations administration by a disenchanted former member of it. Miss Hazzard was understandably shocked by Trygve Lie's weakness in resisting pressures from both the State Department and the F.B.I. to screen American nationals in the Secretariat during the McCarthy anti-Communist "witch hunt" of the early 1950s. She evidently had a great deal of admiration for Dag Hammarskjold's political acumen and dedication, but regrets his rather distant handling of staff relations. U Thant she associates, quite wrongly in my view, with a negativism which became "an actual source of pride in the upper echelons of the Organization" (p. 196).

Miss Hazzard's strictures on recruitment policy are well founded. Successive reports of the Federation of International Civil Servants Associations have pointed out that these policies are subject to pressure from delegations to accommodate their own nominees; that they tend to discrimination against youth and against women; and that they generally exhibit an inattention to candidates' levels of training, responsibility, and experience which makes nonsense of the Charter's requirement that the paramount consideration should be "the necessity of securing the highest standards of efficiency, competence and integrity." Only too often unbridled nepotism seems to be the guiding factor in recruitment practices.

The author is rightly critical of the continued weakness of in-service training, and she notes approvingly those passages in the report by Sir Robert Jackson on the United Nations development system which deplore the lack of any central guiding "brain" which could both monitor and exercise effective control over the system. These failings lead Miss Hazzard to thoughts not of "reform" of the existing apparatus, but of its "demolition and reconstruction in some refreshed, independent, and uncorrupted form" (p. 111).

It is a telling indictment, but there are signs of a political naivete which does not make for confidence in the author's judgment. She seems wholly unaware of the political restrictions under which the Secretariat, both collectively and individually, must operate, and she often seems to envisage their role as makers of policy rather than as advisers and executants. She also has a quite unsubstantiated trust in the beneficent influence of public opinion, "looking to an active body of world opinion" to assert itself to purge the United Nations of its present ills. Nor does she offer any evidence for her belief that any successor body to the United Nations would be any less prone to the defects she has revealed. Nevertheless, so many of the criticisms she levels at the internal administration of the organization appear to be valid that it would be imprudent to dismiss her book as little more than the vaporings of a disgruntled ex-member.

GEOFFREY GOODWIN

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Micronesia at the Crossroads: A Reappraisal of the Micronesian Political Dilemma. By Carl Heine. (Honolulu: The University Press of Hawaii, 1974. Pp. 210. $10.00, cloth; $2.95, paper.)

This little book is recommended to those interested in the developing microstates or to those specifically following the fate of the Pacific islands still held in trust by the United States for this is the first book-length treatment of Micronesian political problems written by a Micronesian. Further, Carl Heine's well-reasoned prescription for his homeland stands in contrast to that of many other indigenous leaders in developing nations.

The author, born into a prominent Micronesian family, received most of his education in the United States. He has held several posts in the Trust Territory government, including deputy director of one of the administrative districts. More recently, he has served as staff director for the Joint Committee on the Future Status Commission of the Congress of Micronesia.

Much of Heine's book corresponds to other books of developing nations – relating the colonial experience, identifying the agents of change, discussing the inevitable clash between the traditionalists and modernists, and analyzing and evaluating the political alternatives. The crossroads dilemma of the title is the problem of choosing a posttrustee status, a momentous undertaking for the Micronesians, a people whose territory has had a succession of colonial rulers since it was first discovered by the Western world in the sixteenth century.

Since 1970, in meeting its obligation under the trust agreement terms which require the United States to prepare the inhabitants for self-government or independence, depending upon the wishes of the people concerned, American officials have been negotiating with Micronesians over a final status which will satisfy as many of the diverse interests represented as possible. Heine points out the many cross purposes at work in this quest for an agreement between the Trust Territory and the American government: the nationalistic mood, with its proclivity for complete independence, has captured some islanders; the desire for the goods of modern technology, as achieved by the neighboring Guamanians, cause others to want a similar close association with the Americans; centuries of insular isolation make others distrustful of any binding alliances, even alliances among the people of the different islands which make up Micronesia; and, finally, there is the urgent felt need of the American military to insure a base for its Pacific operations.

Of the three major status alternatives – independence, free association with a major power, or complete integration into another political system – Heine believes the only realistic course for the Territory is to enter into a state of free association with the United States, an arrangement wherein either could "disassociate" upon notice, United States would manage Micronesia's foreign relations but Micronesia would have sovereign power over her internal affairs and would be assured of the sine qua non if she is to develop, that is, major financial assistance. Heine warns that the longer the islanders loiter at the crossroads of status choice, the more limited is their choice because recent increasing U.S. economic aid brings seemingly irreversible dependency; moreover, he cautions, the tenuous bonds of unity among the various island groups will dissipate as the constraints of an outside authority loosen.

The distinctiveness of the book's content lies (1) in the author's eschewing the easy path of placing all onus for the poor condition of his fellow Micronesians upon the colonial administrators, (2) in his sharp criticism of nationalism, and (3) in his disavowing independence as the best present goal. He does not excuse the colonial rulers from all blame, but he does charge that Micronesians have suffered a great deal at their own hands, tolerating social injustices within their traditional island societies. The real enemies he identifies as poverty, disease, illiteracy, and political inexperience which does not yet allow them to compromise in order to achieve their political goals. He sees nationalism as a dangerous phenomenon because it may perpetuate the harmful attitudes and practices of traditional society and generate internal as well as external hatreds. He argues that if the people choose total independence before being capable of maintaining an economically sound and stable society, they "may find themselves ruled by an indifferent and corrupt minority, with nothing to comfort them except the thought that their new oppressors have skins of the same color as their own" (p. 97).

Interesting as the book is, Heine's ideas need and deserve much more development than he presents in this first work. Also, his weak organization of material causes occasional repetitiveness. Further, the timeliness of its content is lessened as status negotiations move toward solution. However, the validity of his observations regarding separatist tendencies and the effect of growing economic dependency are being confirmed as one of the island groups, the Mariannas, has negotiated independently with the United States and settled on a commonwealth status in the American system while the remaining island districts are still negotiating and appear to be settling for free association status with a hefty economic retainership promised by the United States.

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The Comintern in Mexico. By Donald L. Herman. (Washington, D.C.: Public Affairs Press, 1974. Pp. 187. $10.00.)

Robert Alexander's introduction states the work's general thesis rather succinctly; the Comintern itself, including the twisting and turning of its line, was one of the major handicaps to building a Mexican Communist Party with effective local influence. Sending agents to Mexico in the early days of the Comintern to "develop a maneuver against the United States on behalf of the Soviet Union, with little regard to Mexican circumstances" typified this (p. vi). Local Communist parties were nurtured by the Soviet Union, but the mother of Communist revolution often devoured its children. In Mexico, Communist agents, unfamiliar with local conditions, did not succeed in infiltrating the labor movement and had a negligible effect in securing Soviet foreign policy aims (p. 62).

The Comintern leaders called for a full development of the "bourgeois democratic" revolution in Mexico and the gradually ascending importance of the Communist Party (p. 83), which would support the government most of the time and nudge it in the proper direction. Communist successes in gaining influence among some middle-class intellectuals, the peasantry, the trade unions, and the educational system were apt to be short-lived. They were due to local conditions in the country or the beneficence of a particular regime (Cárdenas) (pp. 162–163), and did not reflect the success of external direction. The greatest achievements were in the Popular Front period, with sharp losses occurring after the Nazi-Soviet pact that were never recovered, even in the period of great power collaboration in World War II (p. 154).

Professor Herman's work, focusing on international communism and Mexico, only briefly deals with communism as a Mexican phenomenon, a task which Schmitt's *Communism in Mexico* (Austin: University of Texas Press, 1965) seeks to accomplish. It contains some useful data on the assassination of Trotsky (1940) and the circumstances under which he had been granted refuge by the Mexican government. But by and large the book only confirms the slavish dependence of many Communist parties on the Comintern line through 1919–1943 (except for the tendency of the Mexican comrades to side with the government in suppressing revolts instead of remaining neutral). It is essentially a country-centered adjunct to Alexander's, Alba's, or Poppino's work.

Herman contends that China might develop an apparatus along the lines of the Comintern to spread its influence through various parts of the world (p. 156). I have reservations about this possibility. While the Chinese probably have a clandestine apparatus in operation, they operate within a radically different international environment from that of the 1920s. The relative failure of the Soviet-directed Comintern may be instructive to the Chinese who avoid suggesting any dictation to outside regimes or movements. The Chinese have preferred the strategy of "parallelism." Foreign audiences will sympathize with Chinese positions because of similar internal situations or external foes. Maoism has not been a commodity prepackaged for export.

Herman attacks the notion that left-of-center democratic regimes offer the best resistance to communism. It depends upon the attitude of the government toward the growth of communism (p. 164). The Communists did prosper relatively under just such a regime, that of Lázaro Cárdenas in the Popular Front period. But then Herman cites the reform regimes of Betancourt and Leoni in Venezuela who resisted the Communists. The comparison is not completely appropriate because the Venezuelans put down an insurrection abetted by the Castro regime. The Soviet Union was far away in the late 1930s, the Communists could be useful allies on some issues, and deals could be made. This is a far cry from a nearby Cuba and a movement that posed an open revolutionary challenge to the regime.

The Comintern in Mexico joins a still modest (excluding Cuba) but growing literature on Latin American communism and Soviet policy in Latin America. Such efforts will be encouraged by the possibility of direct access to Cuba and the post-mortem analysis of the Allende phenomenon.

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Nuclear Navy, 1946–1962. By Richard G. Hewlett and Francis Duncan. (Chicago: University of Chicago Press, 1974. Pp. xv, 477. $12.50.)

Nuclear Navy is a thorough and illuminating history of the development of nuclear-powered ships in the U.S. Navy from 1946 to 1962. Although its portrait of the leading character – Admiral Rickover – is disappointingly bloodless and it does not discuss the relation of Rickover's program either to the rest of the Navy or to the broader issues of military strategy, this careful treatment of a major innovation can be read with profit not only by students of the Navy, but also by those concerned with the development of new technologies and the behavior of large organizations.

From today's perspective it is easy to underestimate the hurdles that the program had to surmount. In the years immediately following the war nuclear power was not yet a reality, the defense budget was small, nuclear resources were extremely scarce (with bombs having first priority), the Navy was not known for its receptivity to innovation, and Rickover's position was weak. Nevertheless the *Nautilus* was launched in January 1954, on time and with its specifications met, and the following years saw the spread of Rickover's "system" both horizontally through the resisting Navy and vertically through the resisting private contractors. It would have been easier to explain why the program languished and went down blind alleys than it is to account for its success.

How, then, did Rickover do it? According to Hewlett and Duncan, respectively chief historian and assistant historian of ERDA, at all stages of research, development, and production, Rickover insisted that he and his organization have all the information and power they needed to make crucial – and often less than crucial – decisions. Partly as a result of his wartime experience in which he found that his "severely practical approach," his tireless energy, and his refusal to compromise on technical excellence paid off handsomely (p. 33), he rejected the usual decentralized method of setting specifications and then letting contractors and builders solve the remaining problems. Instead Rickover and his men exercised much closer control over the laboratories and the shipbuilding yards than was normal. By refusing to be treated like a customer who had only to await the finished product, Rickover re-ordered the relationships between the Navy and the contractors. Painful conflicts resulted, but the Admiral got what he wanted.

This method, the authors explain, required that Rickover surround himself with men who had mastered the relevant technical skills. This may seem innocuous enough, but when acquiring these skills took years of training, this requirement conflicted with the Navy's insistence that its men be competent in as many areas as possible and change jobs frequently. To get the most from these people Rickover structured his office in a way that bore little resemblance to the formal hierarchical pattern characteristic of the military and other large organizations. Lines of responsibility were unclear and conflicting, rank and responsibility did not necessarily coincide, expertise and argument rather than authority carried dominant weight, and unusual techniques – even one that was a cross between a T-group and a Quaker meeting – were used to solve knotty technical problems.

These devices served to provide Rickover with the information and opportunities for making key technical decisions. And decide he did. In early 1949 he decided to push ahead with the development of a water-cooled reactor although it was not certain that this method would succeed. And he decided on concurrent as opposed to sequential development of the key components – the construction of the reactor for the *Nautilus* was started before the test reactor was completed. Newlett and Duncan stress that these choices were symptomatic of Rickover's belief that he faced an engineering problem and that he had to set firm goals rather than accept the favored approach of pure science and explore all the alluring possibilities.

These decisions and management techniques, however, do not tell us how Rickover amassed sufficient political power to get his program through. And Hewlett and Duncan's treatment of this question is not entirely satisfactory. They say little about how his connections to Congress were developed, maintained, and employed. But they do point to a less well-known source of Rickover's power. He held the position of liaison officer in the AEC bureaucracy as well as that of head of a Navy office. At first glance these dual roles might seem to have imposed impossible burdens, but Rickover used them as resources of information and leverage. In the early years he could "take the initiative when neither agency was willing or able to act" (p. 92). And when the submarines came into service he maintained his influence over how they were operated through the AEC's role in setting safety standards.

If *Nuclear Navy* is to be faulted, it is not for the answers it gives but for the questions it fails to address. Four stand out. First, could methods other than Rickover's have worked? To show what Rickover did does not show that alternative approaches would have had drastically different outcomes. Would the program have suffered if it had been less centralized? Did the stress on engineering lead to options being closed too quickly? Second, what were the costs of Rickover's approach? Would a great deal of money have been saved by moving less quickly? Was the gamble of concurrent development justified? For two and a half years the

Nautilus was the only nuclear submarine in commission. Although much was learned from its experiences, it is not self-evident that this information was worth the price of a crash program. Third, what was the impact of Rickover and the nuclear power program on the Navy? Did the conflicts between the Admiral and the Navy make the Navy less sympathetic to technology and innovation? Has Rickover's power inhibited progress in nonnuclear areas? Have too many resources gone into nuclear-powered ships? Fourth, was it really wise to have started developing a nuclear submarine in the late 1940s? From today's perspective the answer may seem a clear yes because nuclear submarines are much better than conventional ones for carrying a major burden of deterrence. But in the period when the program was started no one thought that submarines would serve this function. Rather it was thought that they would perform the whole range of missions that submarines had undertaken in World War II, especially, given the lack of Russian surface targets, attacking Soviet submarines. Although nuclear submarines could perform this mission much better than conventional ones, it is still questionable whether this advantage was great enough to justify the costly crash program that the government adopted. In the case of nuclear submarines – and, I suspect, in many others – the development of the dominant mission followed, rather than preceded, the development of the weapon.

Questions like these are not considered in this book. The authors are historians and, as they note, decided "not to attempt to use the analytical methods of the political scientists or sociologist" (p. xii). Readers of this journal might prefer those approaches as represented in this area by Harvey Sapolsky's *The Polaris System Development* (Harvard University Press, 1972), and Vincent Davis's *The Politics of Innovation: Patterns in Navy Cases* (University of Denver School of International Studies Monograph Series, 1967), but they will also learn a great deal from this book's careful reconstruction of how Rickover led – or dragged – the Navy through an astonishingly quick and successful major innovation.

ROBERT JERVIS

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Sino-American Detente and Its Policy Implications. Edited by Gene T. Hsiao. (New York: Praeger Publishers, 1974. Pp. 319. $16.50.)

In February, 1972, Richard M. Nixon, as President of the U.S., announced that his trip to Peking had "changed the world." In June, 1973, a symposium sponsored by Southern Illinois University was held in Bermuda to assess the initial repercussions of the 1972 Shanghai Communique and the Sino-American detente. This volume grew out of that symposium.

On the whole, it is an informative and instructive book. Together the authors convey a consensus that Sino-American detente has indeed had profound impacts around the world; that in the Asia-Pacific theater there is now a new power alignment and new opportunities are open for imaginative statesmen; that fundamental differences remain between the U.S. and the PRC but their bilateral relations will continue to improve; that the PRC has finally become an accepted, full-fledged, responsible, leading member of the community of nations; and that the Taiwan question has been shelved, but not solved.

As happens with many such conference proceedings, however, there are serious gaps in the book's organizational framework, and the fifteen essays are of uneven quality. In its two parts, the book purports to deal with "broad policy problems" raised by the Sino-American detente and the detente's "impact on third countries and Taiwan" (p. xvii). Yet no attention is given to global implications beyond the Asia-Pacific region in Part I, while the impact upon African and Latin American nations is totally ignored in Part II.

Some of the individual essays represent serious research but others seem not much more than thoughts quickly thrown together before the conference. Among the more substantial pieces, two concern themselves with Asian international politics. Akira Iriye, conspicuously ignoring the cold-war theorists, proposes that "the story of Asian international relations for a quarter century after 1945 was that of a progressive deterioration of the Yalta system," and that the Sino-American detente since 1972 is "part of the search for a new structure of peace, as an alternative" to the Yalta system (p. 17). A. Doak Barnett projects a "dynamic equilibrium" marked by greater flexibility and more political and economic rather than military relationships in the new four-power balance in Asia. He also spells out several scenarios of dangers to the new patterns which are useful guides for thinking about the future.

Two papers deal with Sino-American bilateral exchanges. Jerome A. Cohen discusses specific questions about trade that effectively open our eyes to the realities of both possibilities and limitations. Considering matters of scholastic exchanges, Stephen Uhalley, Jr., argues convincingly for a "new disposition to learn" and a "cooperative group approach" as did the Committee of Concerned Asian Scholars.

Two analysts examine the record of Peking's participation in the UN. John G. Stoessinger gives, in regard to three major UN organs, a summary so brief that it barely whets the reader's appetite. William R. Feeney's follow-up essay covers, with much more substantial data and documentary support, the issues with which Peking has had to cope since 1971. The two analysts agree that China has behaved much like other members and that, on balance, there have been positive effects for the UN as well as for China herself. Hindsight suggests, however, that both Stoessinger and Feeney may well have underestimated Peking's impact upon the UN in a very significant way. The Third World nations have demonstrated in the General Assembly session during 1974 a new "intransigence" vis-à-vis the superpowers. One might ask: Is this a consequence, at least in part, of Peking's encouragement, support, and leadership? Is the UN not going through another important transformation owing to what Ambassador John Scali has called, somewhat tritely, "a tyranny of the majority" (a majority noteworthy of its name were China not a part of it), and the major powers' negative reaction to it?

Turning to impacts on specific countries, George Ginsburgs examines with his characteristic insight Moscow's perceptions and its many maneuvers to counter the detente. He identifies US-USSR relations as the one area in which the Soviets have come out ahead, which is ironic perhaps, but not surprising. Editor Gene T. Hsiao's own analysis of the Sino-Japanese rapprochement is a succinct work of first-rate scholarship. There are few clearer manifestations of the adage that "politics is the art of the possible" than his description of Japan's "reverse separation of politics from economics" policy toward Taiwan after Tanaka's Peking visit (pp. 174–176). The essays by William J. Barnds and Dick Wilson, dealing with realpolitik respectively on the Indian subcontinent and in Western Europe, are also lucid.

Compared to these fine papers, others are weaker. Stanley Karnow reiterates the "love-hate" syndrome in the American news media and speculates that "the Chinese deliberately inflated Nixon's image so that unless he acquiesced to their demands, he would leave China empty-handed" (p. 82). Such a speculation obviously underrates Nixon's and Kissinger's diplomatic prowess while giving the Chinese undue credit. Chong-sik Lee seems preoccupied with explaining Seoul's position in contending that the net impact of Sino-American detente on the two Koreas has been clearly a loss for the South and a gain for the North. Kenneth P. Landon, in a chronological narration, tells us that the detente actually led to Hanoi's decision to accept the Paris peace accord. But the support for his assertion that "if it were not for the U.S. detente with China, there would be no further national American interest in an otherwise local Indochina situation" (p. 224) is tenuous. China may want a Balkanized Indochina as Professor Landon suggests, but to say that the U.S. was in Indochina solely for the benefit of the PRC has implications about American foreign policy which Professor Landon himself might find difficult to swallow. Robert W. Barnett's paper on "A Future of Taiwan" is another unexpected disappointment. Without examining the actual position of either the Taipei Government or any other spokesman of the Taiwanese populace, Mr. Barnett offers this piece of advice: "The challenge of Taiwan's diplomats . . . will be to find a scheme of arrangements in which Taiwan's ideological and administrative differences from the other provinces of China and its physical separateness would be perceived in Peking as offering advantages to all Chinese on both sides of the Taiwan strait" (p. 266). Has he already determined the future destiny of sixteen million people in a high-handed and casual fasion? Finally, Michael Witunski's Epilogue presents a refreshing analysis of Japan's vulnerability and potential limit. But his description of future world population and food problems and the plight of the poor nations, though a reflection of a personal concern shared by many, seem irrelevant to this volume.

Useful as this volume is, June 1973 was too close to the events of the preceding February to permit assessing the important implications of the Sino-American detente. Readers wishing to find a definitive evaluation on this subject will have to await future works.

BYRON S. J. WENG

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Expropriation of U.S. Property in South America: Nationalization of Oil and Copper Companies in Peru, Bolivia and Chile. By George M. Ingram. (New York: Praeger Publishers, 1974. Pp. xx, 392. $25.00.)

As has become apparent in recent reviews in this and other journals, Latin Americanists in political science have begun to join economists in the study of expropriation/nationalization/confiscation of U.S. corporate entities in Latin America. George Ingram has made an important contribution with this comparative case study of three hemispheric nation-states. As Staff Consultant to the Subcommittee on Foreign Economic Policy of the House Committee on Foreign Affairs, he presents a chronological, quite *traditional* study of conditions under which large U.S. corporations have been taken over by Peru, Bolivia, and Chile. The pertinent factors in each case are described in an unprejudicial manner.

I have reservations, however, about the book's relevance as a study in political science. The conceptual framework, such as it is, is sparse. Only a generalized listing of "reasons for expropriation" is given with an analysis which should be quite easily understood by the undergraduate student in an introduction to Latin American politics course having superficially covered material on Latin American economic nationalism. In addition, a section on the "consequences and aftermath of expropriation" is included. This latter portion is more incisive than are more unsophisiticated studies of this type. The methodological looseness of the book lies in the total lack of any propositional statements or hypotheses. One would do well to contrast Ingram's work with Charles Goodsell's recent exemplary study of IPC in Peru (*American Corporations and Peruvian Politics* [Cambridge, Mass.: Harvard University Press, 1974]). Ingram's excessively historical treatment given the data *has* resulted in a very readable book, but like many case histories, it threatens to overwhelm the reader with detail without sufficient analytic tightness.

Despite this criticism, Ingram's statement of causes and "aftermaths" in each of the three cases, together with his remarks in the concluding chapter, represent carefully and consistently balanced appraisals of each. Instead of the polemicized, xenophobic thrust of the legalist so often associated with traditional-historical analysis of political events, the author constructs from an immense amount of data – covering the entire spectrum of materials and philosophy concerning his subject – reasoned statements relating to past and more contemporary behavior both of U.S. corporations and the U.S. government as well as governments of the nation-states pertinent to his study. I am left with the impression that the resulting conclusions are as empirical (factual) and carefully unbiased as anyone could expect. The gnawing question, however, is how the study was ever accepted as a doctoral thesis in one of the more prestigious *political science* departments in this country, since it lacks so much in the crucial area of systematic conceptualization.

With each country – Peru, Bolivia, and Chile – Ingram gives a well-verbalized analysis of the political and economic historical context of each expropriation/nationalization/confiscation (although he never conceptualizes the three terms in any depth: see p. vii). The causal statements for each government's action show fleeting, glancing relationship to the *outline (not* to be confused with what might be considered a tightened framework) regarding Latin American nationalism which is viewed as the "determining" factor in virtually all negative actions against U.S. corporate interests. Further, "nationalism" is never given the treatment that it deserves, never dealt with definitionally or as carefully covered as is his treatment of general political history. By some epistemological quirk, however, the route which the author takes leads to lucid and reasonable statements regarding each government's behavior.

Information concerning IPC (Peru), Standard and Gulf Oil (Bolivia), and Kennecott and Anaconda Copper (Chile) is reinforced and made explicit by an excellent set of 36 charts and tables which show complex economic statistical data. The degree of economic dependency, the issue of the "company town," the ratios of taxes paid to host governments and net profits to gross output and total investment are only a few of the more important variables graphically shown in the Peruvian, Bolivian, and Chilean situations. Possibly, the economic tables are the most important contribution of Ingram's study. They bring more clearly into focus conclusions concerning "blame" as well as "good" or "bad" conduct on the part either of the U.S. companies or of the Latin American governments.

No one can come away from this study with serious misconceptions: First, the U.S. corporations with very few exceptions (possibly the Bolivian-based oil companies) were "asking for" the anger and retaliation of host governments. Second, the host governments involved allowed themselves to be exceedingly vulnerable to a strangling penetration of their economic and political sovereignty by U.S. interests. Third, it is abundantly clear that both corporations and governments used highly questionable bookkeeping systems, legalistic arguments, and political methods to support their respective interests in all three instances. Having sifted through a huge amount of data, the author of this study has demonstrated the correctness of conclusions I have found in numerous other works on Latin American expropriation. This adds to the plausibility of many of Ingram's redundantly stated arguments. Nonetheless, the writing is stylistically good and, overall, interest-holding and informative.

In conclusion, I would like to touch upon some of the more interesting aspects of the *economics of publishing* as seen in this product. With the price as stated above, the book is prepared in photographed typescript format. There are no precise right-hand margins. An index is not included although many might find this to be an unnecessary option. Fully eighty per cent of the Spanish words used in the text and references to Spanish-language sources have *no* diacritical marks. Thus, the vast majority of Spanish words are misspelled! The footnotes are at the end of each of the four long chapters (in long footnote form) and there is a lengthy bibliography at the end of the book. All this is aimed at economy, no doubt, and the price must therefore amaze and baffle – not to mention discourage – the prospective purchaser. Ingram has written a relatively good book; the publisher has done less than justice to him in its physical presentation to the academic community.

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The International Bibliography of Political Science, Vol. XXI, 1972. (London: Tavistock Publications, 1974. Pp. 293. $23.00.)

The International Bibliography of Political Science is a component of the extensive International Bibliography of the Social Sciences prepared annually in four parts by the International Committee for Social Science Information and Documentation (ICSSD) with the financial support of UNESCO. The present volume, the 21st in the political science series, offers 4356 listings for books, periodical articles or other publications issued in various languages throughout the world between 1970 and 1972. The listed items represent publications of true scientific content only and are arranged within a detailed classification scheme consisting of six main sections, namely: A – Political Science; B – Political Thought; C – Government and Public Administration; D – Governmental Process; E – International Relations; F – Area Studies. The main sections are further subdivided by subject categories or countries. An outline of the complete classification scheme is printed in the introductory pages of the bibliography together with references to the listed items. Additional access to the listed literature is provided by separate author and subject indexes in English and French.

Most of the bibliographic listings refer to articles selected from about 1300 periodicals regularly consulted by the ICSSD and its national correspondents. Entries for an article frequently include references to an abstract of the article published in issues of the International Political Science Abstracts. Entries for books are occasionally accompanied by a reference to reviews published in the periodical literature.

In view of its selection goal, the bibliography does not provide access to a wide range of political information material, such as publications which are very recent in origin, journalistic or polemic in presentation, or concern special branches of study. The bibliography also excludes unpublished political documentation, notably conference papers, unpublished theses, internal government reports, etc. As in the past, the chief value of the bibliography therefore lies in the fact that it provides an annual international publications record of political studies which have gained recognition in the scientific community.

FREDERICK HOLLER

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Between Sovereignty and Integration. Edited by Ghita Ionescu. (New York: John Wiley & Sons, 1974. Pp. 192. $11.95.)

One of the themes of the ninth Congress of the International Political Science Association in Montreal in August, 1973, was "Pathways to Peace: National Sovereignty or Supranational Integration?" This book collects the nine main papers which were discussed and adds an analysis abstracted from two Eastern European late submissions.

In Ionescu's Introduction, his restatement of the thematic question prejudicially anticipates an answer which a number of the papers reject. In the interests of peace, asks Ionescu, shall we have "*balkanization,* i.e., the erratic behaviour of a multitude of precarious nation states . . . prone to crises" or shall we have "modern federations, integrating vast and viable territorial units?" (p. 8). The first answer to this question, Max Beloff's, concludes a very brief examination of the political crisis of the European nation-state with the observation that "one may be writing off the nation-state all too soon" (p. 31). Dennis Kavanagh, looking calmly at the political significance of the large business corporation, comes to a similar conclusion. The nation-states, in this context, he finds, are too small for some purposes and too large for others, but the situation "will have to become much more unpleasant for the states to institute a new compact" (p. 64). Geoffrey Goodwin, in a paper of great breadth and penetration, considers the alleged erosion of external sovereignty. He concludes that "peace is to be sought not by adumbrating blueprints premised upon the imminent demise of the sovereign state, but through some pragmatic mix of the recipes for reconciling the facts of state sovereignty with the requirements of international order" (p. 116).

The "Two Views from Eastern Europe" by Ioan Ceterchi and Karel Pomaizl are quite unreservedly hostile to any diminution of national sovereignty, for they see it as a continuing "primordial" factor in social progress. There is a need for cooperation, but nations join international organizations, the authors argue, in order to defend and further their national interests and their sovereignty. Donald Puchala too rejects the integration = peace equation. His paper is replete with sweeping, often naive generalization. Thus: "There is no question . . . that European integration . . . has contributed to world peace. Europe is no longer the internecine battlefield that it was for centuries. The Franco-German border is no longer arrayed with fortresses" (p. 164). For fortresses he should look at the East-West German border and ponder the role of the cold war and nuclear stalemate in Western unity. He concludes, however, more or less reversing the case he has tried to make, that "historically speaking, there is almost an inverse relationship between international integration and peace" (p. 179). Finally, Karl Deutsch, in a provocative concluding paper, asks why we should want integration. "This notion that groups that do not integrate must destroy each other is a widespread but false belief" (p. 181). An integrated community might mobilize much larger capabilities, which might help deal with the dangerous problems of world poverty, but, Deutsch warns, political integration, even of the pluralistic kind, requires a very high degree of integration of the social fabric, difficult to attain. None of the direct answers to the question, then, suggests that the nation-state cannot survive or that it must go.

Other contributions, less directly concerned with speculation on the survival potential of the nation-state or pathways to peace, include a very clear essay by David Coombes on "concertation" (i.e., government in a private-enterprise economy based on the prior consultation and consent of the interests affected by any decision). This has rather more immediate practical implications than any of the other contributions because of its narrower scope. The chapter is titled " 'Concertation' in the Nation-State and in the European Community." Coombes compares the two levels to expose the contradictions inherent in the concept of "concertation," e.g., the likelihood that short-term difficulties and unforeseen crises may upset the balance of interests upon which "concertation" is based. Believing that "concertation" cannot meet the problems of the state or of the regional community, Coombes stresses rather the importance of strengthening governmental authority at appropriate levels of centralization and devolution. This injunction, federalist in its implications, and at odds therefore with most of the other papers in this collection, is, nevertheless, the one which is most convincingly argued.

Herbert Spiro would almost certainly reject it. In a stimulating examination of American political development, with its obvious relevance for integration theory, he attacks the view that the American system evolved in conditions of internal peace imposed upon the expanding United States from the "center" established by the constitution – Washington, D.C. He argues rather that growing interdependence sanctioned the federal constitution and government. The Civil War was an aberrant occasion during which the South perceived its problems as irrelevant and peripheral to the rest of the country. Of course, there is some truth in this assertion, but Spiro strains it somewhat. For without an original federal constitutional drafting and ratification exercise, and the creation thereby of an independent federal power, there would *be* no federal government today. A central government with the range of powers Washington has acquired and now exercises would not have grown out of interdependence. Rather, as Goodwin reminds us, quoting Hoffmann, "it is one of Rousseau's deepest insights that interdependence breeds not accommodation and harmony, but suspicion and incompatibility" (p. 102). A high degree of interdependence of internal "interests" does not eliminate suspicions and incompatibilities even in a well-established sovereign state. It reinforces the vital anterior procedures for authoritative decision making.

Other papers in this collection are contributed by David Apter, "Luxemburgensis" and Léon Dion. The first two of these, respectively "The Premise of Parliamentary Planning" and "The Emergence of a European Sovereignty," are relatively unremarkable. Léon Dion's paper, "Anti-Politics and Marginals," however, is an examination of the internal threat to the stability of the liberal democracies. It reduces to a case study of radical opposition movements in Quebec on the provincial and federal stage. This particular case, complicated as it is by the problem of nationalism, lends itself to some exaggeration of the more general crisis of protest and revolt in the democracies, but Dion's is nevertheless an interesting contribution to a symposium of more than average coherence.

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The Middle East in World Politics: A Study in Contemporary International Relations. By Tareq Y. Ismael. (Syracuse, N.Y.: Syracuse University Press, 1974. Pp. 297. $12.00, cloth; $5.00, paper.)

One can approach the study of the Middle East in international politics in three possible ways: chronological-historical, involving periodization and events-selection according to some justifiable historical criteria; issue-oriented, in which important issues such as the Arab-Israeli conflict or Middle East oil are chosen to be explained; or a conceptual scheme in which elements of the first two approaches are included but within an analytical framework that transcends mere description and a configurative focus and instead provides the basis for a comparative assessment in both vertical and horizontal terms. This study on the role of the Middle East in world affairs, edited and co-authored by an Iraqi-born, American-educated political scientist teaching at the University of Calgary in Canada, strives unsuccessfully to incorporate all three approaches into a single format and, in the process, does an injustice to qualitative descriptive history, distorts issue-oriented analysis, and trivializes the subordinate international systems approach.

Six of the book's eleven chapters are divided according to conventional country-by-country categories prefaced by a historical overview by Harry N. Howard, who also contributes chapters on United States and United Nations relations with the Middle East. P. Edward Haley provides a single section on British-Middle Eastern encounters in the modern period and the history of post-war Franco-Middle Eastern contacts are presented by Naseer H. Aruri and Natalia K. Hevener. Three separate chapters on Soviet, Chinese, and African relations with the Middle Eastern world are authored by Ismael himself who concludes the volume with selections on "Domestic Sources of Middle East Foreign Policy," "Oil: The New Diplomacy," and "The Middle East: A Subordinate System in Global Politics."

Lacking any kind of clearly delineated analytical scheme or even an elementary organizing framework by which to order data and comprehend reality, this book plunges the reader into a brief and rather superficial historical introduction in which a battery of discrete and at times totally unrelated and even inaccurate historical facts are routinely presented (e.g., the French Protectorate in Morocco was established on March 30, 1912, with the signing of the Treaty of Fez, not in 1904. Similarly, the British defeat of Napoleon in Egypt occurred in 1801 not 1799 [p. 3]). The country-by-country selections that follow consist of little more than descriptive and often hurriedly rehashed accounts of the major events that have characterized a particular country's or region's relations with the states of the Middle East and North Africa. Since no attempt is made to define politically or delineate geographically the Middle Eastern-North African region until the book's *final* chapter, it is quite understandable although unfortunate that each author proceeds apparently oblivious of what preceding and subsequent authors have written, resulting in often annoying repetitions in factual and interpretive materials.

Ismael's chapter on the "Soviet Union and the Middle East" illustrates the kinds of analytical and substantive shortcoming that characterize much of this entire volume. After casually dismissing any discussion of pre-1945 Soviet foreign policy intentions and practices in the Middle East, the author proceeds, via an array of uneven secondary sources arranged in a cut-and-paste format, with a second-rate journalistic account of some of the major episodes in modern Soviet-Middle Eastern history and the means, both intentional and accidental, by which it achieved significant status in the area. Incredibly the author provides a mere two paragraphs on the October 1973 Arab-Israeli war with all its policy implications for Soviet defensive strategy and long-term politico-ideological objectives in the Arab world. He fares no better with a prognostication that "with the American role in the Arab world bankrupted [*sic*] by its total commitment to Israel during the [October 1973] war, the future should witness greater Soviet-Arab cooperation in the political and economic realms" (p. 114).

Howard's chapter on the United Nations borrows extensively from a nearly identical yet obviously dated article published by the same author in 1971 (*Current History*, 60 [January 1971], 7–12, 49), in which he presents a crisis-by-crisis discussion of UN involvement in the Middle East including the Greek problem of the late 1940s, the 1958 Lebanese civil war, United Nations action in the Yemeni conflict, the 1964 Cyprus dispute, and the ongoing Palestinian problem. This article puts forth little that is new or useful as evidenced by the chapter's concluding sentence: "During the fall of 1973, following the outbreak of another round in the Palestine conflict, the UN was called upon, once more, to observe and report concerning the cease-fire, and thereby to contribute to the possibility of an enduring peace in the area" (p. 194).

Such a seemingly offhand dismissal of so important an event as the Yom Kippur war is not the fault of any of the book's several collaborators but of editor and publisher alike who, in their apparent haste to publish and market a book of obvious timely appeal, took the intellectually narrow but possibly economically lucrative path. A more reasonable approach would have been either to eliminate altogether any discussion of the events of October 1973 – with appropriate explanations in the preface – or, more logically, extend the publication date so that a more contemplative discussion of the latest Arab-Israeli conflict could have been included. Instead, terse, frequently superficial, and at times inaccurate statements, awkwardly appended to the end of practically every chapter, reflect the caliber of analysis of the developments of October 1973. The results are predictable. Ismael writes: "The October 1973 War in fact consolidated the Soviet Union's military commitment to the Arabs. Its confrontation with the United States . . . raised Soviet popularity to new heights in the Arab world, at all levels" (p. 114). Events immediately after the war and since have entirely disproved this interpretation.

A separate chapter on the relatively new phenomenon of "oil diplomacy," while timely, does not contribute substantially to our understanding of the dynamics of international oil politics and the manipulative role of crafty petroleum potentates. Reflecting Ismael's uncertain footing in this area and/or casual investigatory process is the following statement: "And with the creation of the Organization of Oil Producing and Exporting Countries (OPEC) in 1960. . ." (p. 232). I am unaware of any such organization as OOPEC, as OPEC stands for Organization of Petroleum Exporting Countries. Likewise the author quite wrongly asserts that "the [oil] revenues are becoming less useful to the Middle Eastern states that produce [oil]" (p. 233). While massive oil revenues are perhaps becoming less usable to such lightly populated countries as Kuwait, Qatar, the United Arab Emirates, and even Saudi Arabia this is clearly not the case for Iraq, Iran, and Algeria, whose relatively large populations and ambitious developmental schemes require extensive oil-generated revenue and capital formation at a rate at least equal to that achieved in mid-1974.

The book's final chapter on the Middle East as a subordinate international system logically should have appeared in the beginning where possibly it could have served that crucial organizing function that seems so visibly absent throughout this work. In fact, for organizational clarity, the first three chapters should have been, respectively, the present chapters eleven (subordinate system), one (historical background), and nine (domestic sources of foreign policy).

On balance this book offers little that is new or provocative. Its straightforward chronological account has no empirical or theoretical usefulness. It is indeed regrettable that by the time this review appears in print students will already have been exposed to the book's rather shallow and, at times, biased interpretation of an area of the world that remains desperately in need of what Ismael himself promises but does not provide: "an objective analysis of the role of the Middle East in world affairs" (p. vii).

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Soviet Conquest from Space. By Peter N. James. (New Rochelle, N.Y.: Arlington House Publishers, 1974. Pp. ix, 256. $8.95.)

This is an interesting study of Soviet space achievements and their implications for the Soviet-American military balance in the decades ahead. Peter James – a former space systems analyst from private industry – presents an overview of the Soviet science-technology-military-espionage-industrial establishment and reaches some distressing conclusions about whether the United States can match Soviet space accomplishments. He describes Soviet manned and unmanned spacecraft, space station, and reusable earth-to-orbit shuttle programs. In the process, he persuasively argues that America has neglected the military potential of near-earth space.

One fascinating sidelight in the book is the author's account of his numerous meetings with Soviet scientists and intelligence officers at various technical conferences throughout the world. Mr. James's assessments of their space programs were then keyed into the U.S. intelligence system. The entire process of the exchange of scientific and technical information is one which should receive more attention from political scientists.

In a sense, Mr. James strengthens the cause of present-day hawks, inasmuch as he suggests that the U.S.S.R. is striving for absolute strategic superiority. He insists that the Soviets lead the United States in the development of manned orbital spacecraft by five years and of earth orbital space systems by three to five years.

He criticizes the American government for cutting back on space programs for budgetary reasons and he chastises the intelligence community for being too compartmentalized and separated from scientists and technicians to effectively assess Soviet military and strategic capabilities. He supports the prevalent criticism that the C.I.A. is dwarfed by the military intelligence services to such an extent that there is no efficient coordination of American intelligence efforts.

U.S. space planning was centered around the Apollo mission, argues the author. With the completion of that project, this nation does not have the launch vehicles to compete adequately with the Soviet Union in the decades ahead. He predicts massive Soviet space breakthroughs in the late 1970s. The political problem is that the public allegedly has been lulled into complacency by the spectacular success of Apollo and is more concerned with inflation and related difficulties than with spending more money on near-earth orbital space projects.

Mr. James contrasts the Soviet public's fascination with space programs to America's disillusionment with space. He perhaps too glibly blames the mass media in this country for downplaying these activities and for dramatizing the "excessive" costs involved.

One can adopt the author's strong case concerning the need to expand our near-earth space capabilities without accepting some of his hawkish attitudes about other military systems, SALT agreements, and détente. We certainly need a wider public debate about defense policies for the future. I would favor expanded space programs along with more comprehensive SALT accords; others could maintain precisely the opposite. It is hoped, then, political discussions could escape the dead-end of promilitary versus antimilitary arguments.

Soviet Conquest from Space is a well-written primer on Soviet space programs and is designed for the general reader. It does not presume great technical knowledge. Its principal weakness is that it is much too superficial in its coverage of the political implications of America's policies relating to the Soviet challenge.

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Return from the Nuclear Brink: National Interest and the Nuclear Nonproliferation Treaty. By Lloyd Jensen. (Lexington, Mass.: D.C. Heath and Company, 1974. Pp. 176. $12.50.)

India's nuclear explosion makes this book very timely. It offers systematic and thoughtful comments on the past and probable future of the Nuclear Nonproliferation Treaty. The approach is businesslike and unemotional, and clearly faces the important fact that "just as there are risks with armaments, there are also risks in disarmament and arms control" (p. 33).

The general message can be summarized by quoting three sentences (from p. 113):

> Events since the opening of the Nuclear Nonproliferation Treaty for ratification provide little optimism regarding a return from the nuclear brink.... Even should a number of critical threshold states ratify the NPT in the near future, one cannot overlook the many reservations which both participants and non-participants alike have directed toward the Treaty, all of which raise serious questions about its viability. Since withdrawal requires only a three-month warning period, the prospect of treaty abrogation remains a continuing concern.

The importance of the twenty "near-nuclear" or "threshold" states is emphasized. They probably have the most to lose by adhering, not least in terms of political bargaining power foregone. Professor Jensen points out that a state may obtain maximum leverage if it is not a signatory to the Treaty, is able to produce nuclear weapons, but has not in fact acquired them.

This book went to press before the Indian explosion. With the benefit of hindsight, it is always easy to criticize a forecaster, and, while the author clearly foresaw the possibility of an Indian explosion, his prediction was (p. 99) that Mrs. Gandhi's opposition to nuclear weapons would prevail, "despite the extensive public and private interest in India favouring the bomb." The Indians insist, however, that their explosive device was not a weapon, although the author, as many others have done, explains that in terms of international perception there is little difference between conducting a peaceful nuclear explosion and possessing nuclear weapons. Perhaps in 1984 some nation will be obliged, for purely defensive purposes of course, and in the cause of restoring peace, to detonate a peaceful nuclear explosion on a rival capital.

The next group of threshold countries viewed as possible proliferators are Israel, Japan, and the FGR. Israel's acquisition could be imminent (It is suggested that the Israelis may have only to hook up one last wire [p. 98]). Japan and Germany are not seen as likely to go nuclear unless important changes develop in their security positions, but Jensen points out that their obligations to refrain from the domestic production of nuclear weapons do not forbid acquisition from abroad (p. 95).

Of the various interests which influence a state's attitude towards the NPT, the author concludes that military security is paramount. According to an analysis of the content of debates about nonproliferation in the UN General Assembly, nonaligned states have the greatest concern over security. Presumably members of alliances can rely on collective support, which in most cases includes a nuclear guarantee. Many of the others feel the lack of any reliable guarantee rather acutely. All were conscious of loopholes available to states wishing to evade or withdraw from commitments. The author attempted to relate attitudes toward the Treaty to other categorizations of states, but the only significant ones found were alignment and nuclear potential. The states on the threshold of a nuclear weapon capability tended to be the most reticent to sign.

Although the specific procedures are not discussed, attention is given to the question of safeguarding against unauthorized dissemination of fissionable material. It is clear that safeguards will not be foolproof, will add unwelcome costs, and will be regarded by some as an opportunity for commercial and military espionage. But, in their favor, it can be said that safeguards against nuclear proliferation could establish a basis for international inspection procedures which are likely to be needed for more comprehensive arms control or disarmament measures that may be negotiated in the future.

Many nations have reservations about the NPT on economic grounds, usually because they fear that it will institutionalize the advantage of the nuclear weapon states at the expense of the have-nots. It is extremely difficult to segregate peaceful from military development potential, particularly since power reactors inevitably produce material that can be made into bombs, since it is impossible to prove that a nuclear explosion has no military significance, and since commercial as well as military competition involves secrecy. Undoubtedly some countries are hoping for important economic gains from nuclear technology, and are concerned lest the NPT hinder their access to it.

Finally, there are the questions of prestige and equality. These have their international and domestic aspects, and may be more persuasive for some states than questions of security or economic advantage.

Return from the Nuclear Brink covers a lot of ground in a modest space.

G. R. LINDSEY

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The Birth of Nations. By Philip C. Jessup. (New York: Columbia University Press, 1974. Pp. 361. $14.95.)

Precise and accurate in detail and enlivened by humorous episodes of diplomatic *gaffes*, the achievement of independent statehood by Korea, Indonesia, Morocco, Tunisia, Libya, Somalia, and Israel is recorded by the author of *The Birth of Nations.* Dr. Jessup was an observer and an active participant in the frequently unprecedented parliamentary diplomacy that marked those nationalistic movements. For many years he practiced and taught international law. He served as Ambassador-at-Large under President Truman. He was often the United States representative at sessions of the General Assembly and of the Security Council of the United Nations. Subsequently he served as a judge of the International Court of Justice at The Hague.

It may surprise many readers, familiar with Dr. Jessup's learned treatises on issues of international law, that in the present work he resorts to obstetrical terminology. Thus, the "rebirth" of Korea in 1948, an ancient state although ruled by Japan from 1910 to 1945, was accomplished through the "midwifery" of the United Nations in general, and of Jessup in particular. A chapter dealing with Morocco and Tunisia is entitled "Prenatal Pains." The United Nations "delivered" Libya and Somalia. The prolonged agony of the period of labor, and the sharp birth pangs of Israel are recounted. The clinical figure of speech is stretched to cover the "abortive" empire of Bao Dai in Vietnam. The attempted secession from Ethiopia by Eritrea was "stillborn." The epithet "national bastardy" is applied to Manchukuo, whose out-of-bounds parent resided in Tokyo. "Rhodesia is not even admitted to the diplomatic maternity ward" (p. 303). Dr. Jessup speculates that if Japan had won in World War II, "Manchukuo would have been legitimated . . . but puppets are very likely to be the victims of infanticide" (p. 334). However, the author does not attempt in this thin volume to describe the seven countrys' post-partum depressions or raging fevers.

In a carefully drafted chapter on Israel it appears that in 1947 while a member of the American delegation to the UN General Assembly, Mrs. Eleanor Roosevelt (characterized by Jessup at page 267 as "an ardent champion of the Jewish people") offered her resignation to President Truman when the official United States position regarding the partition of Palestine was suddenly changed. In the continuing tortuous course of the UN preoccupation with that area, Mrs. Roosevelt in 1948 wrote to Secretary of State George Marshall. She complained that "the way in which the recognition of Palestine [*sic*] came about has created complete consternation in the United Nations . . . representatives of other governments have stated quite frankly that they do not see how they could ever follow the United States' lead because the United States changed so often without consultation" (p. 286). Jessup, whose duty it became to announce to the General Assembly that the United States recognized the *de facto* authority of the new state of Israel, felt the full force of the reaction to the recognition move. "There was a lack of confidence in the integrity of United States' intentions and disbelief of further statements of future intentions and policies . . . the reaction of others, such as Canada, China, and a number of Latin Americans . . . was due to a feeling, which they frankly expressed, that they had been double-crossed" (p. 285). President Truman had earlier been warned by the prescient Secretary of Defense Forrestal "that if we antagonized the Arabs we would endanger our access to the vital oil supplies of the Middle East" (p. 287). Obviously deeply involved in the Israeli matter, Jessup cites and corrects a number of published reports on the American role.

Truman on other occasions strongly supported the United Nations. Jessup heard the President repeating to himself, "We can't let the UN down!" in 1950 at the time of the invasion of Korea. He "laid heavy emphasis on the fact that the United States was working through the United Nations" in the Korean crisis, and upbraided Secretary of State Dean Acheson for not mentioning the UN in a briefing of Congressional leaders (p. 10). Jessup observes that it was largely the previous UN action "in helping to establish the Republic of Korea [that] added a particular reason for invoking the UN and contributed to the stand of many governments in support of resistance to the [1950] aggression. . . . In contrast, the UN played no role in the evolution of the independent states of Indochina and there was no comparable inclination to have the Security Council play a part in trying to make peace in Southeast Asia" (p. 42).

A dilemma facing the United States was whether or not to throw its influence behind emerging nationalistic ambitions even at the cost of friction with the colonial powers, some of which were old friends and recent wartime allies. The dilemma was repeatedly confronted in the independence movements of Indonesia, Tunisia, Morocco, Libya and Somalia.

Dr. Jessup adds much to an understanding of the maneuvers that resulted in the emergence of the new states. The impulse to write in lighter vein, and to reveal the sometimes hectic atmosphere in which agreements are reached, was encouraged by Dr. Jessup's wife, who accompanied him on his governmental missions. She took an important part in writing the book, which is properly described as a joint venture.

WILLARD BARBER

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The Superpowers in a Multinuclear World. Edited by Geoffrey Kemp, Robert L. Pfaltzgraff, Jr., and Uri Ra'anan. (Lexington, Mass.: Lexington Books, 1974. Pp. 300. $15.00.)

The Indian test of a nuclear explosive in May 1974 suddenly thrust the problem presented by the proliferation of nuclear weapons back into the public consciousness. In the six years since the negotiation and signing of the Non-Proliferation Treaty (NPT) in 1968, the further spread of nuclear weapons had been given scant attention by our foreign policy makers, but the Indian test, combined with the Conference to review the operation of the NPT scheduled for May 1975, is now forcing these issues off the back burner, making a serious discussion of the subject particularly timely. Unfortunately, *The Superpowers in a Multinuclear World* does not make a major contribution to our political thinking or to the literature on this subject.

In May 1973, the International Security Studies Program at the Fletcher School of Law and Diplomacy, Tufts University, convened a conference on the U.S.-Soviet strategic balance and nuclear multipolarity, and the current book is a compilation of the papers prepared for that conference together with a summary of the discussions. As such, it suffers from the weaknesses of most other writings growing directly out of a meeting rather than from a coordinated analysis of a subject; the topics are rarely analyzed in great depth, and they express the personal views of a wide variety of individual authors. The editors point out that the papers do not purport to be a representative cross-section of opinion on nuclear weapons, nor do they attempt to present all points of view. For this reason, Appendix A, "Conference Report," which summarizes the discussions at the meeting, may be of the most general interest. While coming from a wide variety of backgrounds and experience, the authors, and in large part the conferees, are representative of a school more impressed by the political and perhaps even military value of nuclear weapons than I am.

The first three papers deal with the U.S.-Soviet strategic balance in a biased and, in my view, unrealistic manner. John Erickson presents the U.S.-Soviet force structures as demonstrating American inferiority and Russian desires to acquire a first-strike capability; these views have little basis in fact (he ignores the U.S. lead of 8500 to 2800 in nuclear warheads), and the paper would serve only to mislead a serious scholar. Yuri Ra'anan believes that a credible deterrent against Soviet intervention on behalf of Soviet clients embroiled with friends and allies of the West requires that some "measurable edge of U.S. superiority remains and can clearly be perceived" (p. 20). The author ignores that seeking superiority can only lead to a never-ending arms race and is meaningless when both superpowers have many thousands of nuclear warheads. By contrast with Ra'anan, Andrew Pierre, in his later, more useful paper entitled "Can European Security Be 'Decoupled' from America?" (previously published in *Foreign Affairs*, July 1973) points out that the concern in the U.S. regarding the numerical imbalance in strategic offensive missiles in the SALT I Agreements found little parallel in Europe.

Perhaps the most interesting paper is that of Bhabani Sen Gupta, "How Close Is India to the Bomb?" written a year before the Indian nuclear test. This is a good unprejudiced analysis of the political considerations behind the Indian nuclear program and may be useful as one speculates where India may go after having taken its first step down the road toward nuclear weapons. In his thoughtful companion piece, "An Indian Nuclear Force in the 80's," K. Subrahmanyam states his belief that the decision to exercise the nuclear option will not come until later in this decade and that India would not have a nuclear force, in which he includes delivery vehicles – such as missiles – at least until the end of the 1980s. With respect to "Japan's Nuclear Options," Masataka Kosaka points out the realistic problems for Japan to acquire nuclear weapons presented by its dependence on foreign countries for its nuclear resources, but he argues that Japan can maximize her political influence by not renouncing the option altogether.

The spread of nuclear weapons to Israel and the Middle East would have much more serious and dangerous repercussions than to any other area; unfortunately, no paper addressed this issue, and it was not the subject of any detailed discussion at the Conference. Maybe we will have to rely on actual experience to learn what should be the U.S. strategic posture in a multinuclear world.

HERBERT SCOVILLE, JR.

McLean, Virginia

Footsteps into the Future: Diagnosis of the Present World and a Design for an Alternative. By Rajni Kothari. (New York: The Free Press, 1975. Pp. 173. $8.95, cloth; $3.95, paper.)

As a volume in the series *Preferred Worlds in the 1990's*, the mission of this book was prescribed. It was to be a contribution to an educational process whose goal was global reform. The freedom of the author – cooperating with a group of Indian fellow-citizens – was further limited by guidelines designed to turn the series into a set of "relevant utopias." These were meant to be models of future world-order systems which were neither realistic effigies of the present nor figments of the imagination. The author was asked to analyze the contemporary scene, to prognosticate on the basis of his analysis, to outline his preferred world order, and to propose a strategy for realizing it. He was however allowed to use his own perspective. He did so, with the result that in argument and argumentation the book is reminiscent of the international demand of Third World states for the improvement of their condition, with the difference that it is here made in a sophisticated rather than crudely propagandistic manner.

In a humane spirit, the author envisages a world enabling everyone to be better off, at least spiritually, than he or she is now. The emphasis is, nevertheless, upon improving the lot of the Third World. To the author, the crucial issue in restructuring world reality is "to establish the autonomy of nations in the Third World and on that basis to promote a sense of collective self-regard among them so that we can realize the values of individual freedom and dignity, justice in the distribution of worldly goods, and opportunity for participation in decision-making structures at various levels" (p. 6). The *leitmotif* of the book is autonomy and dignity, with the nation-states as the instrument for their realization. Third World states are as yet inadequate instruments, the author claims, because the developed world is dominating them, and because the overwhelming culture and values of the developed world inhibit changes in the intellectual climate indispensable for global reform.

In the face of the contemporary experience that most nation-states rob sections of mankind of autonomy and dignity rather than bestow them, some justification of this unorthodox role assigned to the nation-state might have been expected. But there is only a call for an intellectual effort to alter prevailing worldviews toward an emphasis on self-control and for a merger of the Western culture of science with the more integral and universal perspective of Oriental civilizations (with the implication, perhaps, that in the resulting climate nation-states might indeed accomplish human autonomy and dignity). There is a call also for institutional changes to control the shortcomings of nation-states. Kothari pleads, for instance, for functional international agencies to satisfy the technical and welfare needs of the world population – especially that of the Third World – with power to bring "authoritative pressure" to bear upon states to implement agreed-upon measures (p. 14). An international armed force is recommended to curb violence between and within states. A set of suggested international institutions would protect human rights and standards of justice wherever these are violated. The difficulty of reconciling the power and activities of these agencies with the autonomy of states is hardly mentioned and not resolved.

These intellectual, cultural, and institutional changes are intended to undo the evils the author details in his extended diagnosis of the contemporary scene: violence, exploitation, inequality, injustice and misery among men and states. Their roots are traced to world politics, the social and economic structure underlying them, and the philosophical assumptions informing them. Yet these same evils could be found in the ancient world of the Orient, where they survive. It appears questionable, therefore, whether the responsibility for the prevailing evils as well as for the desirable changes must rest as heavily upon the developed world and as lightly upon the Third World as the author suggests. Had the author been less sweeping and conventional in his analysis of the contemporary scene (e.g., in reemphasizing the hackneyed and practically irrelevant dichotomy between Eastern and Western worldviews); and had he paid some attention to the (admittedly meager) findings of political science regarding world politics and the behavior of states, perhaps he might have conceived of a more "relevant" utopia.

WERNER LEVI

University of Hawaii

Confrontation: The Middle East and World Politics. By Walter Laqueur. (New York: Quadrangle/The New York Times Book Co., 1974. Pp. 308. $8.95.)

Political and historical accounts written soon after the events take place can be as brilliant and enduring as Thucydides' *Peloponnesian War* or as technically impressive as W. L. Langer's or A. J. P. Taylor's appreciations of nineteenth-century European diplomacy. Most books of "contemporary history" are neither, and, happily, they are soon forgotten. Laqueur's *Confrontation* is one of those easily forgotten – as easily forgotten, perhaps, as the newspaper accounts on which it is almost exclusively based. There is, in fact, only one section that sticks in the mind and that, a part of Chapter 3, was not written by Laqueur but by Major General (Ret.) Dr. M. Peled.

It is useful to consider General Peled's analysis first, for his is the only piece of writing in the book that rises above journalism and a fashionable pessimism. General Peled offered the heretical notion that the occupation of Sinai is a military liability for both the Israelis and the Egyptians. As proof he recalls what has happened to the armies that occupied that barren place, whether in 1956, 1967, or 1973. His argument is not only striking but fruitful, for at a stroke it explains some of the errors of Arab and Jew in regard to Sinai during the last twenty years. It also becomes a hard military argument in support of the idea that Israel's refusal to give ground in Sinai immediately after the victory of 1967 was a tragic political blunder, pregnant with hardship for Israel and the West.

In the remainder of the book there are many weaknesses and one strength. Three of the weaknesses will be mentioned. First, Laqueur is too quick to regard journalistic accounts as authoritative. One example of this must suffice, because of limited space, although many could be given. At the height of the 1973 war, when Israel had surrounded the Third Egyptian Army, the Russians sent a message to the Americans described variously as "brutal" and "urgent" by those who saw it. A few pages after he mentioned this ambiguous communication, Laqueur gave an exact description not only of it but of an ultimatum to the Israeli government from the U.S. allegedly based on the Russian note. The source of this controversial and vital information is given. It is an account in an Israeli newspaper, *Ma'ariv,* published less than a week after the ultimatum and what Laqueur calls Israel's "retreat."

A second weakness is even more damaging to the book. Throughout, Laqueur pays little attention to the influence of internal politics on the formulation of the foreign policies of the Arab governments, although he pays considerable attention to the effect of internal politics on Israeli policy. Laqueur suggests, for example: "The discrepancy between the extreme demands voiced by public opinion and what could be done in reality became a major problem in Israeli politics" (pp. 256–257). If this was so in Israel, was it not also a problem in Egypt, Syria, Jordan, Lebanon, Iraq and even in Saudi Arabia and Iran? To neglect the large literature on this subject and to fail to examine equally the role of internal politics on both sides is, in effect, to write history from the point of view of Israel.

A third weakness of the book is the author's liking for long, vituperative essays condemning Western Europe based on what are, essentially, unargued assumptions. Laqueur calls the European governments foolish, ineffective, cowardly, selfish, small, and amoral. He finds no major faults with U.S. policy (p. 275). The heart of his criticism of the Europeans is that they have refused to unite either with one another or with the U.S. In Laqueur's eyes, the wages of disunity are immorality, pettiness, bankruptcy, and eventually Soviet domination. One wonders. If Western unity is the key to lowering oil prices, solving the Arab-Israeli conflict, and pursuing a moral foreign policy, does the U.S. have the only and best path to that unity? Is it effective and sensible policy for the U.S. to pursue a course so alien to its European allies that the Alliance comes to be divided as easily and bitterly as it was by the oil embargo? Is it sensible for the U.S. to stand so far apart from its closest friends and then to berate them? Is there no morality in a limited appreciation of the merits of the Palestinian case? Is support for Israel and American policy the only morally admirable position to take in the Middle East? One wonders.

The strength of Laqueur's book, and it is not negligible, is that despite the flaws, Laqueur recognizes the ideal of objective history. Even though he writes from the point of view of Israel, he is able to criticize Israeli policy. In his view, for example, the Israelis mistakenly followed a policy of *immobilisme* after 1967 and were not magnanimous in victory (pp. 253–257). That is not much, but it is enough to keep the ideal of objectivity alive.

P. EDWARD HALEY

Claremont Men's College

Foreign Policy Analysis: A Comparative and Conceptual Approach. By Howard H. Lentner. (Columbus, Ohio: Charles E. Merrill Publishing Co., 1974. Pp. 295. $8.95.)

Professor Lentner has written a useful conceptual and comparative analysis related to the making of foreign policy. The comprehensiveness of the book is particularly impressive as the author examines innumerable propositions related to the actors in the foreign policy process and the international environment in which they operate. The author then proceeds to examine the determinants – both internal and external – of foreign policy choice, followed by a discussion of such foreign policy acts as decisions, mobilization of resources, and the application of instruments and techniques. The final chapters are concerned with policies and consequences.

While the many variables and propositions related to foreign policy decision making are impressive, one often gets the feeling that they provide little more than a checklist of factors affecting foreign policy with little effort to determine which are more important than others. Indeed, some of the propositions seem a little trite, but this is one of the hazards of propositional inventories.

The evidence used to illustrate the propositions is primarily anecdotal and is drawn from a variety of settings. Some of the propositions are stated without illustrative examples or references to empirical findings, while in other instances two or three pages of narrative are used to illuminate a point. The Suez crisis of 1956 and the Indo-Pakistani conflict are used most extensively in this connection.

Although the illustrations of specific events to document various propositions proved useful, the author might have relied a bit more than he did upon data-based studies which test hypotheses about the relative importance of various determinants. Admittedly, this literature is scattered and sometimes contradictory, but it is more systematic and preferable to shopping for isolated incidents to support a point.

For Lentner the two most important factors influencing a state's reaction to its external environment and situation are its capacity and its predisposition. A valiant effort is made to provide a gross measure of relative capacity for each state. The indicators used for this purpose are GNP, population, size, and location. In the nuclear-missile era the latter two factors may be somewhat questionable in some instances, just as population can be a benefit or an impediment to a state's capacity. More effort might have been made to justify the final index theoretically and logically. Once exerting so much energy in constructing the index, the author might also have related the capacity measures more systematically to foreign policy output in order to test the relative impact of capacity upon state behavior. The same might be said of the various typologies developed to indicate state predispositions.

A particularly useful aspect of the volume lies in the effort to develop models relevant to a variety of situations in order to enhance comparative analyses. This is done by presenting six abstract models of interaction involving differing numbers of actors and coalitions. Patterns of interaction are then represented graphically in terms of the instruments utilized, e.g., diplomatic, informational, military, and economic. The problem with such an analysis, however, is that it is primarily static. Thus it seems improper to have used NATO to illustrate one of the models, for the patterns of alliance interaction have changed considerably over the years.

Whereas the author pays considerable attention to the bargaining process involved in various types of decision-making units, the ability to mobilize resources, and the question of implementing decisions, little attention is given to the substance of policy. As a result, a vast literature is overlooked which attempts to relate internal and external determinants to specific policies of conflict and cooperation. In terms of the discussion of policies, the author also limits himself to a short chapter dealing with policies of insulation, engagement, and expansion with little effort to link the analysis with the earlier discussion of determinants. The final chapter concerned with consequences of foreign policy choice does add a useful discussion of the implications of foreign policy for domestic society in an effort to complete the circle between domestic and foreign politics.

The book is clearly an important addition to the literature on comparative foreign policy. Given its fairly abstract nature and limited empirical discussion, it is most likely to be effective at the graduate rather than the undergraduate level, so that the students' empirical grounding will better enable them to evaluate the numerous propositions presented.

LLOYD JENSEN

Temple University

Canada, The United States, and the Third Law of the Sea Conference. By R. M. Logan. (Montreal: C. D. Howe Research Institute and National Planning Association, 1974. Pp. x, 122. $3.00, paper.)

As the title suggests, this book is a straightforward survey of Canadian and U.S. policies with regard to the main issues being addressed at the UN Conference on the Law of the Sea. Six issue areas are covered in separate chapters: living resources, the continental shelf, the deep seabed, marine pollution, navigation, and marine scientific research. Each chapter describes the international law pertaining to the issue area, U.S. and Canadian official policies and the interests behind them, the history of bilateral relations, and current bilateral problems in the respective areas.

Only two weaknesses are notable in the study – one minor and one more significant. The minor consideration is the uneven treatment of the two national positions. Canadian policies and the assumptions behind those policies are sympathetically and thoroughly documented. The U.S. position, particularly as it relates to navigation of straits, is not. Whether or not the U.S. assumption that a high-seas corridor presently exists in international straits is correct, it is the basis for U.S. insistence on a right of unimpeded transit through and over international straits that will be overlapped by twelve-mile territorial seas. Thus to say that the U.S. "insists that the present right of innocent passage through international straits be replaced by a new right of free transit" (p. 27) is to misrepresent U.S. assumptions and hence policy.

Perhaps a more important defect of the study is the impression it leaves that there are few significant differences between the U.S.

and Canada in the larger negotiations about the law of the sea. The author does indicate the troublesome nature of certain bilateral problems. In dealing with policies in the international negotiations, however, he stresses the similarity of national interests and describes the differences as simply reflecting "variations in the relative significance the two nations attach to particular offshore interests" (p. 2). Unfortunately this approach does not account for a decade of troubled relations between the two countries over ocean issues and the strong sentiments felt in both governments against the policies of the other. While the two nations may in fact have very similar interests, it is the interpretation of those interests through the policy process that accounts for the very different thrust of Canadian and U.S. policies. This study does not consider the policy implications of Canada's strongly felt need to pursue an independent role in the negotiations, nor U.S. resentment of such independence on the part of a close and trusted neighbor.

This first book-length study of U.S.-Canadian law of the sea relations should prove of interest to government officials as well as to scholars. The volume covers the period up to Spring of 1974, that is, before the Caracas Conference. Although the U.S. position on offshore jurisdiction has been modified since the book went to press, the study remains useful and its conclusions valid in most respects.

ANN L. HOLLICK

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Radicalism in Mediterranean France: Its Rise and Decline, 1848–1914. By Leo A. Loubère. (Albany: State University of New York Press, 1974. Pp. 258. $20.00.)

Professor Loubère has written a study of the political evolution of the six Mediterranean departments of France, closely connected to the changing economic and social structure of the region. This comes down largely to a chronicle and discussion of the emergence of an extreme Left in 1848–49, its political quiescence during the Second Empire, the rise of radicalism thereafter, and its twentieth century decline before the challenge of socialism. Such developments take place against a background of viticultural expansion from the 1840s to 1869 which turned Languedoc into a vast wine factory, and of market crises thereafter. Loubère presents the *Midi*'s tendency toward the Left as a byproduct of modernization in the wine industry, and the politics he traces are reflections of the accidents that this industry encountered on its way.

The book is useful to students of French politics because it stresses those peculiar local aspects that tend to be ignored in more ambitious, hence more general, accounts of the Third Republic. This point of view is well documented, particularly in the latter chapters that cover the years 1881–1914 when – in a country dominated by moderate *Opportunistes* and *Progressistes* – the *Midi* challenged Paris as a nursery of Radical deputies.

Loubère shows the reality of unorganized political action, sparked by one man or a very few, and based either in a cafe or in the offices of a local newspaper. He notes the plethora of such gazettes, often competing against each other: each the mouthpiece and rallying point of what was more a local faction than a party, at a time when national parties existed largely as loose coalitions of local cliques and *seigneuries.* True, "national politics offered local Radicals a means of upward mobility not available in other professions" (p. 173). But localism was pre-emininent, local personalities were of the first importance, and particular (i.e., local) concerns dominant. In the Mediterranean South, the first concern was wine. The most successful politicians there were usually local men, often owners of vineyards, always staunch defenders of whatever policy suited vintners best.

In this context, one cannot miss the disparity between the general, generous programs drawn up in Paris and the specific platform planks of purely local concern. Likewise, a new reality distinguishes between national politics (the Boulanger crisis, the Dreyfus case, even the struggle between Church and State) which affected the *Midi* little, and those local affairs that so largely marked its politics. Finally, Loubère makes clear that, whatever the case elsewhere, a region where worker-owners were numerous and local economy at all levels was dominated by wine knew little class-consciousness among the "working class." Even when this appeared, along with strikes, early in the twentieth century, it soon gave way, as in 1906–1907, when more immediate common concerns cut across class barriers.

The wine crisis of the 1880s had brought the Radicals success as carriers of local demands and representatives of local protest. The wine crisis of 1906 caused them to decline, because by then Radicals had become the ruling party in the Chamber. Protest now had to be incarnated by Socialists, often with Radical, sometimes with right-wing support. As this happened, the Radicals, who once represented the more urban and economically progressive areas of the South, became more "moderate" and found new strongholds in the more backward and rural uplands. But many of the Socialists were no more socialist than many of the Radicals had been radical. In both cases, the labels covered the protest of local populations more concerned with their needs and frustrations than with doctrine or national policy.

None of this is really unfamiliar, but the particular tale has not been told before; and many of the details it contains (notably its sociological analyses) are new. Out of detailed local studies of this sort a truer picture of "modern" French politics is emerging: more diverse, more archaic, less modern too, than we have tended to think. Mr. Loubère's book is not the most polished contribution to this new edifice, but it is workmanlike and serviceable.

EUGEN WEBER

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The Political Uses of Sea Power. By Edward N. Luttwak. (Baltimore: The Johns Hopkins University Press, 1974. Pp. 79. $6.50, cloth; $2.25, paper.)

International relations specialists generally agree that the global system is now undergoing a highly complex transition from one to another historical epoch. Precisely when the new era will settle into clear-cut patterns, and exactly what the configuration will be, are matters for considerable speculation. Meanwhile, one thing seems evident: While many nations continue to seek or to maintain military forces ranging in size from substantial to massive, systematic constraints and inhibitions against using such forces in actual open warfare grow steadily stronger. The upshot is a paradoxical situation, perhaps lasting years into the future, when the aim of the game for many nations will be to win wars without firing a shot, or, at worst, very few small nonnuclear shots. In other words, we are talking about a new emphasis on the political and psychological uses of military forces short of overt combat.

In this new situation now emerging, as long as nations persist in wanting to use military force on each other as a key element of persuasion, but ideally without firing any shots, a whole new kind of thinking is needed to guide foreign and military policy making. An enormous research gap must be filled if the new modes of warless warfare are not to degenerate into devastating massacres. Edward N. Luttwak, staff member at the Washington Center of Foreign Policy Research, The Johns Hopkins University, has made a major start toward bridging this gap with this brief but brilliant book. It is an essay likely to become an enduring classic in the literature of military strategy and tactics at the grand level.

The Luttwak book is a rarity in several respects. First, it delivers substantially more than the title promises. It deals, it is true, with the effects of rattling naval sabres in various ways, but only because warships have more inherent capabilities for nonshooting missions of "suasion" than do other forms of military force. This is not simply "gunboat diplomacy" reinvented. The book sets forth fundamental definitions and conceptual frameworks for thinking about the political uses of *any* kind of military force which are likely to serve as the basic terms of reference in this context for years to come.

Another respect in which this book is a delightful rarity is the author's crisp prose. Incorporating perceptive subtleties and deft nuances within a sparse economy of words, Luttwak achieves an understated elegance of language while packing great range and rigor into only seventy-nine pages. He inductively draws on historical cases from Athenian naval policy prior to the Peloponnesian War, up to and including the naval policies of several nations involved in the Yom Kippur War of 1973. Luttwak certainly has not said the final word on his important subject, but this seminal work has put down the pretheoretical foundations for many areas of new research suggested by stimulating ideas on almost every page. Since Luttwak is the pure scholar – and a quite modest one, with no evident interest here in polemics designed to establish himself as the court strategist for any regime anywhere (thus another noteworthy difference between this author and many writers on military strategy, perhaps he will pursue some of his own provocative leads.

The book ought to be read and reread by all specialists in foreign policy, military studies, diplomacy, and international politics more broadly. It has gaps, weak spots, and an inconclusive ending, but these shortcomings can be easily excused in a work which constitutes such an impressive beginning in relatively virgin territory. If by a happy irony something called peace should be initially approximated by the consistently successful practice of warless warfare, Luttwak would deserve some of the credit.

VINCENT DAVIS

University of Kentucky

Makers of American Diplomacy: From Benjamin Franklin to Henry Kissinger. Edited by Frank U. Merli and Theodore A. Wilson. (New York: Charles Scribner's Sons, 1974. Pp. 728. $17.50.)

This volume consists of twenty-five essays by twenty-seven contributors (all historians except for two political scientists) on twenty-five mostly major makers of American diplomacy. The majority of the authors received their Ph.D.'s in the late 1950s and 1960s, although several older scholars also contributed to the collection. While the editors assert that no one interpretive view prevails, the essays largely reflect current liberal conventional wisdom as it has developed under the impact of the Vietnam trauma. As a result, an apologetic and at times even masochistic mood permeates much of the work, and many specific judgments are flawed by overreaction to America's misadventure in Indochina.

In the introduction, the editors state their intention of showing the role of elements of personality of individual American statesmen in shaping events. Nevertheless, they go on to say that, "two hundred years have wrought amazingly few changes in the essentials of foreign affairs – a surprising statement unless one reflects on the basic components of the world views of Alexander Hamilton and Henry Kissinger. Survival in a hostile international environment has remained the touchstone, and the options by which survival is to be ensured appear to be as restricted and fraught with peril today as in the nation's formative years" (p. xvi).

I shall comment on the most interesting of the twenty-five essays – the five dealing with Franklin, Hamilton, Jefferson, Madison, and John Quincy Adams, and the twelve pieces on late 19th- and 20th-century figures from Mahan to Kissinger.

The first five essays revolve to a considerable extent around the eternal argument between idealists and realists. Cecil Currey suggests that Franklin was both idealisitic and realistic in his diplomatic attitudes and actions. Currey says: "It may be that Benjamin Franklin's great contribution in the field of diplomacy was in the realm of ideas rather than on the field of action" (p. 20). Currey writes that Franklin perceived the relevance of isolation and distrust of other nations to the young Republic's strategic position. But Franklin also propounded the idea of a union of countries (as well as of colonies) for their common welfare and security, and stressed the importance of strengthening international law.

Ian Mugridge's essay on Hamilton and Lawrence Kaplan's on Jefferson clearly contrast the realist and idealist views of those statesmen, while as is customary allowing for Jeffersonian realism in practice. Hamilton, of course, counseled the need for attention to "good faith and justice" as well as to the national interest. Jefferson, on the other hand, clearly perceived and acted upon the classical balance of power approach in his posture during the Napoleonic Wars. Kaplan concludes his essay: "A characteristic American approach to international relations has been the casting of national interest on a moral base; Jefferson's contribution was to shape this conceit and to seek a relationship with the external world that followed it." Incidentally, neither Mugridge nor Kaplan have novel insights on Hamilton and Jefferson. Patrick White's piece on Madison retells the well-known story of the latter's idealism, and Norman Graebner's essay on John Quincy Adams reminds us of Adams's balanced approach. Neither White nor Graebner tells us anything new.

Turning to the second cluster of essays, Kenneth Hagan's piece on Mahan and Eugene Trani's essay on Theodore Roosevelt are both solid. Trani in particular has resisted any temptation in the wake of Vietnam to exaggerate TR's "imperialist" faults. Ross Gregory in his contribution on Wilson submits that however excessive Wilson's idealism was, he did contribute to preserving the European balance of power through intervention in 1917, and also held up a vision of world order which could one day be realized. William Kamman in his essay on Stimson concludes with an important warning in the wake of Vietnam: ". . . hopefully the architects of change in United States foreign policy will not exaggerate the virtues of withdrawal, weak executives, and the lack of idealism. . ." (p. 428).

The last eight essays deal with leaders who performed during the maturity and "awareness" of many readers, i.e., FDR, Truman, Acheson, Kennan, John Foster Dulles, John Kennedy, Fulbright, and Kissinger. Theodore Wilson and Richard McKinzie, while not unfair in their very critical view of FDR's foreign policy blunders, are guilty of some cheap shots at the man's character and competence. John Lewis Gaddis's piece on Truman reflects the balance found in his much praised *The United States and the Origins of the Cold War.* But it needs to be said that Gaddis is too "Martian" in this essay as well as in his book. He has obviously never had any first-hand experience with Soviet foreign policy.

Robert Ferrell and David McClellan in their piece on Acheson are among those victimized by overreaction to Vietnam. They assert that Acheson was too tough on the Russians. They fail to acknowledge Acheson's role in the Truman Administration's inadvertent invitation to North Korean aggression in 1950, although they correctly note that Acheson did not warn against American forces moving toward the Yalu after Inchon. Thomas Paterson's piece on Kennan is flawed by an attack on Kennan's character, and is also too critical in appraising Kennan's understanding of Soviet foreign policy. Paterson like Gaddis is too charitable toward the USSR's role in the world. Herbert Parmet's piece on Dulles is a welcome, balanced view of a narrow but much maligned man.

Robert Divine's treatment of JFK is a compendium of most of the current liberal cliches about that so-called "Cold Warrior." David Trask's essay on Fulbright is excessive in its praise, although an interesting account of the man's intellectual development and political role. The final piece – Michael Roskin's essay on Kissinger – is easily the worst in the collection. Roskin has apparently not read much of the literature judging by his brief bibliographical note, and perhaps accordingly is largely wrong in his understanding of the roots of Kissinger's thinking. Bismarck – not Metternich – is HK's major mentor. Roskin, like Kissinger, is also much too optimistic in his view of the "successes" of *détente.*

It is questionable whether this volume adds much to our knowledge of American diplomacy and some of the men who have made it. The book is also, as noted many times, flawed by the influence of the Indochina debacle on the present outlook of most of the contributors. But it is reassuring to know that most of the young authors of these pieces understand that "survival in a hostile international environment" is a basic challenge to American foreign policy makers now as in the past. And it is also comforting to know that the temporary vogue of the New Left is apparently already *passé* among at least most of this group of young diplomatic historians in the U.S.

DONALD BRANDON

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Reflections on the Cold War: A Quarter Century of American Foreign Policy. Edited by Lynn H. Miller and Ronald W. Pruessen. (Philadelphia: Temple University Press, 1974. Pp. 207. $10.00.)

The war in Vietnam served one useful purpose. It stimulated a reassessment of the assumptions upon which American foreign policies have been based since at least World War II. The roots of these policies, upon examination, almost invariably lead back to the cold war. But what was the cold war? How did it come about? Were there alternatives? Answers which to many were self-evident in the 1950s grew increasingly inadequate from a later perspective. Unfortunately, those assaults on the conventional wisdom which attracted the widest attention during the 1960s merely substituted new myths for old ones. If traditionalists often appeared as counsels for the defense of American diplomacy, the revisionists served as prosecuting attorneys. Their "searing indictments," as the blurbs used to say, provided titillation in the heady atmosphere of the 1960s, but not much else. Indeed, some of the more celebrated or these works (e.g., Gar Alperovitz's *Atomic Diplomacy,* for instance, or Barton J. Bernstein's early writing on the same subject) now stand as *curiosa;* embarrassments which even their authors are unwilling to defend. At the same time more serious revisions of existing interpretations were being developed which compel attention, whether or not one agrees with them. The present volume, consisting of papers read during a lecture series in 1970–1971, offers a convenient sampling of both types.

Three of the essays ought to be read by anyone interested in the subject. Stanley Hoffman, after a perceptive review of the existing scholarship on the origins of the cold war, puts forward his own analysis. He emphasizes the degree to which both the United States and Russia misconstrued the intentions of the other, and how these misunderstandings shaped their own policies. Norman B. Hannah, the only contributor who can be said to begin from an orthodox position, tries to show why the cold war evolved differently in Asia and Europe. In the latter area, he points out, a basic equilibrium respected by both sides was achieved fairly quickly. Not so in Asia, which helps account for the dangerous struggle still going on. Richard A. Falk stresses the similarities between the United States and the Soviet Union as counterrevolutionary powers, and probes the interaction between American foreign and domestic policies. Though prone to the facile equations (Attica = domestic policy, My Lai = foreign policy) one expects from a Mary McCarthy or Gore Vidal, Falk's is a stimulating essay.

Harrison E. Salisbury and Walter LaFeber offer provocative interpretations of more limited subjects. In an admittedly speculative paper, Salisbury suggests that the North Korean invasion of South Korea (directed by Stalin) was an early manifestation of the Sino-Soviet split which American policy makers failed to perceive until much later. Instead, Truman and those around him interpreted the move as a thrust by monolithic communism against the "free world." LaFeber also focuses on the Korean war, more specifically on the American decision to cross the 38th parallel. This decision, according to LaFeber, "marked the moment when the United States decided to move from a policy of 'containment,' as defined by President Truman in 1947, to a policy of 'liberation,' or the rolling back of Communism" (pp. 74–75). Together with what he refers to as "diplomatic offensives" in Europe and elsewhere, LeFeber sees this move as a major turning point in the cold war. Implicit throughout his prose is the notion that the Korean war provided the Truman administration with a convenient excuse for what it wished to do all along. Perhaps, but LaFeber's case is not persuasive.

The other chapters in this book are not as rewarding. Barton J. Bernstein offers an account of the Cuban missile crisis which differs from existing ones only in the degree of culpability he assigns to Kennedy. Since there is plenty of blame to spread around, one wonders why Bernstein felt the need to involve himself in tortured explanations of how the Soviet Union did not technically lie in announcing it had "no need" to place missiles outside Russia, at a *time in fact it* was placing them in Cuba (p. 114). The conclusion of Richard J. Barnet's essay is that the arms race "is now independent of the Cold War and has a life of its own" (p. 159). True enough, but Barnet fails to tell the reader how and why this has happened. Instead he gives a selective, sketchy account of the cold war's evolution. Ronald W. Pruessen selects as a case study the American response to economic crises in Greece, Turkey, and Western Europe in 1947, and comes to the unremarkable conclusion that the response was often stated in economic terms. Lynn H. Miller's chapter on the United States and the UN consists of little more than a roster of American sins. Miller condemns the American response to Korea as a violation of the spirit if not the charter of the UN, for instance, but reports without comment Russia's threat to atomize the British, French, and Israelis in the Suez area in 1956. Presumably this is because the latter situation "was generally viewed in the rest of the world as the kind of aggression that the UN was specifically designed to prevent" (p. 171). So it goes.

Reflections is a mixed bag, as are most anthologies. Already dated in some respects – several contributors appear to have believed that campus upheavals portended revolution and civil war at any moment – it provides a good introduction to cold war revisionism.

ROBERT JAMES MADDOX

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Law and Civil War in the Modern World. Edited by John Norton Moore. (Baltimore: Johns Hopkins University Press, 1974. Pp. 648. $22.50, cloth; $8.95, paper.)

This book incorporates twenty essays and five comments, the outcome of a Civil War Project undertaken by the American Society of International Law. Most contributors are Americans (some hail from the United Kingdom), and, inasmuch as the project was conceived and implemented at a time when America was traumatized by the Indo-China War, the whole volume seems to be haunted by the specter of Vietnam. A civil war, like a case of drowning, may be observed either from the viewpoint of one submerged or from that of another rushing to the rescue and resentful of being sucked into the whirlpool. This book looks at the problem through the eyes of the latter. The term "intervention" appears in the very titles of ten out of the twenty essays (not to mention the bibliographical list). There are even contributions scarcely relevant to civil wars as such (e.g., "Selected Constitutional Issues Arising from Undeclared Wars," by Lawrence R. Velvel, pp. 190–214), which can only be explained in the specific context of American involvement in Indo-China. Perhaps the volume should have been designated "Foreign Intervention in Civil Wars."

As pointed out by James N. Rosenau, the literature on intervention is "long on moral judgments and short on analytic insights" (p. 129). Gaining such insights, and conveying them to their fellow scholars, is obviously the desire of practically all participants in this project. The task, however, is difficult if not impossible. Suffice it to note that they themselves are apparently underwhelmed by the cogency of each other's argumentation. In the rather trenchant comments, Tom J. Farer contends that John Norton Moore (the book's editor) "has failed to comprehend the problem of intervention" (p. 549), whereas Moore responds that Farer's reasons are given "with his usual confusion" (p. 565). There is also a sharp exchange on the controversial issue of humanitarian intervention between Ian Brownlie (pp.

217–228) and Richard B. Lillich (pp. 229–251). On the whole, one is left with the despondent conclusion that, in Jerome Alan Cohen's words, intervention is "a murky concept" and "a slippery vehicle" (p. 349).

Scholars, of course, rarely agree with one another, particularly in the fields of political science and international relations. Nothing political is conducive to the consolidation of an exact science. And the question of intervention is, in all likelihood, among the most volatile in international relations. The merits or demerits of intervention are usually debated against a given political background, and many a scholar is willing to shift his position when the *dramatis personae* in the international scene change. To adduce Stephen M. Schwebel's example: Hitler wanted to unite the German-speaking inhabitants of the Sudetenland with Germany; Nehru wished to unite the Goans with India (p. 457). Yet, it is all too easy to condemn the one while applauding the other.

Intervention raises many problems, ranging from desire and capacity to intervene (on the difference between desire and capacity in this respect, see Oran R. Young, p. 112) to the legality of intervention. Under current international law, foreign intervention in a civil war is permissible (if at all) only when requested by, and extended in support of, the constitutionally lawful government fighting to suppress a rebellion (see Richard R. Baxter, p. 524). Any attempt on the part of a foreign country to aid and abet the rebels is viewed as unlawful intervention in the domestic affairs of the local state, if not as an outright act of aggression (p. 525). The trouble is that a civil war may evolve in such a way that one can no longer tell which is the lawful government of that state and who is rebelling against whom.

The crux of the issue of intervention, however, lies elsewhere. All too often, intervention generates counterintervention, and gradually local issues become the concern of the whole world. In the nuclear era any major conflict threatens to be "a war to end all wars" in an apocalyptic sense which would have been incomprehensible to previous generations. The challenge, as enunciated by Cyril E. Black, is "the construction of a system of world order within which the unrest within individual countries can run its course without becoming a source or an excuse for international conflict" (p. 69).

But a principle of nonintervention – even given the debatable assumption that it can be strictly and impartially implemented – is not a panacea. The tremendous importance of the issue of intervention must not eclipse the phenomenon of civil wars. Civil wars may take place without serving as a vortex for foreign intervention. Nevertheless, the struggle between the exclusively local forces is liable to produce devastating results. Is it necessary to belabor the point that the United States suffered more casualties in its Civil War than in all its wars with other countries (prior to Vietnam) combined?

If we are really addressing ourselves to the question of law and civil war in the modern world (the ostensible subject of this volume), we cannot escape the conclusion that the situation is paradoxical. On the one hand, the prohibition of wars between nations constitutes the cornerstone of contemporary international law. The launching and waging of an interstate war of aggression is not merely illicit: it is a crime punishable by international law (*vide* the Nuremberg judgment and verdict). On the other hand, international law does not forbid civil (intrastate) wars. Evidently, the internal constitutional law in each state interdicts any attempt to remove the local government by force. But international law as such remains indifferent to the outbreak of a civil war.

Admittedly, international law is not entirely indifferent to the conduct of hostilities in a civil war. Under Article 3, common to the four Geneva Conventions of 1949 for the Protection of the Victims of War (frequently referred to as a "Convention in miniature"), certain minimal humanitarian obligations are imposed on parties to an internal conflict, provided the level of hostilities is such that one can talk of an armed conflict as distinct from minor incidents. This *ius in bello interno* is profoundly examined by Richard R. Baxter (pp. 518–536). Efforts are now in progress to revise the law and expand the protection granted to the victims of civil wars.

The cardinal point still is that there is no international *ius ad bellum* pertaining to civil wars. We cannot even detect germinal rules as a precursor of things to come. The difficulties are self-explanatory: "confronting change with orders to preserve the status quo" (to quote John W. Burton, p. 110) simply will not do. Yet, international law cannot afford the luxury of overlooking wars merely because the antagonists do not cross any international frontier. At a time of growing interdependence, particularly within regional groupings, the concept of an impenetrable frontier is just outmoded. Modern international law has encroached upon what used to be regarded as the "reserved domain" of states at so many points. Why should it flinch from making the attempt to regulate internal violence?

This challenge to orthodoxy in international law remains to be met.

YORAM DINSTEIN

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The United States and the Caribbean Republics, 1921–1933. By Dana G. Munro. (Princeton, N.J.: Princeton University Press, 1974. Pp. 394. $17.50.)

Whether or not the United States can be said to be playing an imperial role in the Caribbean today, there is no doubt, of course, that it did play such a role from the Spanish-American War until the 1930s. Dana Munro, Professor Emeritus of History at Princeton and a former foreign service officer stationed in the Caribbean and in the Latin American Division of the State Department in the 1920s, has written his second major diplomatic history of that period, drawing upon his own experiences as well as official government records. His argument, in effect, is a qualified, moderate defense of the U.S. imperial role.

According to Munro, the 1920s should be seen as a transition period from the open, unabashed military interventionism of the previous decades to the more far-reaching noninterventionism of the Good Neighbor Policy. To be sure, the main thrust of U.S. policies during the 1920s continued to be the discouragement of revolution and general instability, based on the traditional premise that "disorderly conditions" (as Munro puts it) invited European intervention which could threaten vital U.S. interests in the area, particularly the Panama Canal. By 1921, however, growing anti-Americanism in Latin America, as well as domestic U.S. opposition to military intervention, induced the State Department to attempt to realize its objectives by different means.

The main body of Munro's book consists of highly detailed descriptions – sometimes fascinating, sometimes just wearying – of the U.S. role in Nicaraguan, Cuban, Haitian, and Dominican politics. Throughout the narrative run three major arguments. First, the United States did not deliberately seek permanent, imperialist control in the Caribbean, but rather sought only to "stop disorder," "correct conditions" that might lead to internal uprisings and external interference, and "promote orderly republican government" (pp. 9, 23, 10). Second, contrary to appearances, the United States did not favor dictatorship in its pursuit of stability but on the contrary believed that real stability could only be achieved after the establishment of democracy. Indeed, Munro argues that if anything, Washington could be criticized for excessive activism on behalf of free elections: "I think we oversimplified the problem, but we could hardly be accused of not being interested in the development of democratic institutions" (p. 381). Apparent examples to the contrary, such as U.S. acceptance of the Machado regime in Cuba and of the budding totalitarianism of Trujillo in the Dominican Republic, do not reflect U.S. satisfaction but only a growing reluctance to actively intervene – or re-intervene – in the internal politics of those unfortunate countries. Finally, U.S. policies were in no measure inspired by a desire to support U.S. private investment in Caribbean countries. The State Department frequently blocked private loans and investments when it considered them to be politically unwise or potentially exploitative of the local peoples, and when it did back such business activities, it did so in support of its own political objectives rather than corporate profits.

How convincing are these arguments? As in his earlier works, Munro is entirely persuasive in refuting neo-Marxian interpretations of U.S. policies. He is considerably less persuasive in arguing for the benevolence of U.S. political purposes in the Caribbean, and insufficiently thoughtful about the ultimate *consequences* of U.S. activism, whatever the intentions. The State Department sought democracy in the Caribbean only as long as it didn't conflict with stability, which had higher priority; hence its unwillingness to use its considerable influence to force change upon "stable" repressive regimes throughout the area, even where such regimes had come into power as a direct consequence of previous U.S. interventions, as had the Somoza and Trujillo dictatorships. And while Washington did *not* consciously and purposefully set out to create an imperium in the Caribbean, it can hardly be denied that the consequences of its repeated interventionism, political and economic as well as military, were remarkably similar to the more open nineteenth-century imperialism of European powers in Asia and Africa. From Munro's narrative vividly emerge all the familiar patterns: the well-intentioned but infuriating paternalism, the myriad of good works in health, education, construction, etc., the appreciative and cooperative elites whose political and economic fortunes are tied to the metropole, but on the other hand the growing resentment and nationalism that eventually led to disengagement, or as others would have it, the development of more subtle forms of indirect control.

Perhaps most seriously, Munro completely accepts and repeatedly asserts (with little supporting argumentation) the underlying premise of U.S. policy in the Caribbean, then and now: that the area is indeed "vital" to U.S. "national security," and that as a result political instability there is of direct concern to the American government. In the nuclear era this article of faith is increasingly being questioned, but even in earlier times its validity is far from obvious, at the least requiring analysis rather than assertion.

Still, Munro has produced another well-written, scholarly history, providing much grist for many a mill.

JEROME SLATER

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Controversial Subjects of Contemporary International Law: An Examination of the New Entities of International Law and their Treaty-Making Capacity. By Chris N. Okeke. (Rotterdam: Rotterdam University Press, 1974. Pp. 243. $20.00.)

Systematically arranged in seven parts, this volume discusses the way in which different entities can participate in international law. Throughout the book Dr. Okeke takes account of the fact that international relations are no longer a matter of states alone and that increasingly, international legal contacts are established through other channels. According to Okeke the essential element of international personality is vested in new entities not by international law but by the facts of international life (p. 19). A construction of international law as a purely interstate system is outdated. Apart from states, the study covers: members of a federation, the Holy See, Rhodesia, national liberation movements, Bangladesh, Biafra, intergovernmental organizations and nongovernmental organizations (including corporations). Although recognizing that their status is inferior to that of states, Okeke finds for each of them grounds to demonstrate that they have sufficient legal personality to participate in international law. This conclusion means that recognition by (other) states is not decisive for the establishment of new legal persons. Okeke therefore accepts that *inter alia* Rhodesia possesses the essential criteria for statehood. This does not, of course, preclude collective action against it.

When a popular uprising is evidently supported by a wide majority of the people concerned, as in the cases of Bangladesh and Biafra, a new person comes into existence under international law, as soon as other states become involved (e.g., by supplying arms, by humanitarian help or by recognition, see p. 158). A continuation of assistance to the "legitimate" government may then become an unjustified intervention by third states (p. 142). Okeke accepts the principles of "territorial integrity" and "nonintervention" to the extent that no state should encourage or exploit rebellion inside another state. But if an insurrectionist movement has acquired sufficient force and stability, other states are bound to recognize its legitimacy. In the last resort, self-determination – even in a noncolonial setting – should be placed above territorial integrity (pp. 177 and 227). For each of the subjects Okeke gives a survey of the positions taken by authors and governments which are most involved. In particular the thorough investigations of the Soviet literature are of great interest (Okeke studied in Kiev for six years before he came to the Netherlands, where he wrote his book).

Sir Gerald Fitzmaurice ably typified the book when writing in its foreword: "It is a work that cannot fail to provoke thought, written positively yet with restraint, it makes a balanced presentation of the various topics that the author deals with, and comes to conclusions that are defensible even where controversial." In most cases, I would even submit that Okeke's conclusions are on the more defensible side of the controversies.

HENRY G. SCHERMERS

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The Limits of Law. Edited by J. Roland Pennock and John W. Chapman. (New York: Lieber-Atherton Inc., 1974. Pp. 276. $8.95.)

This collection of essays is based upon papers originally delivered at the meeting of the American Society for Political and Legal Philosophy in Chicago (December, 1970), though revisions have been made and further contributions added.

"The limits of law" is a peg upon which theoretical and empirical generalizations of many kinds can be hung.

(1) Ethical Limits: Two essays launch a sapping operation against "paternalism." Legislation that might appear to be unjustifiable unless one accepts the right of the law-maker to coerce us for our own good can, it is argued, be justified by the harm-to-others principle. Michael D. Bayles (chap. 12) achieves this by assuming that only direct invasions of liberty require justifying at all. If drug taking is punished, that is paternalism and unwarranted;

but if the drug seller is punished, it is done to prevent him harming another, so paternalism is not involved. The consumer whose supplies are cut is to be told presumably, that no one is inferfering with him. Donald H. Regan (chap. 13) achieves an even more spectacular analyzing-away of the problem: a motorcyclist who fails to wear a crash helmet can be regarded, he says, as a different person from that later entity whose skull is cracked; consequently, he is *one* who harms *another* and so is caught by the harm principle.

A more substantial contribution to the ethical debate is made by Alan Dershowitz (chap. 9), whose paper receives inadequate criticism from Stephen Wasby and Martin Golding (chaps. 10 and 11). Granted that preventing harm is the principal justification for interfering with liberty, first, who is to decide what constitutes harm, and secondly, by what procedures and by what standards of proof is the occurrence of harm to be ascertained? Liberals usually allocate the first role to democratic legislatures and the second to due-process-observing courts. Dershowitz points out that, in the context of civil commitment of the mentally ill, the American legal system allocates both roles to psychiatrists. Once a psychiatrist is able to diagnose illness, a man will be interned if the psychiatrist adds that he is likely to be a *danger* to himself or others. Comparison might have been made with the case of the political terrorist: if he is interned without having already committed a harm defined by the legislature and found to have occurred, beyond reasonable doubt, by a jury, the sacrifice of libertarian values is at once recognized.

(2) Functional Limits: Michael A. Weinstein (chap. 7) suggests that whichever out of three functions of law we take to be primary – exploitation, regulation, or creation – its achievement must be limited by reference to the other two. Sergie Cotta (chap. 6) tells us that law is the ought-system centered in the principle or value of justice, that it limits politics whose overriding value is solidarity, but is limited by ethics whose overriding value is love. It is not clear how these phenomenological insights bear on practical issues.

(3) Limited Compliance: Two essays discuss the extent to which recent controversial decisions of the Supreme Court have been obeyed. Victor G. Rosenblum (chap. 17) takes the evidence to show that the Court is not as limited, as a vehicle of reform, as was previously thought. Graham Hughes (chap. 8), on the other hand, concludes that it should exercise caution.

(4) Limited Effectiveness: Law may be complied with, but its goals not achieved. How are we to measure goal success? Three essays consider this question, and criticize recent empirical research about the effectiveness of the law for failing to grapple with it. Kenneth M. Dolbeare (chap. 14) suggests that the effectiveness of law should be measured in terms of a conceptual framework, such as the crisis-of-legitimacy thesis. Jerome Hall (chap. 15) substitutes as the test of effectiveness the extent to which law maximizes certain kinds of values. Hugo Adam Bedau (chap. 16) correctly argues that the first thing to do is to isolate law as a variable relevant to social constancy and social change from among the complete set of change-relevant variables; after which it is likely that we will find that there is such an enormous variety of relevant phenomena that we will repudiate all current large-scale generalizations about the law's effectiveness.

(5) Inherent limits: Are there limits to the achievement of any goal resulting from the nature of law or the nature of men? The first five essays may be seen as dealing primarily with this question. Julius Cohen (chap. 3) and Kent Greenawalt (chap. 5) concentrate on the legal end: legal regulations are general rather than particularized; they are often vague, and the assessments of fact to which they have to be applied are often uncertain; further, legal enforcement always entails cost, which has to be set against the achievement of any goal. David J. Danelski (chap. 1) considers the human end also: in assessing what law can achieve, we must have regard to the many factors governing the moral and legal socialization of the maturing individual. William Leon McBride (chap. 2) suggests that, though limits of this sort may have applied in the past, they might not in a desolate future society armed with manipulative techniques, bugging, data collection and so on.

June Covin Tapp (chap. 5) summarizes a mass of fascinating research about psychological reactions to law and other rule-systems, particularly among the young. Her principal empirical contention seems to be that there is nothing in the psychology of the human animal which prevents him from reaching a stage where he will make judgments about "legality" not in terms of rules of positive law alone, but by reference to other rule-systems and general principles of justice and compassion as well; although it is the case, apparently, that only a few blessed individuals actually reach this stage. With this contention she couples a highly controversial philosophical prescription: we ought to educate ourselves and others to assess legality in this "postconventional" way. To adjudge law-violating conduct "legal" by reference to some extra-legal standard may seem attractive in the case of an underprivileged deviant; but where is a line to be drawn to prevent comparable appeals by lawbreakers in high places to their own extra-legal standards?

This book has something to offer to readers with a wide variety of interests; but, because of the diversity of issues implicit in the title, many will wish to read it selectively. There is not a great deal in the way of original philosophic discussion. Perhaps the most useful essays are those, such as chapter nine, which investigate the bearing of recent empirical research on traditional questions of legal philosophy.

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The Soviet Foreign Trade Monopoly: Institutions and Laws. By John Quigley. (Columbus: Ohio State University Press, 1974. Pp. 256. $15.00.)

There are a number of reasonably scholarly works on Soviet foreign trade now in print. This, however, is the first to focus specifically on the monopoly "issue" associated with the Soviet system of foreign trade. Professor Quigley directs his study toward explaining Lenin's key role in the decision to create a state foreign trade monopoly, the cyclical nature of the degree of monopoly control through Soviet history, and the relative efficacy of both the concept and implementation of the system during Soviet industrial and commercial development. The final chapter, devoted mainly to a review of Soviet and East European criticism – both implicit and explicit – of the contemporary usefulness of the monopoly, ends this short but complex volume on a note of substantial uncertainty.

But it is not for this somewhat strained focus on the monopoly issue that the book deserves a wide readership within both the scholarly and business communities. This is an important study because, better than any other now available *anywhere*, it provides detailed insight into what American businessmen call the "purchasing influences" of the Soviet market – the institutional and behavioral patterns which affect their international buying habits. American corporate executives are continually puzzled that even relatively small sales to the Soviets require months and sometimes years of intermittent negotiations before a contract is signed. They would be rather less mystified after a careful reading of this book. As described by the author, the political and bureaucratic labyrinth through which an order for foreign sale or purchase evolves is tortuous and fraught with pitfalls. The Soviet industrial end-user, for example, is far removed and insulated from the Soviet foreign trade organization. Separated by layer upon layer of bureaucracy, the Soviet end-user and the actual monopoly FTO (combine) purchaser cannot easily signal each other. The numerous "middlemen" include the end-user's industrial trust and ministry, specific agencies of *Gosplan* and *Gossnab*, the All-Union Council of Ministers, an assortment of other state committees, special units like the *Gosbank* and *Vneshtorgbank;* and within the Ministry of Foreign Trade, the monopoly trading combine is subject to particularly heavy-handed guidance from at least one geographically organized "administration" and one product-related "main administration." The author's major contribution to new knowledge is his detailed legal and operational description and – sometimes – analysis of the historical interplay between all of these agencies.

After noting that this is a good and useful book for specialists and knowledgeable laymen, it is incumbent upon the reviewer to point out a number of weaknesses. First, and most serious, this book bears an uncanny resemblance to contemporary Soviet-authored works on similar subjects, in its almost complete lack of critical analysis. Only in those passages relating to Lenin's role in the establishment of the monopoly, and the final chapter dealing with contemporary Soviet and East European criticism of the system, does the study show evidence of a true analytic approach. Even then the critique remains primarily within the Soviet frame of reference. This is unfortunate from a social science standpoint, though in fairness it should be noted that the author is a professor of law, not a political scientist.

Second, and closely associated with this "laundered" approach, the author provides solid historical-political reference points only when dealing with the in-fighting during Lenin's brief period as chief Soviet policy maker. Yet foreign trade policy and its various institutional frameworks have been substantial political and economic issues through all of the succeeding eras of Stalin, Malenkov, Khrushchev, and Brezhnev. None of these leaders or their policy inputs are mentioned except in passing. In fact one looks in vain for Khrushchev's name in the book's index.

Which leads to a final point: As in most Soviet scholarly works, the index is virtually nonexistent. This may not be the fault of the author; nonetheless it is a major fault. The lack of a real index makes reference use of this work particularly irritating, and yet it is ultimately as a sourcebook and roadmap that its chief value is to be derived. Publishers should not be allowed to get away with such a disservice, nor should authors allow it to happen.

ROBERT W. CLAWSON

Kent State University

Air, Water, Earth, Fire: The Impacts of the Military on World Environmental Order. By Ruth B. Russell et al. (San Francisco: Sierra Club, International Series No. 1, 1974. Pp. vii, 71. $2.00, paper.)

This collection of essays adds significantly to the belatedly expanding literature on political aspects of accelerating ecological disruption of the human habitat. Ruth B. Russell (editor as well as contributor) summarizes the "nature of military impacts on the environment." Arthur Westing (plant biologist) reviews disruptive ecological impacts of American military operations in Indochina. Herbert Scoville (military applications of science and technology) reaches beyond the consequences of thermonuclear war, to demonstrated hazards deriving from nuclear weapons testing, including allegedly safe explosions underground. He also goes after the nuclear power industry, a byproduct of military-oriented research and development. He gives special attention to the drive for "breeder-type" reactors which produce quantities of plutonium, one of the most dangerous materials man has invented, and its radioactive residues which have a half-life of some 25,000 years – i.e., eternity, in the context of human affairs. To date fission reactors, especially breeder-type, have exhibited a dangerous propensity to mechanical failure, with release of fallout into the environment. Furthermore, they present an unsolved (and possibly unsolvable) problem of how to keep their lethal residues out of the environment. In any case, increasing dependence upon energy generated by nuclear fission portends high risk of catastrophe, in the absence of a political order that is proof against sabotage, terrorism, human failure, and recurrent large-scale war. I question what prospect there is for achieving domestic and international order even remotely approaching the stability necessary to make fission-produced energy even marginally safe? One wonders how many political scientists have put this question on their teaching and research agendas.

Susan Zolla (pathologist) and Michael McClintock (physicist) describe the hazards of biochemical weapons. They also emphasize American resistance to tighter restrictions, the difficulty of getting military compliance with existing restrictions, and the Pentagon's defense of these weapons as second-strike deterrents – an argument that all sides can use to block any effective control of further development and stockpiling.

Edith Weiss (climate and weather modifications) reviews American efforts to gain military advantage at high environmental cost by inducing excessive rainfall in Indochina. She also exhibits the tip of the iceberg of largely clandestine research to master larger scale climatic control – a development which the late John von Neumann predicted (1955) could produce more worldwide havoc than all the nuclear weapons then in existence.

Except for nuclear weapons and strategy, political scientists (with a few exceptions) have largely bypassed the political aspects of arresting and controlling ecologically disruptive technological innovations. *Environment: A Bibliography of Social Science and Related Literature* (Washington, D.C.: U.S. Environmental Protection Agency, Government Printing Office, 1972), listing nearly 5,000 items published before 1973, confirms this neglect. D'Amato observed that "political theory operates upon the luxurious level that the environment is mere background" (in *Politics of Ecosuicide*, ed. L. L. Roos. [New York: Holt, Rinehart & Winston], 1971). More specifically, political scientists seem generally to take for granted that there will always be a habitable earth and plenty of people to inhabit it – so why bother with these self-evident parameters?

To date, economists have largely prempted this salient borderland between political science and technology. A few political scientists are moving in; more are needed. For those who have not already read widely on the destructive impacts of military operations, weapons research, weather control, and other risks implicit in nonmilitary as well as military disruption of the biosphere, this symposium is an excellent place to begin. Its price is low; it is well written; and the entire booklet can be read in an hour or so.

HAROLD SPROUT

Princeton University

Asia and the Road Ahead: Issues for the Major Powers. By Robert A. Scalapino. (Berkeley: University of California Press, 1975. Pp. 337. $10.95.)

One of the more devastating consequences of the Vietnam War in the United States has been the serious decline of public interest in Asian affairs. Students now prefer an economics course in marginal utility, or perhaps abnormal psychology, to, say, histories of the Meiji Restoration. Members of Congress seem similarly inclined, or more so. Press coverage of the area continues to the extent that censorship permits and reader interest suggests, but articles tend to lack the depth, perception, and enthusiasm of a decade or two ago. Some like to think that improved relations with the People's Republic of China supports policies encouraging a steady decline in economic aid save in selected enclaves, a low profile in diplomacy, and a casual "hands off" approach to any other than the most obvious affronts to United States' interests. Robert A. Scalapino's *Asia and the Road Ahead* opens the issues in contemporary Asia for serious discussion once more. His work will not be met with huzzahs by those who wish that that part of the world and its troublesome problems would drift out of sight.

The book takes a broad approach to the foreign policies, and the internal conditions on which they rest, with a stress on Japan, China, India, Indonesia, the U.S.S.R., and the United States. Although emphasis is placed on six major nations, all of Asia is considered, as are the external interests of most of the rest of the world. In effect, the study is a lengthy essay, addressed in the end to suggestions for adjustments in U.S. foreign policy for the region.

The complexity of the issues and the hazards of prediction are enormous in a book of this kind, especially since changes occur so rapidly. For example, since the text was completed in the late spring of 1975, Chou En-lai and Mao Tse-tung have died; Abdul Razak is no more in Malaysia; and the Liberal Democratic Party is in trouble in Japan following the Lockheed scandals; Indira Gandhi has been defeated and replaced by Morarji Desai; Fernando Marcos has made diplomatic peace for the Philippines in Peking; Mujibar Rahman of Bangladesh has been assassinated, and Indo-Bangladeshi relationships have deteriorated even further. It is to the credit of Robert Scalapino's thesis that changes of such magnitude do not destroy the grounds for his major proposals.

The structure of the analysis is of substantial value. First, the choice of six key states for more intensive coverage, rather than a total examination of the area, gives policy focus that would have been lost in a gross survey. Secondly, each section links history and internal conditions to foreign policy alternatives. Thirdly, more questions are raised than are answered. But, finally, the author does not hesitate to express his opinion on changes to U.S. policy that should be made from among the several options now available. The lengthy bibliographic essay on the related literature (pp. 303–324), and a good index (pp. 325–337), will be valuable for students and for those who wish to scrutinize parallel and contrary approaches to the subject.

Professor Scalapino rejects policies of calculated withdrawal, both from Asia and Europe, as a short-sighted isolationism that runs contrary to the need for international solutions to such problems as nuclear weapons and arms control, to say nothing of trade, Third World economic growth, and political futures for the weaker nations. He also rejects the increasingly popular "enclave policy" that would link more tightly the U.S. to Western Europe, Japan, and perhaps a few other selected countries, and let the rest float as best they can. The interdependence of the world in realms of natural resources, maritime jurisdictions, and population-food problems, to cite a few examples, militates against enclave solutions.

The author proposes instead a policy of "selective internationalism" in Asia, with special concern for East Asia, the Pacific Ocean, and with somewhat lesser attention to Southeast Asia, and even less to South Asia. He also suggests that careful consideration be given to the development over time of a Pacific Community to provide a set of interrelated bridges between the many countries involved.

Scalapino's conclusions, at least in broad terms, probably will appeal to most readers who are academic students of international affairs; disagreements will arise on specifics. This reviewer, for one, can accept "select internationalism," with its emphasis on East Asia and the Pacific as critical elements for U.S. policy in the late 1970s. However, no U.S. policy for Asia can be complete without taking more fully into account South Asia, more particularly India. South Asia, as a whole, contains almost as many people as the PRC. The experience of the past three decades shows that political, social, and economic issues on the subcontinent are so explosive that it is likely that outbreaks of violence will occur there frequently, and with repercussions internationally. Furthermore, Southeast Asia, hopefully under the wing of a strengthened Association of Southeast Asian Nations, has a good chance of providing an effective balance in the region to offset political feuds along ideological and power lines.

It thus appears that "selective internationalism," while being appealing for its apparent caution and implied restraint on the need for U.S. commitments in Asia, ultimately must eschew "selective" in favor of *"effective* internationalism." This means a U.S. policy that recognizes as important all parts of the Asian mosaic of countries, with increments of policy being extended where they are most needed at given times, but considered within a pattern of advanced planning that takes into account the whole arena.

Asia and the Road Ahead is an essay that should be read and criticized by all concerned with United States' foreign policy. It constitutes a most useful brief for a national debate that may assist in creating a new consensus on a rational U.S. foreign policy for Asia.

RICHARD L. PARK

The University of Michigan, Ann Arbor

The Afro-Asian Dimension of Brazilian Foreign Policy, 1956–1972. By Wayne A. Selcher. (Gainesville: University of Florida Press, 1974. Pp. vi, 252. $10.00.)

Departing from traditional studies of diplomacy with the Great Powers, this book deals with Brazilian relations with Africa, the Middle East, and non-Communist Asia (collectively called "Afro-Asia"), utilizing a "regional approach" which treats the geographical regions as undifferentiated while explicating more salient relationships with Portuguese Africa, Japan, and South Africa in detail. Employing a combination of quantitative and expository methods, Professor Selcher seeks to map interregional patterns, to test the viability of Third World cooperation, and to assess Brazil's policies toward the developing regions.

Depending, according to Selcher, on Brazil's sense of identity with the West, the U.S., and the "Luso-Brazilian community" (Portugal, Brazil, and Portuguese Africa), on the one hand, or Latin America and the Third World, on the other – the former identities stressed by the "Cold Warriors" and the "Lusotropicologists," the latter urged by the "culturalists" and the "nationalist-neutralists" – Brazil's policy toward Afro-Asia has seemingly traveled a tortuous and erratic course. In the 1950s, when ties to Portugal, France, and the United States were uppermost, Afro-Asia was neglected. In the heyday of the "independent" policy in the early 1960s, President Quadros proclaimed Brazil's opening to Africa and the Third World and called for the formation of "a single front in the battle against underdevelopment and all forms of oppression," with racially democratic Brazil acting as the leader, "the bridge" between Africa and the West. After the coup of March 31, 1964, the previous rhetoric was discarded, ties to Portugal and the United States were reaffirmed, and the "Luso-Afro-Brazilian community" was hailed. Through it all, Selcher identifies a basic regularity, namely Brazil's continuing effort – first emotional, then reactive, and finally pragmatic – to achieve world power status in the next century.

The major contribution of the book is the careful examination of the objective basis for Afro-Asian-Brazilian relations. Utilizing measures from Rummel's Dimensionality of Nations (DON) project, Selcher studies four dimensions: diplomatic representation, salience, emigration and communications, and exports. Relations with the U.S., Latin America, Western Europe and Eastern Europe are examined as well as those with Africa, Asia and the Middle East. The message that comes across is that Afro-Asia is relatively unimportant for Brazil, the less developed areas of Afro-Asia doubly unimportant. Together, Afro-Asia accounts for less than 10 per cent of Brazil's exports, treaties, communications, and consular representations, and the atypical, more developed states of Japan and South Africa account for large portions of the interactions. If the latter countries, as well as Australia, New Zealand and Israel, are excluded, Afro-Asia purchases only 2.7 per cent of Brazil's exports (p. 125) and accredits about 10 per cent of Brazilian diplomats (p. 105), hardly a firm basis for a Third World policy.

On anticolonial issues, Selcher finds considerable discord. Brazil resists strident anticolonialism because of its extensive trade with South Africa and because of its desire to preserve Portuguese culture in Africa to facilitate future Brazilian trade and influence. Despite the displeasure of the Afro-Asians, Brazil strengthened its ties to Portugal in the mid-'sixties through the "Treaty of Friendship and Consultation" and the "Convention on Equality of Rights and Duties," declaring 1972 the "Year of the Luso-Brazilian Community."

On economic matters, Selcher tells us, Brazil generally supports Third World initiatives, favoring improvement in the terms of trade, stabilization of commodity prices, increased economic assistance, internationalization of the resources of the sea, etc. On a number of concrete issues, however, conflicts arise as a result of Brazil's more advanced development. Brazil seeks to end Africa's duty-free access to the EEC and opposes special preferences for the so-called "Fourth World," while favoring preferences for the LDC's generally, especially for manufactured or semimanufactured products. On coffee issues, where Brazil is the dominant producer, it resists expanding production of the cheaper African coffee and risked renewal of the Coffee Agreement disputing with the United States over soluble coffee. On cocoa, where Africa is dominant, Brazil seeks to expand its share of the market and to undermine African preferences.

Since interests often conflict, Selcher discovers little basis for strong ties between Brazil and Afro-Asia. He finds the foundation for a Third World movement questionable (p. 233), the basis for the African policy of the early 1960s unrealistic, founded, he believes, on meager knowledge of Africa and excessively paternalistic views of Brazil's capacity to lead Africa. Selcher believes that since 1967 a more realistic, pragmatic policy has been pursued with more modest, long-range goals: to diversify exports by winning new markets in Afro-Asia, to promote unity among the LDC's to achieve common objectives, to ply a middle ground between Portugal and Black Africa which preserves Portuguese culture, enhancing Brazil's future influence in Africa and Asia.

Although documentation is sometimes scant, the account a bit too matter of fact, and the themes and wording occasionally obscure, Selcher's book is a useful contribution to our understanding of Brazilian foreign policy and relationships among Third World countries. Subsequent events – the oil crisis and the decolonization in Portuguese Africa – have made these relationships even more important for the future.

K. LARRY STORRS

Congressional Research Service,
Library of Congress

The Arab-Israeli Military Balance Since October 1973. By Dale R. Tahtinen. Foreword by Robert P. Griffin. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1974. Pp. 43. $2.00, paper.)

This pamphlet is an admirable introduction to the realities of the Arab-Israeli military balance for the reader unacquainted with the intricacies of military hardware. Coherently divided into sections on air defense, mobile ground forces, naval forces, nuclear and CBR scenarios, American involvement, and superpower war possibilities, the work provides a succinct description and analysis of weapons systems and inventories in the Middle East at the outbreak of the October War.

Tahtinen argues that the "major lesson" to be learned from Arab-Israeli wars is that every new outbreak of hostility occurs "at a higher and more dangerous level of warfare, bringing the superpowers ever nearer to confrontation" (p. 1). He supports this claim with respect to the October War by an impressive examination of the escalation in armaments that accompanied the war and its aftermath. Tahtinen further extends the argument by picturing the higher military and civilian costs that escalation into the realm of chemical, biological, or nuclear warfare may bring. This prospect adds urgency to the quest for a final peace settlement in the area.

The Arab-Israeli Military Balance has one major flaw: a subtle but distinct anti-Israel flavor that occasionally manifests itself in faulty analysis. For example, the author justifies his argument against the further supply of highly accurate "smart" bombs to Israel by citing the alleged use of antipersonnel bombs against civilian targets in Damascus during the October War (p. 16). Though smart bombs could be used against civilians in a future war, it is economically and militarily obvious that their primary function is to neutralize military targets, especially modern anti-aircraft systems. Israel needs such weapons to enable her air force to compensate for the overwhelming Arab advantage in personnel and equipment. To date, American supplies of these weapons have helped maintain an approximate military balance in the region. Without such balance, progress toward peace is impossible.

Aside from this major flaw, the book suffers from two minor problems. First, because of its brevity, many of its arguments are suggestive, rather than exhaustive. For example, both the issues of potential superpower involvement and nuclear scenarios require more thorough treatment. Second, since this book was prepared in early 1974, it is already somewhat dated. Nonetheless, *The Arab-Israeli Military Balance* provides a helpful background for understanding the events of today.

MARVIN C. FEUERWERGER

Harvard University

French Communism: 1920–1972. By Ronald Tiersky. (New York: Columbia University Press, 1974. Pp. 425. $15.00.)

One cannot read this scholarly book without allowing oneself moments of respite, because almost each page deserves close attention in view of so many data and quotations. It is a reference book for the history of the French

Communist Party from its beginning in 1920 (after the split within the Socialist Party at the Congress of Tours) through the year 1972. The book retains its validity also in 1977, because the French political situation has not changed during the last five years.

The French Communist Party until very recently remained faithful to Moscow, but it seems now to follow the example of the Italian Communist Party. It publicly castigated in 1975 the harsh regime in the Soviet forced-labor camps and no less harsh treatment of Soviet dissidents. Earlier it allowed itself in 1968 to criticize only mildly the Soviet invasion of Czechoslovakia. It wants to demonstrate now its independence of Moscow and thus enhance the trust of the French Socialist Party whose leader François Mitterand pursues the policy of cooperation with the F.C.P. in the hopes that the Left coalition would win one day victory in the general elections and form together the government of France.

It is interesting to observe the split in attitudes toward the Communist parties among the West and South-European Socialist parties. On the one hand, almost all Western European Socialist parties, headed by the West-German and the British Labor Party, categorically reject any cooperation with the Communist parties which are, it is true, weak in Western Europe except for France. On the other hand, the French, Spanish and Italian Socialist parties face strong Communist parties and, reluctantly or not, are forced to cooperate nationally or locally with the respective Communist parties in spite of all difficulties and mutual competition.

The author was somewhat ahead of the current developments in claiming that the French Communist Party pursued a line independent of Moscow already in the 'sixties, because it vigorously opposed President de Gaulle, while the USSR cultivated good relations with him. He says that the French Communist support of Mitterand's rival candidacy in the presidential elections in 1965, "despite the wishes of the Soviet leadership," "was a step away from unconditional support of Soviet foreign policy" (pp. 243–244). If he had looked at that problem from the Soviet point of view, he would have discovered no contradiction. De Gaulle was considered in Moscow a good partner in international politics because of his independence of the United States. Mitterand, supported by the French Communists, would have been an even better partner. In both cases the USSR would not be a loser and could envisage the prospective elections with equanimity. While it is true now that the French Communist Party takes care, first of all, of its own domestic interests and to that extent is independent of Moscow, there is an analogy with what was true in the 'sixties. The USSR entertains excellent relations with President Giscard d'Estaing, while the F.C.P. opposes him fiercely.

According to Tiersky, the "respectability" of the F.C.P. was won during the Nazi occupation because of the F.C.P.'s important role in the resistance movement. Its present "respectability" is due largely to Mitterand who publicly proclaims the Communists as loyal partners in spite of recurring quarrels between the two parties. Assuming for a moment that the Socialists and the Communists would win one day a decisive electoral victory (not an impossible prospect), will the Communists remain loyal partners, and would the two parties agree on the attitudes to take regarding the European Economic Community and the North-Atlantic Alliance? It is interesting to note in this respect the results of a July, 1972, public-opinion poll (quoted on p. 307). Answering whether they believed that the Communists would remain loyal partners in a socialist-communist coalition government, 50 per cent of the respondents denied it, only 23 per cent gave an affirmative answer, while 27 per cent were uncertain.

The French Communist Party began only in 1975–76 to assert its independence from Moscow, among others, by its criticism of the harshest aspects of Soviet regime, and by its claim that French socialism would be different from the Soviet and would respect individual freedoms and the existence of parties other than the Communist if they were willing to cooperate in building a socialist society. It rejected the Soviet dogma of dictatorship of the proletariat, i.e., one-party system. Its new stand was proclaimed, for instance, by its representative at the 25th Congress of Soviet Communist Party (February–March 1976).

The author renders his due to the administrative capacity of the Communist municipalities within the so-called Red Belt in the vicinity of Paris and at Le Havre or Nimes. The same could be said about the efficiency of Italian regional and municipal administrations. This good record of the Italian Party contributed, among other things, to the recent successes of the Italian Party in the regional and local elections, which gave it one-third of the Italian electorate.

One must say that during the crisis in May–June 1968, when the students and workers joined in the revolt against de Gaulle, the F.C.P. proved its cautiousness in evaluating its chances of seizing power. It refrained from imitating Lenin and did not try to channel the discontent into a revolution. This explains also their present cooperation with the Socialist Party instead of going it alone.

One should note a small error: the Nazi-Soviet agreement was signed on the 23rd not the 22nd of August, 1939.

This book is of interest not only for the specialist in Western-European politics but also for the general public, since France is an important part of the Western community and since the French Communist Party will remain a significant factor in French politics.

W. W. KULSKI

Durham, North Carolina

China and the Great Powers: Relations with the United States, the Soviet Union, and Japan. Edited by Francis O. Wilcox. (New York: Praeger Publishers, 1974. Pp. 103. $10.00.)

The result of the 1973 Christian A. Herter Lecture Series sponsored by The Johns Hopkins School of Advanced International Studies, this concise volume edited by the School's Dean Emeritus, Francis O. Wilcox, is a timely and valuable primer on China's rapidly evolving Great Power relations. In his introductory comments it seems particularly appropriate for Wilcox, who as Assistant Secretary of State for International Organization Affairs (1955–60) formulated strategies to maintain the PRC's exclusion fron the UN and other international bodies, to acknowledge both the need for President Nixon's policy reversal toward Peking and the PRC's commendable degree of moderation and restraint in the UN.

The second chapter by John Stoessinger analyzes the Sino-Soviet relationship, the origins of the split, subsequent bilateral relations, and the impact of each power on the rest of Asia, the United States, and the West. Though covering some familiar ground regarding the origins of the split, Stoessinger argues that so long as the PRC maintains territorial claims against the USSR, future relations will probably be marked by continued low-key, low-risk cold war competition punctuated by limited physical border skirmishes, regardless of the advent of new leadership. One shortcoming perhaps is the writer's failure to consider the possibility of a renewed Cultural Revolution which might tempt Soviet hard-liners to launch a pre-emptive nuclear strike against China at a time when U.S. deterrent incentives could be eroded by domestic political factors. If one overlooks the nearly verbatim use of three paragraphs from his textbook, *The Might of Nations* (4th ed., New York: Random House, 1973, pp. 60–61), as an introduction, the author does very well to compress such a sweeping topic into sixteen pages.

Writing on Sino-Japanese relations, Edwin O. Reischauer notes that because the two nations have never been major world powers simultaneously, historical parallels are no guide to the present situation. While offering a convincing review of the reasons motivating the recent Peking-Tokyo rapprochement, the author's major contribution is a thoughtful analysis of the influence of economic, military, and political considerations on future bilateral relations. Reischauer concludes that neither power would gain greatly by moving toward partnership or alliance, but dangerous rivalry is equally unlikely except as a by-product of greatly improved Sino-Soviet relations or a serious deterioration in Japanese-American ties. The subsequent Japanese energy and economic crises, however, could belie the author's predictions should Japan be forced to choose between the PRC or the USSR for energy imports.

In his essay former Senate Majority Leader Mike Mansfield, an ex-academician with a long interest in Asian affairs, displays an unmistakable zeal for improved Sino-American relations and suggests a number of specific initiatives for the 93rd Congress to "wipe the slate clean of the anti-Chinese legislation of the past two decades" (p. 55). In clear contrast to the more analytical approaches of the other authors, Mansfield unequivocally calls for rescinding the Formosa Resolution, the termination of the U.S. military aid mission and large-scale military grant aid and credit sales to Taiwan, a deliberate phase-out of U.S. forces in Korea and Thailand, a reduced military presence in Japan, a re-examination of SEATO and the defense treaty with Taiwan, and expanded U.S.-PRC cultural and economic relations including Most Favored Nation status for Peking. While most of these suggestions are commendable, the author's advocacy role does not permit an examination of the ramifications for Chinese security vis-à-vis the Soviet Union of a severely reduced U.S. military presence in Asia, nor the domestic political difficulties implicit in such a drastic change of policy toward Taiwan.

The last and perhaps the most thoughtful chapter in the book by Jerome Allan Cohen examines the political and legal complexities of the "normalization" of Sino-American relations (defined as the establishment of formal diplomatic relations at the ambassadorial level). Cohen suggests that the process has been impeded by three obstacles: (1) continued U.S. recognition of Taipei; (2) the 1954 Mutual Defense Treaty; and (3) the legal status of

Taiwan. After a careful review of each, the author, as later events seem to show, correctly predicts that Peking's patience with the liaison office arrangement would wear thin. He urges the U.S. to follow the Japanese example in normalizing relations in the not-too-distant future and warns that by delaying Washington is gambling on continued moderate leadership in Peking. Finally, the writer advances several highly imaginative formulas to accord equal legitimate status to the PRC while guarding against forcible integration of Taiwan with the mainland. Writing before recent changes in political leadership in the U.S. and the PRC, Cohen would probably agree that the chances for more rapid normalization of relations are much improved.

Although the broad array of perceptive and at times provocative insights comprises the book's strength, an unfortunate brevity and the absence of footnotes – understandable given the confines imposed by the original lecture format – are only partially offset by a helpful bibliography and appendices. Nevertheless, this book has undoubtedly provided a firm foundation for more extensive future scholarship in this field and should be widely read.

WILLIAM R. FEENEY

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Who Killed the British Empire?: An Inquest. By George Woodcock. (New York: Quadrangle/The New York Times Book Company, 1974. Pp. 356. $10.00.)

This book is not a novel of suspense, and George Woodcock quickly and predictably renders the verdict of his inquest on the death of the British Empire: the killer was not a single identifiable executioner but rather a web of circumstances or conspiracy made up of human beings and social and economic forces. No empire, Woodcock argues, has ever been destroyed solely by the will or the action of its subjects. When an imperial power expires, a combination of four factors nearly always contributes to its demise: (1) the will of the oppressed peoples, often provoked by the desire to imitate and then to supplant the alien rulers; (2) the threat of external enemies, especially of rival empires; (3) the decline of the will and ability of the imperial people to rule, including the loss of the image of themselves as a master race; and (4) the power of economic circumstances, which can compel the imperialist to shed an intolerable burden. Of these, he considers the loss of self-confidence by the rulers as the crucial factor.

Having settled the general identities of the Empire's murderers, Dr. Woodcock turns to a discussion of the circumstances leading to its death, an event of considerable importance in world history since it meant the elimination of "an imperial complex that less than thirty years ago embraced a quarter of the world's land surface and ruled a quarter of the world's population" (p. 7). In a relatively brief (considering the time span and the expanse of history he deals with) but wide-ranging account, he surveys the Empire at the point of its greatest territorial expansion and traces developments from 1783 to the point of crisis in 1930. Weaving together the distant and the immediate past, he provides illuminating thumb-nail sketches of the acquisition and imperial value of nearly every territory once colored red (to indicate the British Empire) on the world map, from Tristan da Cunha and Ascension Island to Uganda and Mauritius; but he concentrates mainly on Canada and India, arguing that the whole history of the decline and death of the Empire could be written in terms of the constitutional history of those two areas.

Woodcock sees 1930 as the year that historians will probably find most potent in its effects of all the Empire's critical years. The Empire seemed then at its height, but three events signalled its coming demise: the recommendations of the Imperial Conference that were enacted a year later as the Statute of Westminster; Gandhi's great salt march in India, the campaign of noncooperation, and the release of Gandhi from prison to confer as an equal with the Viceroy; and, under pressure from Japan and the United States, the surrender of Weihaiwei to China, Britain's first abandonment of territory to a nationalist government since the American Revolution. Woodcock says:

> In all three incidents Britain was responding as she would not have done before 1918 to external pressures: to the wishes of the dominions, to the rebelliousness of the Indians, to the nationalist ambitions of the Chinese, to the rival imperial intents of the Japanese and the Americans. The significant point is that in each case the response, if not exactly a surrender, was a stepping back from power. 1930 was the year in which the weakening of the British will first became clearly perceptible (p. 252).

The remainder of the book concerns the rapid dismantling of the Empire after World War II, with preponderant attention to the loss of India and the failure of British efforts to maintain a sphere of influence in the Middle East.

Throughout his study of history, Woodcock keeps an eye on the Empire's executioners. He concludes: "The verdict should probably read that the British Empire died partly of natural causes, but that its end was precipitated by the acts of many identifiable individuals, though . . . no single individual assassin can be identified" (p. 327). The natural causes include the contradiction between imperial policies and British political values ("One cannot rule indefinitely according to Curzon and teach according to Milton and Shelley"); the revolution in military techniques; and the relative decline of Britain as an industrial and financial power. There were also states and individuals who helped make certain its end. First among these villains was the United States, which directly and indirectly had been bringing pressure against the Empire for more than a century; and after the United States, Japan, whose triumphs in the Far East irretrievably changed the image of Britain in the minds of colonial peoples. Then there were the individuals: Gandhi; a raft of Canadian leaders, from Papineau to Mackenzie King, who, by relentlessly extending the bounds of independence, provided examples for nationalists elsewhere; George Padmore; Kwame Nkrumah; Gamel Abdel Nasser; and a great many British liberal and radical thinkers and propagandists. The end came comparatively peacefully – "in the final resort, it was weariness as much as conviction that made Britain decide to abandon the burden of empire" (p. 332).

Woodcock, who taught English literature and now edits the periodical *Canadian Literature,* is little concerned with theory, though he indicates his belief that the reasons for imperialism were always more complex than the "merely economic factors" used by Lenin. A principal problem of the book is that the author seeks to present so much of the Empire's life and history that he must skimp in his discussion of its killers; and his examination of them is inadequate. The role of the British anti-imperialists and their effect on the British political culture and governmental policies needs fuller treatment. He does have general comments on them scattered through his volume; but there is not a word on such important theorists and publicists as J. A. Hobson and H. N. Brailsford, and little about the role of "Little Englanders," "Pro-Boers," and Labour and Socialist spokesmen. Also needed is a more complete discussion of the United States as a principal villain in the Empire's downfall.

The specialist whose interest is the theory or practice of imperialism is likely to find some useful information in *Who Killed the British Empire?* but little depth of analysis. The book will prove most valuable to those who want a broad, interesting, and well-written survey of the Empire's development and decline, with some modest generalizations along the way.

KENNETH E. MILLER

Rutgers University

A Dangerous Place: The United Nations as a Weapon in International Politics. By Abraham Yeselson and Anthony Gaglione. (New York: The Viking Press, 1975. Pp. 240. $8.95.)

The founders of the League of Nations and of the United Nations were exceedingly ambitious; their object was no less than control over international politics. The UN Security Council in particular was fashioned as a center of power and authority; its purpose – through regulation of armaments, pacific settlement of disputes, and coercive sanctions – was to resolve, worldwide, the dilemma of order vs. change.

Success in this endeavor, the present authors assert, requires such conduct by the United Nations as to enable member states to acknowledge it as "the supreme spokesman for that international justice and morality to which they pledged themselves when they joined the organization" (p. 2). Yet one is at a loss to find in the Charter a hierarchy of values by which justice and morality can be confidently deduced. On the one hand the Charter invokes "law" – whose thrust favors the status quo; while on the other it invokes "justice" – which in practice is the demand for change. Implicit herein is tension between order and change, which poses a dilemma incapable of resolution in terms of first principle. The Charter's ambiguity in this regard is deliberate. By design, it rejects the precedent of a positive guarantee of territorial integrity and political independence contained in Article X of the League Covenant as too rigidly supportive of the status quo. Also by choice, the Charter omits any definition of aggression. Thus it invites the play of politics, seeking discipline through ad hoc judgments as to what goes and what doesn't. For example, acquiescence by the United Nations in the demise of Hyderabad and Goa under military pressure from India – while admittedly unprincipled, as Professor Yeselson and Gaglione charge – can be regarded as appropriately discretionary in the face of anachronistic situations.

Yet, the fact that the United Nations is above all a political institution does not necessarily dispute the authors' contention that the organization exacerbates rather than reconciles international conflict and is, therefore, a dangerous place. Political wisdom is controversial at best. Its attainment by a Security Council that has failed to emerge as a dominant center or by a General Assembly bound by the dogma of one state/one vote is not a foregone conclusion. Doubt naturally ensues, which the present volume resolves by wholly discrediting the United Nations. Indeed, believing that the United Nations cannot be made to work for peace, the authors would prefer to see the abolition of its political functions (p. 212): not because the organization makes no difference, but, on the contrary, because, as a weapon in "the armory of nations in conflict," resorted to with the object of subduing one's opponent, it makes matters worse. The authors depict the United Nations as an arena in which antagonists demean each other through propaganda and vituperation, struggle for advantage over questions of status, or seek to prevail through dubious legitimization of national policies.

Had international politics submitted to an authoritative and powerful United Nations, the postwar world would be very different from the one we know. Hindsight, however, confirms how improbable such an outcome is. In actual practice, the United Nations has emerged as a variable whose influence in particular international situations may be for better or worse. That it is always for the worse one cannot judge as confidently – certainly not as passionately – as does the present volume. Nor can one hope for salvation in an overarching justice and morality that the authors would have the United Nations use as point of departure. Unfortunately such appeals are themselves weapons in the armory of nations in conflict. Politics is a morass from which there is no obvious exit.

Accepting fatalistically the continuance of the United Nations as a factor in world politics, the authors urge prudence and restraint on its members. With this one can heartily agree, while admonishing the organization, for its part, to cultivate the art of mediation with more conscious effort than it has heretofore. In the main, however, the book – conforming to the current disenchantment with the United Nations – is a polemic that reflects the corrosive effect of the Arab-Israeli conflict. It is therefore dated, for, responding like the weather to constant flux, attitudes toward the United Nations shift through time.

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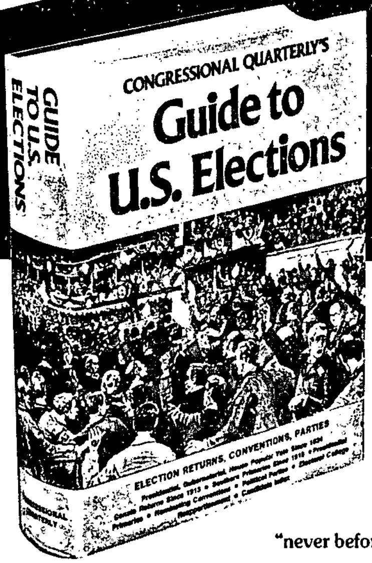
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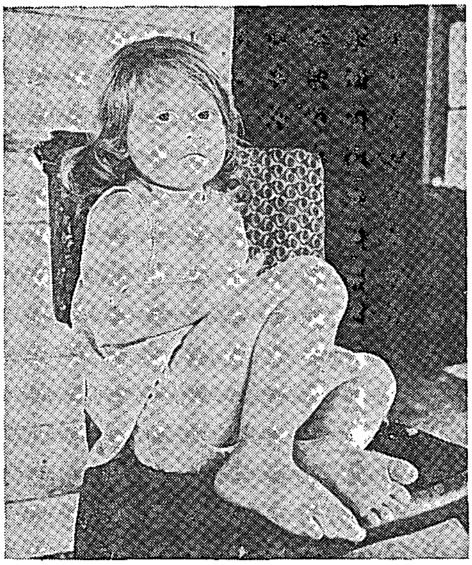

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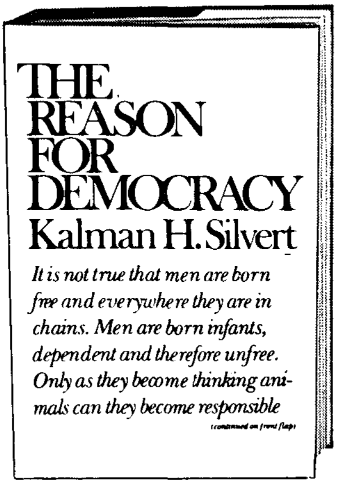

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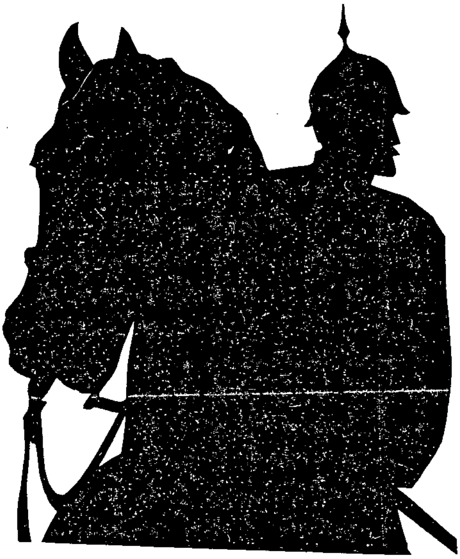

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